Instruments — Observations — Theories

Studies in the History of Astronomy
in Honor of James Evans

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Studies in the History of Astronomy in Honor of James Evans

edited by
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and
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For Jim Evans

Teacher, Editor, Colleague, Coauthor

Contents

Preface

Christián Carman

A few years ago I was in James Evans's pleasant home in Seattle working on a common project. We were not sure about some values regarding the Babylonian relations defining the synodic periods of the planets. Standing up, he said: "I will go to look at a very reliable book and I'll be back in a minute". A minute later, he came back with a big blue volume. To judge by his appearance, it was a book that he used very often: it was old, worn and had plenty of marks in pencil. He opened the book at the last pages, looked at the index, searched for the page indicated in the index, found the searched values, and copied them on a sheet of paper. The big blue volume was a copy of his own *The History and Practice of Ancient Astronomy* (Oxford University Press, 1998)! Of course, he overacted a bit to produce a hilarious situation, but after this first time, I saw him many times consulting his own book. The reason is clear: it is a really trustworthy book, and he knows that. Working with him over several years I realized why: he is absolutely careful with every detail he introduces in his works. He checks and double checks every datum and calculation. I must say that he is as careful as you can be without falling on the side of obsession.

And of course, he is not the only one who uses *The History and Practice*. Even if it is intended as a pedagogic introduction to its subject, it is referenced in numerous academic papers dedicated to ancient astronomy. It is a very accurate and comprehensive book, but probably his greatest virtue is its clarity and its "self-containedness". You can start reading it almost no previous knowledge about astronomy and its early history, and if you are patient, methodical, and carry out the exercises the book proposes (in the manner of a typical university textbook on modern science, for the book is the mature fruit of more than two decades of teaching an introductory course on the history of astronomy at the University of Puget Sound), at the end you will be ready to face any more technical study on virtually any topic. The volume combines classical explanations with Jim's own contributions in a natural way. Reading the book, one can easily be unaware, for example, that the lucid explanation of Ptolemy's introduction of the equant point is his own contribution. Even more: some of his personal contributions are not published independently in papers, for example his proposal for explaining the value of the lunar elongation at a dichotomy introduced by Aristarchus of Samos in his *Treatise of the Sizes and Distances of the Sun and Moon*.

This double status of the book (being both an introduction for non-specialists and a source for specialists) reflects one of the most unique and valuable characteristics of Jim as an academic scholar. There are excellent scholars, very acute and profound, but who write in such a way that only specialists can follow them (and often with a big effort); there are also very good science communicators who can write pedagogical introductions, but do not produce their own research. Jim integrates the best of both worlds: he is a very acute researcher but with a serious preoccupation for being clear and didactical. His research papers are so approachable that they could be used for communicating science to the layman, and his didactic material is so serious that can be cited by scholars. The pedagogical excellence of his writings is not just the effect of a natural talent, but the result of a deliberate effort to convey the content as clearly as possible.

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I was witness in many of our coauthored works of how many times Jim goes back and forth on just one sentence just to make it clearer.

And his concern and pedagogical skill is not limited to written texts. It is really wonderful to attend his introductory courses. He manages to awaken in the students a genuine interest on the topics he explains. The most exciting class of his Ancient Astronomy course I ever attended was, again, the one in which Jim described the approach Ptolemy could have had for introducing the equant point. He first described the problem: the retrograde arcs of Mars have different lengths, and the distance between one retrogradation and the next is also variable. Then, he described the tool that Ptolemy had: the eccentric point that Ptolemy had found be very successful in the solar model. Later, Jim showed that if you move the eccentric point on one direction, you can reproduce fairly well the different length of the arcs, but the location of the retrograde loops is a disaster. On the other hand, if you move the eccentric point in the opposite direction, the prediction of the location of the loops is accurate enough but the description of the length of the arcs is really bad. At this point you can perceive in the student some kind of delusion and desperation, because it seems that there is no possible solution. He really strives to introduce the students into this atmosphere. Then he explains how Ptolemy decided to divide the functions of the eccentric into two points, making one point the center of the deferent and the other the center of uniform motion. And how, moving each center on contrary directions, the length of the arcs as well as their location is perfectly explained. The atmosphere now changed to a sense of relief and admiration. He falls silent for a few seconds and then asks for applause for Ptolemy. Or rather, he gives permission to the students to applaud, because the applause was already waiting in the hands of the enthusiastic students who had accompanied Ptolemy on his adventure to solve the problem of retrograde motion.

I think that the secret of Jim's success as a teacher (he cofounded the University of Puget Sound's program in Science, Technology, and Society, and was named Washington Professor of the Year in 2008) is not only that he really loves what he teaches. This goes without saying. His passion for astronomy can be proved with just one example: in the ceiling of his living room has drawn the exact place that the Sun illuminates at 12:00 noon on the summer solstice. The last time he painted the ceiling, he left this mark untouched. But he also loves and genuinely cares for the students whom he teaches. Again, one example: as soon as he receives each semester the list of the students, he studies their names and photos so that in the very first class he can name each student at sight! So, we can say that he has two friends: the history of astronomy and each student. Usually, you want that two very dear friends who do not know each other should meet, and Jim's introductory courses can be understood as he making two friends—history of astronomy and students—meet each other.

As a researcher I always have been impressed by his intuition for knowing where the answer lies. Jim has a deep respect for the empirical data, but also the great courage to let himself be guided by intuition even when the data seem to point in the opposite direction. In the years I worked with him, the evidence, once looked at in the right way, always ended up confirming what his intuition had pointed to. His mood is placid when things do not work out, but an unconcealed, almost childlike enthusiasm when interesting proposals emerge or the data finally fall into place.

As a research colleague and likewise as an editor Jim is extraordinarily generous, as contributors to the *Journal for the History of Astronomy* of which he is editor know very well. He does not understand scholarly research as a competition but as a collaborative job. He is always ready to teach and correct when there is need. He, along with his lovely wife, Sharon, didn't have any problem in spending one dinner helping me to distinguish the pronunciation of the words "good" and "wood"! But he is also very delicate correcting people, and also unusually kind and modest. I remember when a neighbor of Jim asked me why I was visiting him—he simply couldn't believe that I came all the way from Argentina just to study with him. He lived across the street, but he didn't know that James Evans is one of the world's leading authorities on the history of astronomy.

This collection of papers offered in Jim's honor by his friends and colleagues is a reflection of that side of his expertise, but it deserves to be mentioned that there are other sides too. He graduated from Purdue University, Indiana in 1970 with a B.S. in electrical engineering; and he earned a Ph.D. in physics from the University of Washington in 1983. Is there anyone else today who could have published, in the same year (2006), both a translation of an ancient Greek handbook of astronomy, Geminos's *Introduction to the Phenomena* (with J. L. Berggren), and an interdisciplinary volume on quantum mechanics (coedited with Alan Thorndike)?

Memorial note

Sadly, two contributors to this *Festschrift*, died before its publication.

Alan Stewart Thorndike (1945–2018) was James Evans's colleague on the faculty of the University of Puget Sound, and coeditor with him of *Quantum Mechanics at the Crossroads: New Perspectives from History, Philosophy and Physics* (Springer, 2006). Later, he worked with James and with Christián Carman on the Antikythera Mechanism, a project for which his expert craftsmanship in metal and wood provided an invaluable practical control of his collaborators' speculative reconstructions.

John Hugh Seiradakis (1948–2020), Professor Emeritus in the Aristotle University of Thessaloniki, was a founding member of the Antikythera Mechanism Research Project. He will be remembered by the many fellow scholars and students engaged in study of the Mechanism over the last decade and a half for his generous and gentlemanly encouragement, instruction, and collaboration.

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Triangular gear teeth

Alan Thorndike

Introduction

The first craftsmen to fashion metal gears did not have the advantage of specially shaped cutters. It may be that they used some ancestral version of a present day file to cut away the metal between the teeth and that they endeavored to leave square or triangular shaped teeth. This note addresses some properties of gears with triangular teeth. The serious reader will find most of what follows, and a great deal more, in the first few pages of Buckingham 1998.

Uniform rotation

When two cylinders roll against each other, without slipping, about fixed centers, their rates of rotation are inversely proportional to their radii. A cylinder of diameter *2D*, in contact with one of diameter *D*, will rotate half as fast as the other. For gears, the result is not quite true. After all, what would be meant by the diameter of a gear? As two gears turn, the point of contact moves up and down along the sides of the gear teeth so that the concept of a diameter is blurred. As the point of contact moves towards maximum radius at the tip of a tooth on one gear, it approaches the minimum radius on the other gear. For equal gears the ratio of the two diameters varies from r_{max}/r_{min} to r_{min}/r_{max} with the passage of each tooth. For a typical gear of 10 teeth, the height of the tooth is about a quarter of the outside radius. Therefore the ratio of the rates of rotation for two 10 tooth gears varies from 9/7 to 7/9. The variation occurs within the passage of each tooth. A gear with *N* teeth will rotate at a steady rate over rotations of any multiple of $2\pi/N$.

The effect of the varying radius can be compensated for by choosing appropriate shapes for the gear teeth. In fact, for teeth with profiles based on the involute curve the compensation is exact. Let us refer to the state of motion where both gears have constant angular velocity simply as the state of *uniform rotation*. We will meet below a necessary condition for uniform rotation. We will find that gears with triangular teeth do not satisfy this condition.

The fundamental theorem

When two gears rotate about fixed centers, they interact almost always at a single point of contact. (The "almost" is necessary because there may be isolated instants when the gears have two points of contact, more about which below). The profiles of the two interacting gear teeth will be curves with a common tangent at the point of contact. Ignoring frictional effects, the forces exerted by each gear on the other lie along the line perpendicular to the common tangent at the point of contact. Because they are in contact, the displacement *s* of the two gears along this normal are equal. In Figure 1 the rotations, i. e. the angular displacements, are dS/AB and dS/DE. The ratio of the two rotations is:

(1) $$(^{dS}/_{AB}) : (^{dS}/_{DE}) = -DE : AB = -AC : CD$$

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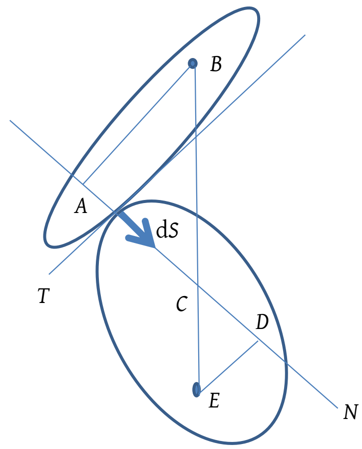

Figure 1. The two shapes *AB* and *CDE* rotate about the fixed centers *B* and *E*. *T* is the common tangent at the point of contact. *N* is perpendicular to *T*. The displacement d*S* is common to both shapes. But the rotations are d*S*/*AB* and d*S*/*ED*. The line of centers *BE* is cut by *N* to form *BC* and *CE* which have the same ratio as *AB* to *DE* . If the shapes are such that the point *C* remains fixed, then the two bodies will rotate uniformly, like two cylinders with radii *BC* and *CE*. *C* is called the pitch point.

The last equality follows from the similarity of triangles *ABC* and *CDE*, and the negative sign reminds us that the gears rotate in opposite senses. For uniform rotation, the ratio of the two rates of rotation remains constant even as the point of contact moves up and down along the gear teeth. For uniform motion, the point *C* on the line of centers is fixed. Two gears are said to have *conjugate gear tooth profiles* if they rotate uniformly. For a given tooth profile, it may be possible to construct one that is conjugate to it. The conjugate profile may be very similar to or very different from the given profile.

Conjugate teeth

Consider the situation where the gears rotate uniformly like cylinders with radii R_1 and R_2. Take the origin of rectangular coordinates (x, y) at the pitch point on the line of centers and take the y-axis along that line. The given data for a tooth profile has the form of a table with specified y values ranging from somewhat below zero to somewhat above. For each y value there is given an

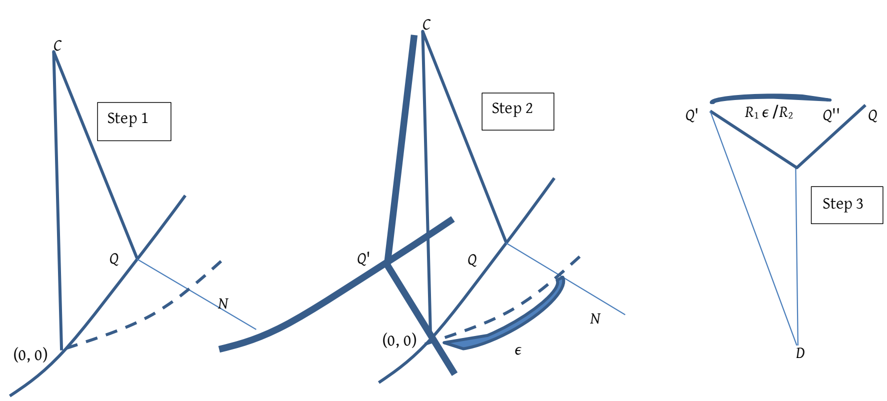

Figure 2. Steps in the construction of the point Q'' (on the gear *D*) which is conjugate to *Q* (on gear *C*).

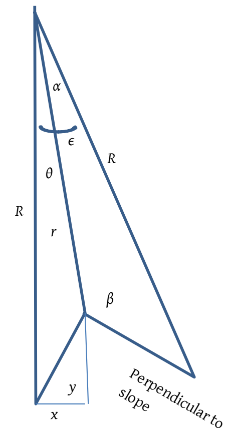

Figure 3. Angles and sides used to determine ϵ.

x value. When referring to a tooth profile, or comparing two profiles, we always mean the set of x, y values when the profile includes the origin $x = 0$, $y = 0$. We will say such a profile is in *standard position*.

We will also use polar coordinates r, θ, centered at the fixed center of the R_1 gear. For each θ there is an r. Adding a constant angle to all values of θ has no effect on the tooth shapes. The construction of the conjugate profile proceeds one point at a time.

Step 1. With the given profile in standard position, choose a point $Q = (x, y)$ on the profile. Define the radius vector (r, θ) from $C = (x = 0, y = R_1)$ to Q. Draw the line N perpendicular to the profile at Q.

Step 2. Rotate the line CQN clockwise about C until it passes through the origin. Record the angle of rotation ϵ. This moves Q to a point Q' on the line of contact.

Step 3. Locate the center of the conjugate gear at $D = (0, -R_2)$. Rotate the vector DQ' clockwise about D through an angle $R_1\,\epsilon/R_2$. Q now occupies a point Q'' on the conjugate gear . Q'' is not the same as Q. Even if the radii of the two gears are the same, $R_1 = R_2$, the distances CQ and DQ' are not equal. Only the pitch point $(x = 0, y = 0)$ maps onto itself.

Repeat steps 1-3.

The angle ϵ is easy to determine graphically. Some consideration of Figure 3 also produces ϵ:

(2) $\theta = \operatorname{atan}\left(x / (R - y)\right)$

(3) $r = \sqrt{[x^2 + (R - y)^2]}$

(4) $\zeta = \pi/2 + \operatorname{atan}(dy/dx)$

(5) $\beta = \pi/2 + \theta + \zeta$

(6) $\sin\lambda = (r \sin\beta)/R$

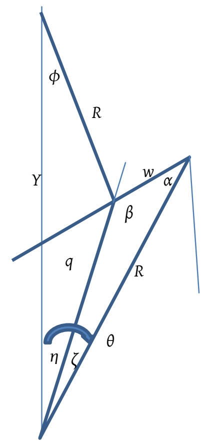

Figure 4. Quantities used to determine the rotation of two gears with triangular teeth. Y is the line joining the centers of the gears. R is the radius vector to the tip of a tooth on the upper gear.

(7) $\alpha = \pi - \beta - \lambda, \varepsilon = \theta + \alpha$

This procedure works for any given profile $r(\theta)$. We will consider next the special case of triangular teeth.

The interaction between triangular gear teeth

Figure 4 shows the contact between two gears with triangular teeth. The tip of a tooth on the upper gear bears against the face of a tooth on the lower gear. The line labeled R is the maximum radius of the upper gear, reaching from the center of the gear at the top of the figure to the tip of a tooth near the center of the figure. As that gear rotates counter clockwise, the angle ϕ increases, the tip of the tooth marked by R slides along the face of the lower gear, marked by w, causing the angle θ to increase as the lower gear rotates clockwise. We first calculate $\theta(\phi)$ and from this obtain $d\theta/d\phi$.

Application of the law of cosines and law of sines to the two triangles having sides YRq and Rwq produces:

(8) $q^2 = R^2 + Y^2 - 2RY \cos \varphi$

(9) $\sin \eta = {}^{R \sin \varphi}/_{q}$

(10) $\beta = \pi - \operatorname{asin}\left({}^{R \sin \alpha}/_{q}\right)$

(11) $\zeta = \pi - \alpha - \beta$

(12) $w = {}^{q \sin \zeta}/_{\sin \alpha}$

(13) $\theta = \eta + \zeta$

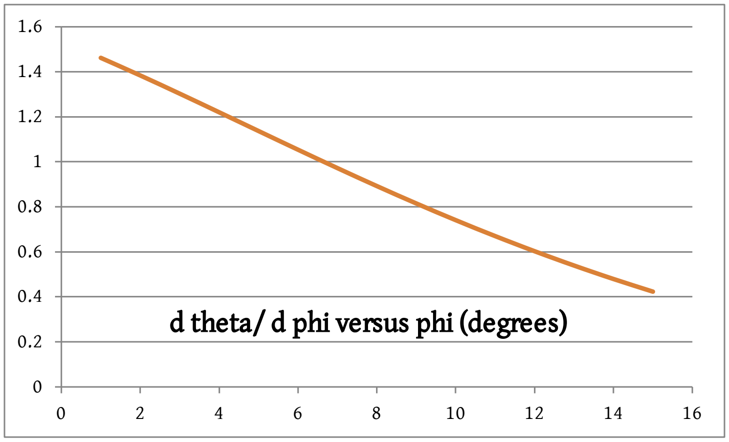

Figure 5. Showing the variation in $d\theta/d\phi$ as a single tip slides over a single face.

The awkward expression for β arises from the inverse sine function having its output between $-\pi/2$ and $\pi/2$. We regard Y, R, and α as fixed parameters, and eliminate β, η, and ζ to get:

(14) $\theta(\varphi) = \operatorname{asin}\left({}^{R \sin \varphi}/{}_{q}\right) + \operatorname{asin}\left({}^{R \sin \alpha}/{}_{q}\right) - \alpha$

(15) $q = \sqrt{(R^2 + Y^2 - 2RY \cos \varphi)}$

The preceding equations were used to calculate $\theta(\phi)$. The ratio of the rotation rates, $d\theta/d\phi$ was found by taking finite differences. For the range of values of $0 < \phi < 15$ degrees, the ratio of the rates of rotation of the two gears varies by a factor of about for the configuration $R_1 = R_2 = 10$, $R_{min} = 9$, $R_{max} = 11$. As the tip of the driving tooth slides along the face of the driven tooth, it may reach a point where the motion of the tip, as the gear rotates, is parallel to the face. The tip vector itself is perpendicular to the face. Then $d\theta$ vanishes. For continuous motion of the driven gear, the next teeth on the two gears must engage before this happens.

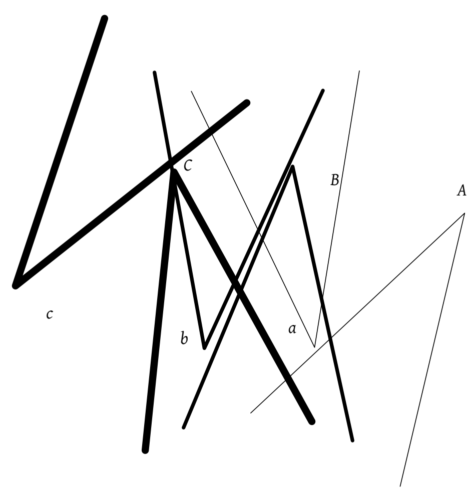

Figure 6. The discontinuity in rotation rates occurs as the gears pass through configuration *Bb*. The line of contact is *abBCa*. It comprises two circular arcs, *ab* and *BC*, and two discontinuous jumps, *bB* and *Ca*.

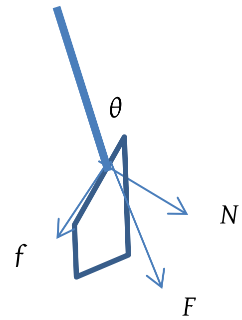

Figure 7. Friction.

Another way to measure the variation in the ratio of the rotation rates is to simulate the actual gears using computer generated gears that can be rotated independently. Begin with the tip of a tooth on gear 1 just touching the face of a tooth on gear 2. Rotate gear 1 through a small angle $d\phi$. Then advance gear 2 in small steps to find the angle $d\theta$ that just maintains the contact of the tip to the face. Repeat for several intervals $d\phi$ within the passage of one tooth. Figure 6 shows the results of one such measurement.

A, *B*, and *C* mark the tip of a single tooth at three times. The positions of the tip of the mating tooth on a second identical gear, at the same three times, are marked *a*, *b*, and *c*. At time *A*, the tip – *a* – of the tooth on the upper gear makes contact with the face of the tooth on the lower gear. At time *B*, the teeth meet face to face. At *C*, the tip of the lower gear meets the face of the upper gear. The rotation of each gear during the time intervals *AB* and *BC* is measured by the arc traced out by the tips of the two teeth. Thus the ratio of the rotation of the upper gear to the lower gear is *ab* : *AB* for the first time interval and *bc* : *BC* for the second. These ratios are about 3:4 and 4:3. This demonstrates that the gears must rotate at different rates in order to maintain contact.

When the tip of a tooth of one gear slides along the surface of the tooth of the other gear, friction will oppose the motion. The force *F* that the tip exerts on the face can be resolved into a normal part *N* and a friction part *f*.

(16) $f = F \cos\theta,\ N = F \sin\theta$

Sliding will occur if $f > \lambda N$ where λ is the coefficient of static friction. Thus $\tan\theta < 1/\lambda$ is a necessary condition for sliding to be possible. If θ is bigger than atan $(1/\lambda)$ the gears jam. In Figure 8, the lower gear drives the upper. At the instant shown contact is being lost at *A* and transferred

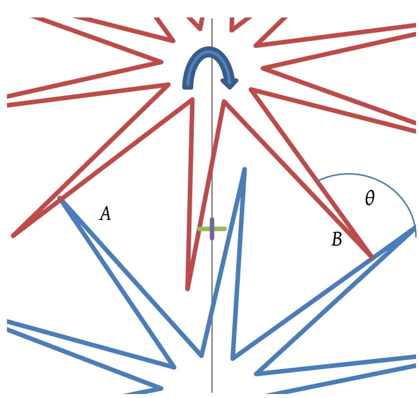

Figure 8. The gears jam when the pressure angle θ exceeds atan($1/\lambda$).

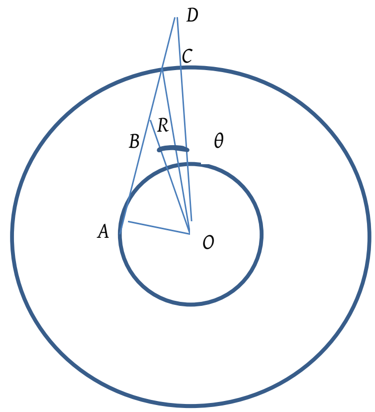

Figure 9. The sides of a triangular tooth are tangent to a circle *OA* < *OC*.

to *B*. However the pressure angle θ is too large to satisfy the sliding condition, and the gears are jammed. Pushing harder won't help.

Let us now construct the tooth profile conjugate to the triangle. We know already it will not be a triangle, because gears with triangular teeth do not transmit uniform rotations. We begin by constructing the line of contact using the condition that the normal to the profile must pass through a fixed point,—the fundamental theorem.

The correspondence between points on the triangular gear tooth and points on the line of contact

Take polar coordinates with origin at the center of the gear. Measure angles positive clockwise from the line joining the centers of the two gears. Then functions of the form $r(\theta)$ can be used to describe the shape of the gear tooth profile and of the line of contact. We will use the notation $r(\theta)$ for the gear tooth profile and $r_p(\theta)$ for the line of contact. There is a one to one correspondence between the two, so our task can be stated as: Given a point r, θ on the tooth profile, find the corresponding point on the line of contact. We will find that this latter point has the same radial coordinate, so our strategy is to pick r, find θ such that r, θ is on the tooth profile, and then find θ_p such that r, θ_p is on the line of contact.

Define the triangular gear tooth with the aid of a circle of radius A as shown in Fig. 9. Here *OA* has length A, *OC* has length R, *OB* has length r and angle θ from the vertical. The circle *OC* is the pitch circle. The line *ABCD* defines one side of the triangular tooth profile. The arbitrary point *B* has coordinates r, θ. We note $DOB = \theta$.

The following sequence of calculations takes us from a point on the tooth profile to a point on the line of contact.

(17) $\psi = \operatorname{asin}(A/r)$

(18) $\theta = \psi - \operatorname{asin}(A/R)$

(19) $\varphi = \operatorname{acos}(r \cos \psi/R)$

In rectangular coordinates with origin at the pitch point,

(20) $x_p = r \sin(\psi - \varphi)$

(21) $y_p = R - r\cos(\psi - \varphi)$

Or, if you like,

(22) $x_p = {}^1/_R \sqrt{(r^2 - A^2)}\,[A - \sqrt{(A^2 + R^2 - r^2)}]$

(23) $y_p = {}^1/_R\,[(A^2 + R^2 - r^2) - A\sqrt{(A^2 + R^2 - r^2)}]$

if we take R to be the unit of length. We obtain the dimensionless equations:

(24) $x_p = \sqrt{(r^2 - A^2)}\,[A - \sqrt{(1 + A^2 - r^2)}]$

(25) $y_p = (1 + A^2 - r^2) - A\sqrt{(1 + A^2 - r^2)}]$

—which show that the line of contact depends only on the ratio A/R. This procedure can be reversed: given points x_p, y_p on the line of contact, find corresponding points r, θ on the tooth profile. And finally, the same procedure can be used to take as given the point on the line of contact, and to find the corresponding point on the tooth profile of a gear conjugate to the initial gear with the triangular teeth. In this case:

(26) $r_2 = \sqrt{[(R_2 + y_p)^2 + x_p^{\ 2}]}$

(27) $\psi_2 = \mathrm{acos}(R_2 \cos\varphi / r_2)$

(28) $\varepsilon_2 = {}^{-R_1}/_{R_2}\,\varepsilon_1$

(29) $\theta_2 = \psi_2 - \varphi - \varepsilon_2$

The cut-away-everything-that-doesn't-look-like-a-gear method

Consider a gear blank with as yet undefined teeth. If both gears are to have the same number of teeth, then the blank gear will complete one clockwise revolution in the same time that the

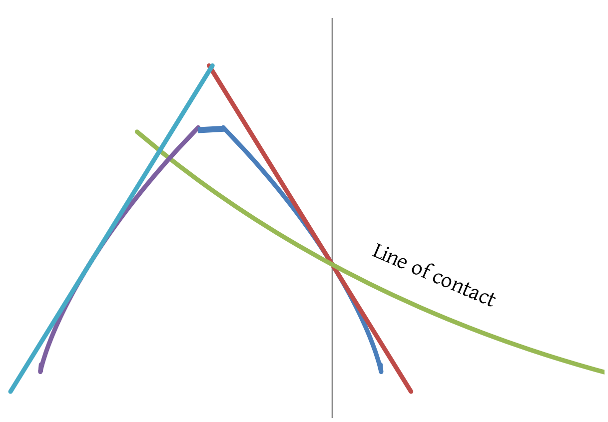

Figure 10. The triangular tooth profile, the line of contact, and the profile that is conjugate to the triangle.

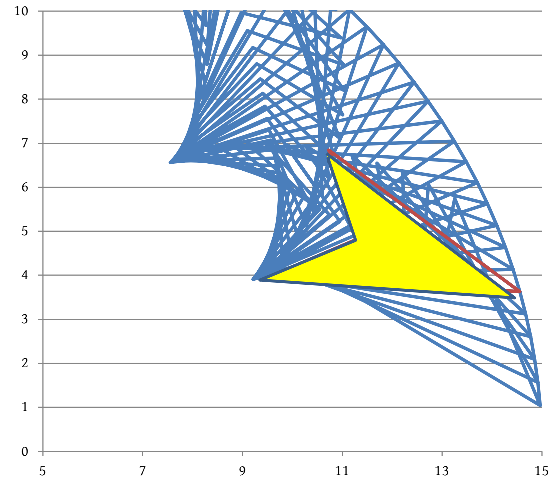

Figure 11. A gear with triangular teeth, only a slice of which is seen here, is rolled around a fixed circular gear blank, cutting away, as it goes, any material that gets in its way. What remains is the conjugate tooth shape conjugate to the triangle. Compare to the conjugate shape determined by analysis in figure 10.

other completes one counter clock wise revolution. If one gear rotates at the rate ω, the other will rotate at the rate $-\omega$. If now we consider the situation from the view point of an observer for whom the first gear is at rest, then, for him, the second gear rotates at the rate -2ω about its center, and rolls around the circumference of the stationary gear. If we imagine the second gear as cutting away at the blank gear every place they overlap, what remains is the shape of the conjugate gear.

Summary

1. Mating gears with triangular teeth do not rotate uniformly. The driven gear speeds up and slows down during the passage of each tooth. For larger rotations, the motion is uniform.

2. The ratio of the fastest to the slowest speed is roughly $(r_{max}/r_{min})^2$ where r_{max} is measured to the tip of a tooth and r_{min} to the gap between teeth. The ratio is within a few percent of unity for gears with many teeth . But it can be any times unity for small gears.

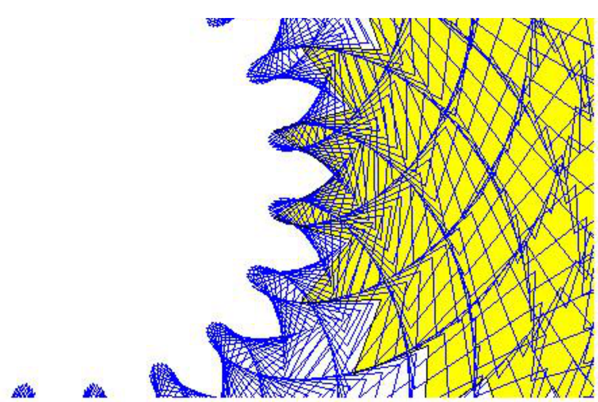

Figure 12. A gear with triangular teeth and its conjugate.

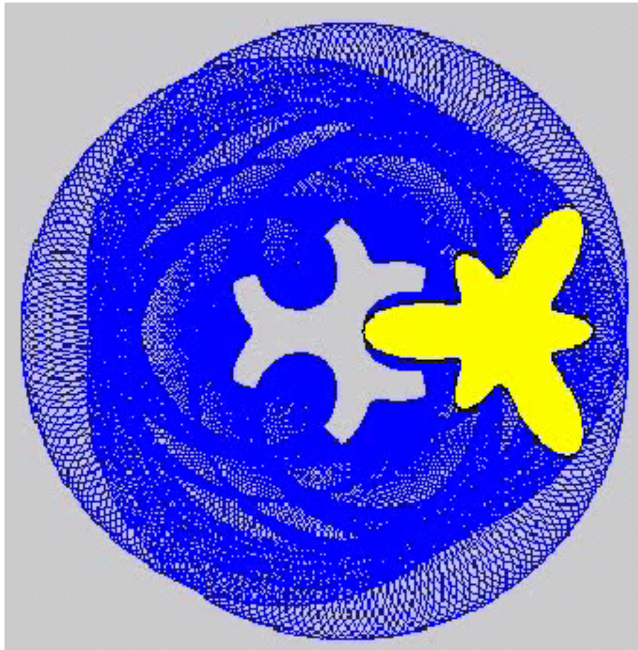

Figure 13. A conjugate pair.

3. Tooth profiles conjugate to the triangle are nearly triangular. The conjugate profiles resemble slightly rounded triangles. So it may be that makers of early gears were able to smooth out the motion of the gears by slightly rounding off the triangular teeth.

4. Finally, Christián Carlos Carman is to blame for these notes being prepared and offered up to honor our dear colleague, Jim Evans.

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The Antikythera Mechanism, Rhodes, and Epeiros

Paul Iversen

Introduction

I am particularly honored to be asked to contribute to this Festschrift in honor of James Evans. For the last nine years I have been engaged in studying the Games Dial and the calendar on the Metonic Spiral of the Antikythera Mechanism,[1] and in that time I have come to admire James's willingness to look at all sides of the evidence, and the way in which he conducts his research in an atmosphere of collaborative and curious inquiry combined with mutual respect.

It has long been suggested that the Antikythera Mechanism may have been built on the island of Rhodes,[2] one of the few locations attested in ancient literary sources associated with the production of such celestial devices. This paper will strengthen the thesis of a Rhodian origin for the Mechanism by demonstrating that the as-of-2008-undeciphered set of games in Year 4 on the Games Dial were the Halieia of Rhodes, a relatively minor set of games that were, appropriately for the Mechanism, in honor of the sun-god, Helios (spelled Halios by the Doric Greeks). This paper will also summarize an argument that the calendar on the Metonic Spiral cannot be that of Syracuse, and that it is, contrary to the assertions of a prominent scholar in Epirote studies, consistent with the Epirote calendar. This, coupled with the appearance of the extremely minor Naan games on the Games Dial, suggests that the Mechanism also had some connection with Epeiros.

The Games Dial and the Halieia of Rhodes

The application in the fall of 2005 of micro-focus X-ray computed tomography on the 82 surviving fragments of the Antikythera Mechanism led to the exciting discovery and subsequent publication in 2008 of a dial on the Antikythera Mechanism listing various athletic games now known as the Olympiad Dial (but which I will call the Games or Halieiad Dial—more on that below), as well as a hitherto unknown Greek civil calendar on what is now called the Metonic Spiral.[3]

I begin with my own composite drawing of the Games Dial (Fig. 1). As one can see from the composite drawing, the Games Dial on the Antikythera Mechanism is divided into four quadrants labeled as in Table 1.[4] The new reading of ΑΛΙΕΙΑ in the final position of year 4 is published

1 The core of this paper was delivered at a conference at the Lorentz Center in Leiden in June of 2013. I especially want to thank my collaborator John D. Morgan, whose perceptive comments have been invaluable. I also wish to thank the Department of Classics at Case Western Reserve University, the Dean of the College of Arts and Sciences, Cyrus Taylor, the Freedman Center for Digital Scholarship, and the Baker Nord Center for the Humanities for their moral and financial support.

2 Price 1974, pp. 13, 57-62.

3 Freeth, Jones, Steele and Bitsakis 2008.

4 The ∟ is the common abbreviation symbol for ἔτος (year), and the letters Α, Β, Γ, Δ stand for the ordinals 1st, 2nd, 3rd, and 4th.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 17–38. Chapter DOI: 10.5281/zenodo.3975715.

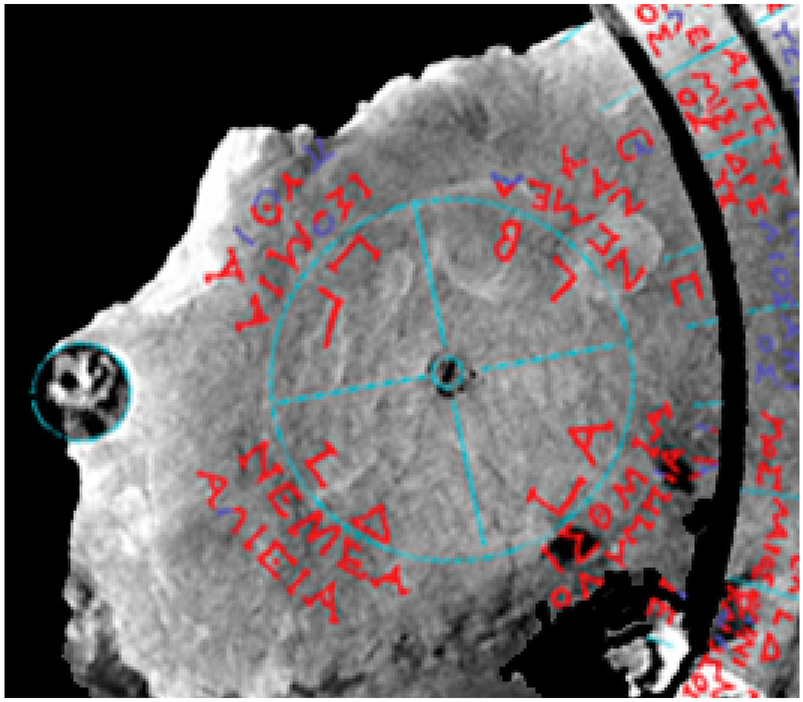

Figure 1. The Games/Halieiad Dial.

here in full for the first time and rests upon the following evidence from the micro-focus X-ray computed tomography.[5]

∟Α´	∟Β´	∟Γ´	∟Δ´
Ἴσθμια	Νέμεạ	Ἴσθ̣μια	Νέμεα
Ὀλύμπια	Νᾶα	[Π]ύθ[ι]α	Ἁλίεια
Year 1	Year 2	Year 3	Year 4
Isthmia	Nemea	Isthmia	Nemea
Olympia	Naa	[P]yth[i]a	Halieia

Table 1. Inscriptions of the Games/Halieiad Dial.

It should first be noted that each CT-generated photo is a slice of the Mechanism at a particular angle, direction and depth (and these slices can be combined into an accurate 3D X-ray volume and viewed with CT viewing software). On each slice only parts of some letters are visible, some more sharply than others. One must, therefore, stack up several image slices (in Adobe *Photoshop*) with the same absolute placement and read the images in a sequence. As the angle, direction and depth of each X-ray changes slightly, some letters or letter strokes that were blur-

5 This article is partly based on data processed, with permission, from the archive of experimental investigations by the Antikythera Mechanism Research Project in collaboration with the National Archaeological Museum of Athens (see T. Freeth, Y. Bitsakis, et al. 2006. In particular, I want to thank Mike Edmunds for releasing some of these data to me, and Tony Freeth and Alexander Jones for responding to my numerous queries. The mention of the possible reading ΑΛΙΕΙΑ by Zafeiropoulou (2012, p. 247) was based on a communication of my reading to her by other members of the AMRP.

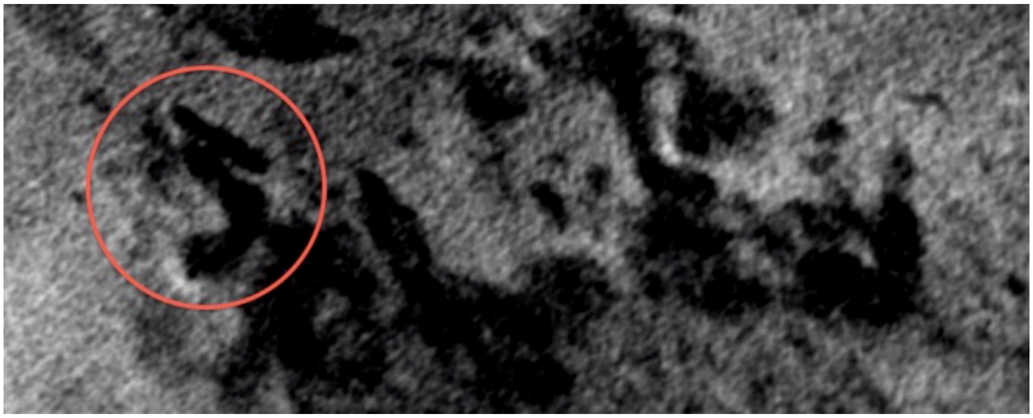

Figure 2. Area of damage above the initial ΑΛ of ΑΛΙΕΙΑ.

ry or invisible in one image come into view or even into focus in another, some more sharp than others, while others become blurrier or disappear altogether. Thus one must make a composite drawing on a graphics tablet by tracing what one sees as one moves through the images, each image adding more strokes to complete the picture. There are sometimes stray shadows or marks of damage (some of this a result of the image slice being at a slightly different angle or level) and often the letters themselves change appearance from looking like deep black grooves to letters in relief, so the technique of reading these images is not foolproof and is subject to interpretation. Nevertheless, the following analysis will demonstrate that the correct reading is ΑΛΙΕΙΑ.

The final two letters, ΙΑ, are clear in several photos, so I believe all would agree these are not in dispute (see Figs. 3, 4 and 5). The interpretation of the first 4 letters, however, is more difficult. Their decipherment begins about ¼ millimeter above them on the surface of the Mechanism, where there are accretions and damage. Here one sees that an area of damage sits above the first two letters, which I have circled in red (see fig. 2).[6] As will become clear, some of this area of damage runs down into the level of letters to interfere with their reading, and it is important to distinguish this damage from the letters themselves.

Going to a slice a little below, one can see that the outlines of 6 letters appear, along with some of this area of damage circled in red (Fig. 3). Here it is especially important to note that to

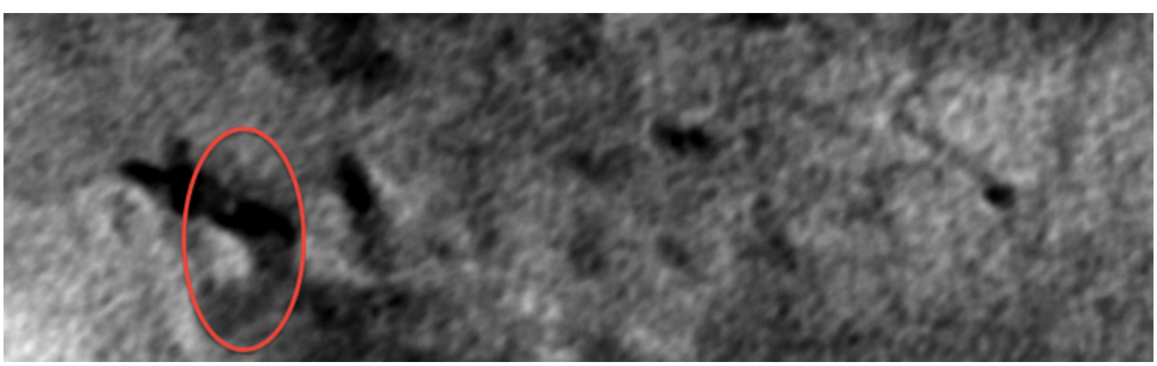

Figure 3. Damage at the level of the letters.

6 Special thanks to the late John Seiradakis and to Magdalini Anastasiou, who sat down with me in Thessaloniki and using the Studio Max VG software snapped these pictures. I also wish to thank Magdalini for sharing the beautiful Antikythera Mechanism font that she created.

Figure 4. The six letters of the games in year 4.

the left of the red oval, some new strokes are visible (an apex along with a diagonal) that were not visible in Fig. 2 in this same area. This suggests that these are, in fact, letter strokes. In any case, as is clear from all relevant image slices, there are six letters, no more and no less. Stepping back a bit, all six letters are visible (Fig. 4).

That these are the only six letters is made even more likely by the fact that the inscriber was careful to center names of the games around the Games Dial (with the exception of the Olympia in year one because he ran out of space next to the Metonic Spiral). Adding another letter to the left, the only place where an extra letter could have possibly gone and escaped the notice of the micro-focus X-rays, would have spoiled the appearance.

Fig. 5 is a close-up of what one sees when one removes the layer of noise above the level of the letters. First it is important to note that with the area of noise above removed, all that is visible in this photo is on the same level of the letters. Thus any visible marks that were not visible in fig. 2 are likely to be letter strokes; conversely, any strokes in Fig. 2 that are still visible in Fig. 5 are likely to be damage. Hence the neat and crisp joining-up of the diagonal and cross-bar of the first letter *on the same level as the rest of the letters and not visible in Fig. 2* strongly implies these are purposeful letter strokes, not damage. In addition, in some slices an apex and a left diagonal of this first letter are visible (see Figs. 3, 4 and 7, for example). Finally, these strokes are on an orientation that is slightly rotated, which is exactly what they should be to follow the arc of the

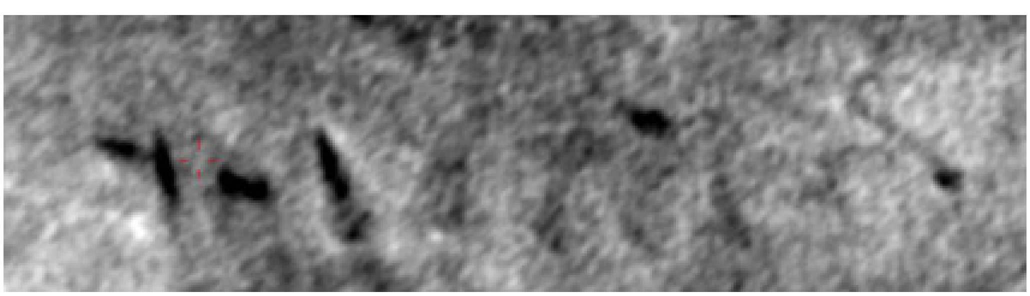

Figure 5. Close-up of 6 letters, with about ¼ millimeter noise above removed.

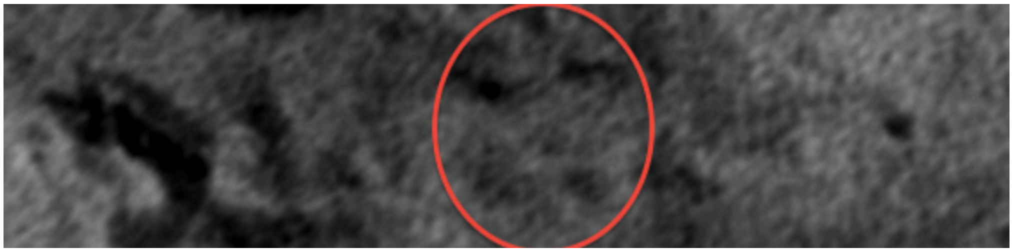

Figure 6. Close up of E.

circle around the Games Dial as the letters of the other games do. Thus the reading of A for the initial letter is extremely likely, if not assured.

To the right of the initial A there is a thick, dark groove. This groove is on an orientation that is the same with respect to the damage above seen in Fig. 2. This is, therefore, very likely to be a part of the area of damage that starts above and continues down into the level of the letters. To the right of this thick, dark groove, one can see the second letter with a right diagonal as well as an apex. Enough of this second letter is preserved in several different slices (see Figs. 3, 4 and 5) to see it cannot be A, Δ, M or N, but only Λ. It too is in the correct orientation for a letter on the arc around the Games Dial.

The third letter consists of one faint vertical hasta that fits between two wider letters, and while a deep groove is not visible in any photo, the outline of an iota, including serifs, is visible in a few slices (see Figs. 3, 4 and 5). Based on spacing (i.e., the need to be a narrow letter) and the outline of a single vertical stroke in some photo slices, I believe the reading of an I is assured.

As for the fourth letter, in a few photo slices, what looks to be the shadowy outline of a N appears. These were the first photo slices I was shown, so this was my first impression (and before I was given the data I spent several weeks of fruitless searching for games that matched the other traces and ended in NIA), but when one looks at all the photo slices, it is clear that this fourth letter is an E, with parts of all strokes visible in some photos, especially the bottom horizontal and lower left corner (see Figs. 6 and 7), and that the seeming diagonal of a N in some photos is

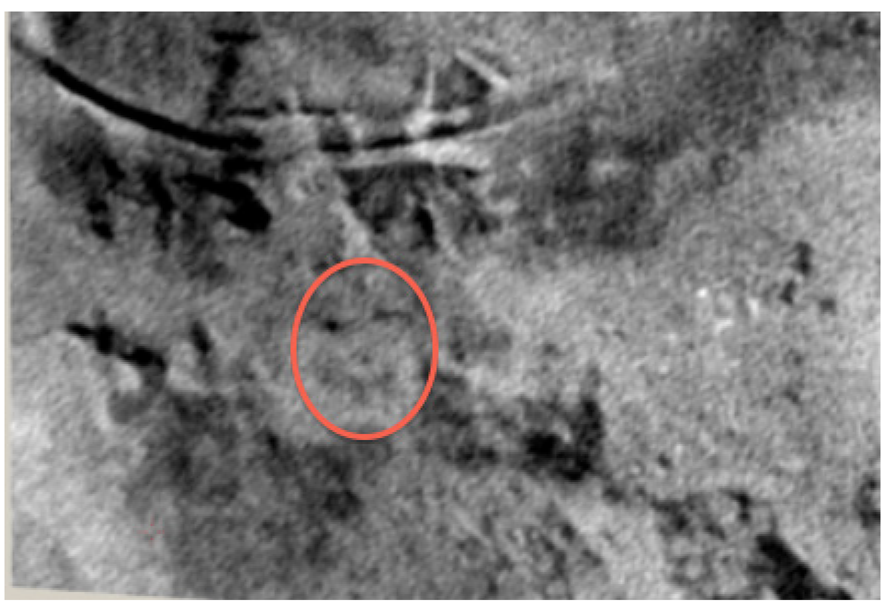

Figure 7. E from another angle.

actually a result of mistaking pieces of the upper left corner, the middle horizontal, and right side of the lower horizontal of the E for the diagonal of a N.

This fourth letter was the key to reading the games, and if I have correctly identified the letter strokes and the damage, epigraphically speaking the reading ΑΛΙΕΙΑ may be given with no dotted letters (see composite drawing, Fig. 1).[7]

In addition to the epigraphical considerations, this reading is strengthened further by the fact that no other games of six letters consistent with the visible traces and ending in ΕΙΑ are attested in Greek or Roman sources.[8] Finally, the AMRP team sensibly argued that these games were listed in chronological order,[9] since otherwise it would make no sense that the Isthmian games were listed before the Olympia and Pythia.[10] This hypothesis finds strong corroboration by the identification of the Rhodian Halieia, which the Scholiasts to Pindar say ended six days after the Nemean games had finished.[11] Apart from the Scholiasts' testimony that the Halieia were in the same year of the Nemea and shortly after them, from inscriptions we know that the Great Halieia were held every four years (the Greeks referred to this as pentaeteric, or every five years,

7 Based upon my composite drawing of letter traces, I am obligated not to dot any letters (the art of the epigrapher is to distinguish between real letter strokes and damage, make a drawing of the actual letter strokes, and then give a text *according to the drawing*). There is, of course, always the possibility that all the photo slices deceive in their totality, but this seems very unlikely.

8 The only other two attested possibilities that come even close are the Ἰλίεια of Ilion (see Hesychius: Ἰ λ ί ε ι α · ἑορτὴ ἐν Ἰλίῳ Ἀθηνᾶς Ἰλιάδος καὶ πομπὴ καὶ ἀγών). These are attested epigraphically one time (*IG* II[2] 3138, end of 4[th]/beg. of 3[rd] BC, but see Frisch 1975, p. 130, n. 52 for other references). These, however, appear to have been called the Panathanaia after 306 BC. There are also the Δίεια in honor of Zeus at Tralleis in Karia, which are also attested epigraphically only one time (*SEG* 22.350, l. 28). These, however, have only 5 letters, plus a delta in a position on the Mechanism for which there is no evidence of a lower horizontal where there should be, if this were the correct reading.

9 Freeth, Jones, Steele and Bitsakis 2008, p. 20.

10 The only other likely system for the arrangement is that of having the trieteric games on the inner circle, and the pentaeteric games on the outer circle, but given the Scholiast's information (see note 10), it appears the maker had the games arranged trieterically/pentaeterically and chronologically.

11 The Scholiast to Pindar *Olympian* 7 147c, who was probably relying upon Istros' lost work περὶ τῶν Ἡλίου ἀγώνων (*On the Games of Helios*), says of the Halieia τελεῖται δὲ μηνὸς Γορπιαίου εἰκοστῇ τετάρτῃ ἡμέρᾳ. ἀπέχει δὲ τῶν Νεμέων ἡμέρας ἕξ ("they finished on the 24[th] day of the month of Gorpiaios, six days from/after the Nemea."). The *Hypothesis to the Nemea* d,e also says of the Nemea: καὶ ἔστι τριετὴς, τελούμενος μηνὶ Πανέμῳ ιη´ ("they are trieteric, and they finish on the 18[th] of the month of Panemos"). Most scholars have accepted the authority of these two scholia that the the Halieia finished on the 24[th] six days after the Nemea had finished on the 18[th], but Perlman (1989, pp. 57-60) argued that the word ἀπέχει means "before" rather than its most natural meaning "after", so that the Nemean games finished after the Halieia around the new moon (last day) of Argive Panamos rather than on the 18[th] of the month, six days before the end of the Halieia. She also ignored another Pindaric scholion that equated Argive Panamos with Julian *Iulius* (Mommsen 1867 *Nem. Hyp.* schol. 1, l. 38: ἤγετο δὲ μηνὶ Πανέμῳ $\overline{\text{τη}}$, ὅς ἐστιν Ἰούλιος), the month that normally most closely corresponded to Athenian Hekatombaion, and argued instead for late August or even early September (i.e., the season that normally most closely corresponded to the end of Athenian Metageitnion). Perlman's heterodox view is now virtually excluded by the evidence of the Antikythera Mechanism. As for Perlman's contention that the Gorpiaios mentioned here is from the Seleucid calendar, it is more likely it refers to Alexandrian Gorpiaios, since Istros is known to have worked with Kallimachos in Alexandria in the mid third century BC. In the time of the Roman Empire, which is the time when the Scholiast was probably working, Alexandrian Gorpiaios ran from June 25 to July 24, and thus corresponded most closely to Athenian Hekatombaion and Julian *Iulius* (see Samuel 1972, p. 177). My own thorough review of the evidence of the Nemea has concluded that they normally fell in the lunar month coincident with Athenian Hekatombaion (July/August), although there may have been times when they were in the lunar month normally coincident with Athenian Skirophorion (June/July).

because lacking the concept of zero they counted inclusively).[12] In sum, when all this powerful and interlocking evidence is taken together, I believe the reading of Halieia is assured; thus the Games Dial is complete.

Games Dial/Halieiad Dial vs. Olympiad Dial

Naturally, a dial on a Greek time-reckoning device listing athletic games including the Olympia in a four-year period makes one immediately think of Olympiads, the four-year period between Olympic festivals that was a standard means to reckon time in Magna Graecia, but year 1 of an Olympiad in ancient Greek literary sources began with the celebration of the Olympia in even years BC divisible by 4 (200, 196, 192 etc.) and ran until the same time of year in the next year when year 2 of an Olympiad began,[13] whereas on this dial year 1 is beginning sometime between the Halieia of Rhodes (in roughly July of odd years one year before the Olympia such as 201, 197, 193…, if the testimony that they finished six days after the Nemea is reliable, which now seems confirmed by the Mechanism) and the Isthmia at Corinth (in the spring of even years BC) and it is ending with the Olympia, not beginning with them. To put this another way, the Isthmia definitely fell in years 2 and 4 of a traditional Olympiad, not in years 1 and 3,[14] hence the years on this dial cannot refer to the four individual years of an Olympiad.

It therefore appears that this dial does not reference individual Olympiad years in the traditional sense, which is why I prefer to call it the Games Dial, although one could rightly also call it the Halieiad Dial, since it does accurately represent Halieiad years. What other function this four-year period on the Mechanism had is not clear, but it may be that it was somehow used in combination with the 365 holes for the days of a solar year on the front of the Mechanism to achieve a leap-year function.

The Halieia of Rhodes and the Naa of Dodona/Ambrakia

The presence on the Games Dial of the Isthmia at the Isthmos of Corinth, the Olympia of Elis in the Peloponnese, the Nemea of Argos in the Peloponnese, and the Pythia of Delphi in Phokis is

12 See especially *SER*, p. 259, no. 5b (= *IG* XII,1 730), which lists the Halieia as being celebrated under the priests of Apollo Erethimios in years 8, 12, 16, 20, 24 and 28 of the list, thus assuring their pentaeteric cycle (the Halieia that should have appeared under the priest in year 4 was probably cancelled due to war; I would argue this apparently skipped year corresponds to the games of 85 BC at the very end of the First Mithridatic War). It is true that some inscriptions refer to both the Great and Small Halieia: *IK RhodPer* 555, l. 14: [Ἁ]λίεια τὰ μεγάλα καὶ τὰ μικρὰ δίς (mid. of 2nd cent. BC). Numerous other inscriptions mention only the Great Halieia: *Clara Rhodos* 2 (1932) p. 190, no. 19, l. 15 (early 1st cent. BC); *Clara Rhodos* 2 (1932) p. 188, no. 18, l. 16 (1st cent. BC?); *Clara Rhodos* 2 (1932) p. 210, no. 48, l. 4 (*ca.* 100-50 BC); *NSERC*, no. 36 (Roman period); *SER* 5, face b, l.3 (AD 4/5?); *Tit. Cam.* 75 (undated). It is hardly surprising to see the Small Halieia were largely ignored.

13 I have thoroughly reviewed the literary sources, and it is clear that the ancient historians who used Olympiad reckoning and whose works survive in quantifiable amounts (such as Polybios, Diodorus Siculus, and Dionysios of Halikarnassos) began a new Olympiad with the celebration of the Olympia, although they were not always precisely sure when it occurred given the variable nature of Greek luni-solar calendars, and they counted the individual years within an Olympiad by assuming the festival was always roughly at the same time of year (the evidence suggests between the middle and end of summer).

14 Based on a thorough examination of the season of the Nemea, Halieia and Olympia, I would also argue that the Nemea fell at the end of years 1 and 3 of a traditional Olympiad, and that the Halieia fell at the end of year 3. Although the Olympia and Pythia are in the correct Olympiad year, they should be at the beginning of years 1 and 3, not the end. I would also argue that the Naa should be at the beginning of Olympiad year 2, not the end of year 2.

hardly surprising – these were the four most prestigious Panhellenic games of Greco-Roman antiquity. The Halieia of Rhodes and Naa of Dodona in Epeiros, on the other hand, were relatively unimportant games, especially the Naa, so they do require an explanation.

As for the Halieia, 35 of the 37 epigraphical attestations of the spelling Ἁλίεια are found on inscriptions from the island of Rhodes or the Rhodian Peraia.[15] Of the remaining two, one attestation is found at Miletus on the west coast of Asia Minor,[16] and a second copy of this same inscription at nearby Didyma.[17] In sum, if other inscriptions are any guide, any attestation of the spelling Ἁλίεια,[18] with an alpha and two iotas, has a 95% likelihood of coming from Rhodes itself, or in reality a higher percentage when one considers that the one attestation at Miletus and the one at Didyma are on monuments honoring the same athlete. That the Halieia were largely local and regional games for second-tier (or possibly older) athletes is not surprising, given that the games apparently often started just days after the Nemea had ended so there was not enough time for premier athletes to participate in both games in the same year.

The appearance of the Doric form Ἁλίεια coupled with the fact that the Halieia were largely local and regional games, point to some relationship of the Mechanism to Rhodes, and this is especially notable given that the rest of the preserved inscriptions on the Mechanism employ Attic-Ionic forms.[19]

Apart from the Halieia, the other odd set of games on the Games Dial are the Naa. These games are even more obscure than the Halieia, with the spelling Νᾶα attested only once at Dodona,[20] once at Tenos,[21] once at Sikyon,[22] once at Priene,[23] three times at Athens (two of these on a single monument for Menodoros),[24] and two times at Delos (on a single monument, also for

15 *IG* XII,1 57, l. 8 (#1); *IG* XII,1 72a, l. 2 (#2); *IG* XII,1 73, a, l. 3 and b, l. 3 (#s 3, 4); *IG* XII,1, 74, l. 2 (#5); *IG* XII,1 75, b, l.2 (#6); *SER*, p. 259, no. 5b (= *IG* XII,1 730), ll. 9a, 13a, 21a, 26a, and 32a; line 17a has the spelling ΑΛΕΙΑ, and although this spelling is common later, here it is almost certainly an error for Ἁλ⟨ί⟩εια, the spelling elsewhere on this same inscription (#s 7, 8, 9, 10, 11, 12); *IG* XII,1 935, l. 2 (#13, partially restored); *ILindos* 2.322, l. 10 (#14); *ILindos* 2.392, b, l. 8 (#15, partially restored); *ILindos* 2.707, l. 2 (#16); *Tit. Cam.* 63, ll. 22-23 (#17); *Tit. Cam.* 75, l. 6 (#18); *Clara Rhodos* 2.188,18, l. 16 (#19); *Clara Rhodos* 2.190,19, l. 15 (#20); *Clara Rhodos* 2.210,48, l. 4 (#21); *NSERC* no. 18, l. 6 and no. 19, l. 9 and no. 36, l. 4 (#s 22, 23, 24); *SER*, no. 4, face b, l. 3, face b, l. 16, and face c, l. 4 has the spelling ΑΛΙΑ, which is probably an error for Ἁλί⟨ει⟩α as is the spelling elsewhere on this same stone, and nos. 18 and 19 (#s 25, 26, 27, 28, 29); *NSER* p. 125, no. 25, l. 2 (#30); *SEG* 39.759, ll. 5 and 16 (#s 31, 32); *IK RhodPer* 555, l. 14 (#33); *SEG* 43.527, l. 22 (#34); Zimmer and Baïrami, 2008, pp. 149-154, l. 1 (#35). The restoration of [Ἁλίε]ια at *IG* XII,1 1039, l. 2 from the islet of Saros/Saria is not certain (the drawing in *IG* suggests the letter where there should be an Ε is more consistent with a Σ, yielding perhaps [Πύθια ἐν Μαγνη]σίᾳ, or [Ἀρτεμ(ε)ί]σια, or [Ἡμερά]σια), etc.

16 A. Rehm *et al.* 1958, no. 201, l. 11.

17 Gerkan and Krischen 1928, no. 369, l. 20.

18 There are a three instances of the Attic-Ionic form Ἡλίεια (one at Athens, one at Samos and one at Ios), and several instances of the spelling Ἅλεια both around the Greek world and at Rhodes (which seems to be a later spelling that begins at the end of the first century BC). These alternative spellings are interesting, but for the purpose of this argument they can be ignored since they are not the spelling on the Mechanism.

19 For a history of the Halieia games, see my forthcoming article in *Eulieme: Studies in Classical Archaeology, Epigraphy, Numismatics and Papyrology* scheduled to come out in the fall of 2020.

20 *L'Épire* 586,71.

21 *SEG* 37.709, l. 13.

22 *IG* IV 428, l. 8.

23 Freidrich and Hiller von Gaertringen 1906, no. 234.

24 *Agora* XVIII, C-196, crowns 25 and 28; *SEG* 38.179 (here restored but likely = *IG* II^2 3152+3153), crown X.

the same Athenian Menodoros),[25] and once at Rhodes, where the games are said to be at Ambrakia.[26] There is also the spelling Νάϊα attested one time at Tegea and three times at Messene.[27] Of these 14 attestations, it is important to note that four, or almost 30% of all attestations, are in honor of the same Athenian athlete, Menodoros, who had monuments in his honor that were erected in both Athens and Delos ca. 135-130 BC that included his two Naan victories. The relatively rare attestation of the Naa in our sources suggests that the Mechanism also had some tie to Epeiros.

The Calendar on the Metonic Spiral and Epeiros

A further tie to Epeiros is found on the Metonic Spiral of the Antikythera Mechanism. Before summarizing the results of a comprehensive study of this calendar here,[28] first I want to address the question of whether the calendar on the Mechanism can be that of Syracuse, the home of Archimedes. The evidence clearly indicates that this is not possible, as the months Apo[llonios] (restored but virtually certain and not on the Mechanism),[29] Karneios (not Kraneios as on the Mechanism),[30] Artamitios with a tau (not Artemisios with a sigma as on the Mechanism),[31] Panamos,[32] and Apellaios[33] are attested at Syracuse and its nearby military outpost of Akrai. In addition, the month name Damatrios is attested on an inscription that probably came from the area of Syracuse,[34] which is a month name also found at the Syracusan foundation of Tauromenion, but not on the Mechanism. In addition, I have now read the month name Ἑλώρειος on an inscription from Tauromenion,[35] but this month name probably originated at Syracuse (Syracuse had military fort at its southern border at the mouth of the Heloros river called Heloros, where we are told games called the Helor(e)ia were held,[36] probably as a part of the festival after which the month Heloreios was named). It is thus likely that Tauromenion's calendar, which is fully known (see Table III), came directly from Syracuse, the co-founder of Tauromenion.

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25 *IDelos* 1957, crowns 25 and 28.

26 Zimmer and Baïrami 2008, pp. 149-154, lines 10-11, where the inscription reads reads ... Νᾶα ἐν Ἀμ[- - - -]/ [- - - -]ς παγκράτιον..., and the correct supplement is almost certainly Νᾶα ἐν Ἀμ[βρακίαι· -?-] / [. . *ca.* 10. .]ς παγκράτιον. See Iversen 2017, pp. 147-148.

27 *IG* V,2 118, l. 21. The three examples from Messene are as yet unpublished, but will be by Andronike Makres (whom I would like to thank for allowing me to mention them).

28 The fuller article may be found at Iversen 2017.

29 *IMagnesia* 72, line 3.

30 Plutarch, *Nikias* 28.1-2.

31 *SEG* 42.836, line 4 (from Akrai, a dependent of Syracuse).

32 *SEG* 42.833, line 8 (from Akrai).

33 *SEG* 42.832, line 8 and probably *SEG* 42.835, line 6 (from Akrai).

34 *SEG* 47.1462. The provenance of this inscription is in doubt; Manganaro (1997, no. III) originally argued for the area of Syracuse, but later changed his mind (Manganaro 2011 = *BullÉp* 2012.520) and now believes it comes from Halaisa Archonidea. The dating of *SEG* 47.1462, however, by the eponymous *amphipolis* (which is only attested as the eponymous office at Syracuse) and tribal/phratry designations in the form of ordinal numbers on this inscription strongly suggest Syracuse.

35 *IG* XIV 426, col. II, line 5 (I now read Ἑλωρẹίου) and Manganaro 1964, pp. 42-52, col. III, l. 30 (read Ἑλ[ωρείου πρ. Διο]γένης).

36 Iversen 2017, pp. 131-134. *Cf.* Hesychius E, 2253: Ἑλώριος ἀγών· τελούμενος ἐπὶ Ἑλώρου ποταμοῦ.

Antikythera Mechanism	*Epirote (Cabanes)*	*Order/Seasons of Epirote (Cabanes)*
Phoinikaios	Kraneios	7 (Aug./Sep.)
Kraneios	Panamos	8 (Sep./Oct.)
Lanotropios	Apellaios	9 (Oct./Nov.)
Machaneus	Machaneus	10 (Nov./Dec.)
Dodekateus	Deudekateus	11 (Dec./Jan.)
Eukleios	Eukleios	12 (Jan./Feb.)
Artemisios	Artemisios	1 (Feb./Mar.)
Psydreus	Psydreus	2 (Mar./Apr.)
Gameilios	Gamilios	3 (Apr./May)
Agrianios	Agrianios	4 (May/June)
Panamos	Phoinikaios	5 (June/July)
Apellaios	Haliotropios	6 (July/Aug.)

Table 2. Comparison of the Mechanism's calendar with Cabanes's reconstruction of the Epirote calendar.

As for other possible homes for the calendar, contrary to the claims of Cabanes,[37] a leading scholar on Epeiros, the calendar on the Mechanism is consistent with all the known available evidence for the Epirote calendar. Here I give the calendar on the Mechanism and the reconstruction of the Epirote calendar by Cabanes side by side. As one can see, Cabanes disrupted the order of the five months in bold, started the calendar with Artemisios rather than Phoinikaios, and assigned the month Phoinikaios to June/July (the month normally coincident with Athenian Skirophorion), and the month Apellaios, in which the Naan games fell, to October/November (the month normally coincident with Athenian Pyanopsion).

The main reasons Cabanes argued for this different starting point, order, and the particular seasons are because he believes that there is evidence at Korkyra. where we know Artemitios comes after Eukleios as it does on the Mechanism, that Eukleios was the 12th month, hence Artemisios the first.[38] He also believes there is another month attested in Epeiros besides the twelve months on the Mechanism (namely the additional month of Haliotropios), which based on its etymology meaning "turning about the sun" should be placed around the time of a solstice.[39] Finally, he believes the Naa games, which are known to have been held in the month of Apellaios,

37 Cabanes 2011, pp. 249-260. See also Cabanes, 2003 = *CIGIME* 2.2, pp. 275-288.

38 *IG* IX,1² 4 798, ll. 50-51: ἐγδανεισάντω / ἐμ μηνὶ Δυωδεκάτωι καὶ Εὐκλείωι τῶι ἐπὶ Ἀριστομένεος (Cabanes reads instead ἐγδανεισάντω / ἐμ μηνὶ δυωδεκάτωι 〈τῶι〉 καὶ Εὐκλείωι τῶι ἐπὶ Ἀριστομένεος, turning what is clearly the month Dyodekatos into the ordinal 12th and positing a scribal error by inserting 〈τῶι〉).

39 I have inspected the stones or photos of the all the alleged instances of Haliotropios (with two iotas), and the correct reading on these is Lanotropios (as on the Mechanism), or what appears to be a variant (H)alotropios (with only one iota). Thus Lanotropios at *IMagnesia* 46, lines 2-3 (Epidamnos) and *L'Épire*, p. 553, no. 32 (Dodona). There is also (H)alotropios at *IMagnesia* 45, line 2 (Apollonia), as well as at *CIGIME* 2.2 76, line 3 and *CIGIME* 2.2 77, line 3 (both from Bouthrotos). I believe (H)alotropios and Lanotropios are different spellings for essentially the same month (based on two different roots that have a similar meaning), just as Karneios/Kraneios and Dodekateus and Dyodekatos/Deudekateus are.

fell in October/November.[40] Elsewhere I have demonstrated that all these claims are demonstrably wrong, or likely wrong.[41]

The Antikythera Mechanism and the Corinthian Family of Calendars

In Table III I summarize the evidence for months for what I call the Corinthian Family of Calendars. As one can see, apart from mostly minor orthographical differences, there is remarkable consistency in the names of the months attested at Corinth and its colonies in Northwest Greece and the cities of Epeiros, and that these months are consistent with those found on the Antikythera Mechanism. The most economical explanation of this remarkable consistency, with a total of at least 125 individual attestations,[42] is that all the colonies of Corinth in Northwest Greece retained the calendar of Corinth with very few changes, which was the same or very similar to that on the Antikythera Mechanism.[43]

The evidence from Epirote cities is also quite consistent with the calendar on the Mechanism. The simplest explanation for this is to posit that at some point the Epirote Confederacy as a whole required that its member states adopt a fairly uniform calendar,[44] and that calendar belonged to a city that was originally a colony of Corinth that had retained the Corinthian calendar.

The Likely Candidates for the Calendar on the Metonic Spiral

As for which cities could be the home of the calendar on the Metonic Spiral on this list, as we have already seen the calendar of Syracuse and its colonies in Sicily, can now be eliminated. Of those left in Table III, significant deviations from the Mechanism are found in the months of Alotropios at Bouthrotos and Apollonia instead of Lanotropios, and the apparent form Δυωδέκατος/Δευδέκατος at both Korkyra and Apollonia rather than the third declension form Δωδεκατεύς. All these places have direct ties to Korkyra, and so it seems likely that Korkyra and its colonies can be eliminated as homes for the calendar on the Mechanism.

Of the remaining candidates for the calendar of the Mechanism, the most likely are Corinth, Ambrakia, Dodona, and Epidamnos. If the Mechanism was built after the destruction of Corinth by Lucius Mummius in 146 BC, which is still an open question but seems likely to me,[45] then

40 The evidence that the Naa games fell in October/November rests upon a demonstrably wrong argument made long ago by Klee 1918, pp. 54-55.

41 See Iversen 2017, pp. 149-159. Daux 1956 also argued that a month of Datyios is attested at Dodona at *L'Épire*, p. 534, no. 1, line 19. For a good photo, see the *editio princeps* of Evangelidis 1956, p. 2. I argue instead that this was a man's name.

42 The data concerning the number of attestations are a bit skewed due to the 74 attestations of months at Bouthrotos alone, but nevertheless the remainder are an impressive amount of evidence.

43 That it was not unusual for colonies to retain the calendar of their mother city is demonstrated by Olbia, which clearly kept the entire calendar of its mother city, Miletos. See *SEG* 30.977 and 53.788.

44 The inscription published by Tziafalias and Helly 2007, ll. 57-83 (= *SEG* 57.510) suggests that the (post 167 BC) Koinon of the Molossoi possessed a common calendar, which was probably the same as the earlier Epirote calendar.

45 Carman and Evans 2014, Freeth 2014 and Jones 2020 have all persuasively argued the Saros Eclipse Dial has a start-up date of April 29, 205 BC. It appears the designer deliberately picked this date because the sun and moon were both very close to their apogees at this month's full moon. As Alexander Jones has pointed out to me, this does not happen very often (the full moon on May 12, 205 BC is a better candidate than that of May 12, 91 BC or May 12

Mainland/Northwestern Greece

Corinthian-Epirote

Antikythera	*Corinth*	*Ambrakia*	*Charadros*	*Dodona*	*Gitana*	*Byllis*	*Epidamnos*
Φοινικαῖος	Φοινικαῖος	Φοινικαῖος	Φοινικαῖος	Φοινικαῖος			
Κρανεῖος							
Λανοτρόπιος				Λανο[τ]ρόπιος			
Μαχανεύς AB[46]							Μαχανεύς
Δωδεκατεύς							
Εὔκλειος				Εὔκλειος			
Ἀρτεμίσιος		Ἀρτεμίσιος	Ἀρτεμίσιος				
Ψυδρεύς		Ψυδρεύς	Ψυδρεύς			Ψυδρεύς	
Γαμείλιος	Γαμίλιος?[47]			Γαμίλιος	Γαμίλιος		
Ἀγριάνιος	Ἀγριάνιος?[48]						
Πάναμος	Πάναμος		Πάναμος	Πάναμος			Πάναμος
Ἀπελλαῖος				Ἀπελλαῖος			Ἀπελλαῖος
Intercalated month							ἐμβόλιμος[49]

72 BC; it is not until the full moon of May 11, AD 308 that another good candidate comes along). Hence, I am (still) of the opinion that the Mechanism was designed within a generation of the Antikythera shipwreck (which sank ca. 70-50 BC), almost surely on Rhodes now that I have deciphered the Halieia on the Games Dial.

46 The intercalation on the Mechanism has now been read at Μαχανεύς in year 11 by various members of the AMRP, including me.

47 I agree with N. F. Jones (1980, pp. 165-177 and 1998) that *SEG* 30.990 (found on Delos) is likely to come from Corinth, or, as Cabanes and Ceka suggest (*CIGIME* 1.2.2.A, pp. 50-51), from Ambrakia. See also *SEG* 56.948.

48 [Ἀγ]/ριạν̣[ίō] may possibly appear on Corinth MF-1975-86, a sacred calendar inscribed on lead found on Corinth's Temple Hill that will soon be published by me in Hesperia (probably first issue of 2021) in an article of unpublished inscriptions from Corinth's Temple Hill. The only other likely supplements are [- - τ]ρ̣ίạ .[- -] or [- - τ]ριạκ̣[άδι].

49 We do not know after which month the intercalary month was inserted at Epidamnos.

Mainland/Northwestern Greece				*Sicily*		
Korkyraian			*Uncertain* ← ? →	*Syracusan*		
Korkyra	*Bouthrotos*	*Apollonia*	*Issa*	*Syracuse*	*Akrai*	*Tauromenion*
Φοινικαῖος	Φοινικαῖος					Ἰτώνιος
	Κρανεῖος Κρανῆος	Κρα[νεῖος]		Καρνεῖος		Καρνεῖος
	Ἀλοτρόπιος	Ἀλοτρόπιος				Λανοτρό[πιος]
Μαχανεύς			Μα[χανεύς]?[50]	Ἀπο[λλώνιος]		Ἀπολλώνιος
Δυωδέκατος		Δευδέκατος				Δυοδεκατεύς
Εὔκλειος	Εὔκλειος Εὔκληος Εὐκλήιος					Εὔκλειος
Ἀρτεμίτιος			Ἀρτεμίτιος		Ἀρτεμίτιος	Ἀρτε[μίτιος]
Ψυδρεύς	Ψυδρεύς	Ψυδρεύς				Διονύσιος
	Γαμείλιος Γαμίλιος			[Ἑλώρειος]		Ἑλώρειος
	Ἀγριάνιος			Δαμάτριος		Δαμάτριος
Πάναμος	Πάναμος	Πάναμος			Πάναμος	Πάναμος
Ἀπελλαῖος	Ἀπελλαῖος	Ἀπελλαῖος			Ἀπελλαῖος	Ἀπελλαῖος
	ἐμβόλιμος[51]					Ἀπελλαῖος β´

50 If the Μα[- - -] preserved on J. Brunšmid 1898, no. 2-14, column I, l. 1. refers to the month Machaneus, it appears that Issa took the form of the Corinthian calendar that was in use by its closer cousins in NW Greece.

51 We do not know after which month the intercalary month was inserted at Bouthrotos.

Corinth could also be excluded. Of these, Ambrakia is the only one with the spelling Ἀρτεμίσιος (attested on an inscription dating to 167 BC[52]), rather than the expected Doric tau.[53] In addition, Ambrakia and Dodona are attractive candidates since the Mechanism mentions the Naan games, which as we saw based on the sparse number of attestations seem to have been a relatively minor set of games. I would, therefore, argue that the Epirote calendar, as represented by Dodona and Ambrakia, is the most likely candidate for the calendar on the Metonic Spiral of the Antikythera Mechanism.

The Antikythera Mechanism, Rhodes, and Epeiros

In the foregoing discussion I have shown that the missing games in year 4 on the Games Dial were the relatively minor Halieia of Rhodes, and that the calendar on the Metonic Spiral cannot be the calendar of Syracuse, but that it is consistent with the known evidence concerning the calendars of Ambrakia and Dodona of Epeiros, both associated with the very minor Naan Games, which also appear on the Games Dial. It would appear, therefore, that the Mechanism had some connection with both Rhodes and Epeiros. How to explain this?

It is clear that the Antikythera Mechanism is the product of an astronomical and philosophical tradition that developed or was wedded to a tradition of manufacturing such devices. Rhodes was one of the few places where similar devices are attested as having been manufactured (and most of the earliest evidence that concerns itself with this kind of astronomical device is connected with Rhodes in the first century BC, right around the time of the shipwreck, which is not likely to be mere coincidence). I would thus agree with those who have argued that the Mechanism was manufactured on Rhodes (and the reading of the Halieia is the first evidence found on the Mechanism itself supporting the association),[54] but I would add the qualification that it was probably made for a client or recipient who came from Epeiros. Whether this owner or someone to whom it came into possession was on the boat when it sank, or the device was being transported for an owner in Epeiros as a stop-off point on the probable journey to Rome is impossible to know.

I would further suggest that the Ur-Mechanism may have been designed for the Rhodian calendar, and the Metonic Spiral was merely adjusted by having the names of the Epirote months substituted for the Rhodian. The odd rotation of the Games Dial *ca.* 8° counter clockwise, or roughly one lunar month, may have also been made as an elegant adjustment for the one month difference in starting points of the Rhodian and Epirote calendars.[55]

52 *SEG* 35.665/1845, Block B, l. 23.

53 Apart from the ubiquitous attestations of Ἀρτεμίσιος in the Macedonian calendar, the spelling Ἀρτεμίσιος is only attested at Dorian Thera on an inscription that otherwise employs some Doric forms (*IG* XII,3 436, date unknown). It is also found on an inscription from Astypalaia (*IG* XII,3 172, ll. 101-102), but this inscription dates to the Roman period and employs the koine.

54 Price 1974, pp. 13 and 56; Iversen 2017, p. 159; Jones 2017, pp. 93-94; Jones 2020, section 7. Although in the first century BC Rhodes clearly attracted a circle of people interested in modeling the heavens in such a way, I would hesitate to ascribe it directly to the workshop of Poseidonios, who is known to have made a similar device on Rhodes around the time of the shipwreck (Cicero *De natura deorum* 2.88, published in 45 BC, but set in the 70s BC).

55 For the seasons of the months of the Rhodian calendar, particularly that the first month of the Rhodian bouleutic year, Karneios, usually began with the first month after the autumn equinox, see forthcoming article by me in *Eulieme: Studies in Classical Archaeology, Epigraphy, Numismatics and Papyrology* scheduled to come out in the fall of 2020. Also see Jones 2020, section 7.

Apart from the direct tie to Rhodes via the reading of the Halieia, further evidence that the Mechanism may be of Rhodian origin comes from the back plate inscription, which refers to the planets as Ἀφροδίτη Φώσφορος, Ἄρης Πυρόεις, and probably Ζεὺς Φαέθων.[56] While these were common names for the planets (usually one or the other names were used, not both), still it is remarkable that the closest epigraphical parallel occurs on the famous "Keskinto" astronomical inscription from Rhodes.[57] This inscription is also noteworthy, because although found on Rhodes, it employs Attic-Ionic forms (the only inscription in Attic-Ionic on Rhodes that I can find), as do the writings of other astronomers who worked on Rhodes. Thus the fact that Attic-Ionic forms are found on the Mechanism is no barrier to arguing it was manufactured on Rhodes, where astronomers used the universally recognized Attic-Ionic dialect of science. In addition, the Rhodians had been allies of Persia until they went over to Alexander the Great during or just after the siege of Tyre in the spring and summer of 332 BC.[58] As such, doubtless there were those on the island who would have known Persian, and given its closer proximity to the east possibly could have come into contact with the Babylonian astronomical records that fell under Persian and then Makedonian control, which would have been extremely helpful for constructing the Saros Dial. In addition, the palaeography of the inscriptions, while on a unique medium, nevertheless is consistent with lettering found on inscriptions from Rhodes dating to the end of the third to the middle of the first centuries BC.

Since the palaeography of the inscriptions has been cited so authoritatively to defend this or that date, it is particularly important to address this issue carefully. Kritzas (as reported in T. Freeth, Y. Bitsakis *et al.* "Decoding the Ancient Greek Astronomical Calculator Known as the Antikythera Mechanism," *Nature* 44, November 2006, Supplementary Information, p. 7) believes "the style of writing could date the inscriptions to the second half of the 2nd Century BC and the beginning of the 1st Century BC, *with an uncertainty of about one generation (50 years)*" [italics mine].

This statement has been interpreted by some to mean the writing dates definitively to *ca.* 150-100 BC (perhaps to tie it to Hipparchus), but Kritzas's analysis actually suggests a window of *ca.* 200-50 BC, with which I would largely concur (and just to be clear, I would add that my *circa* includes an uncertainty around the edges, so it could conceivably be even a little earlier than 200 BC and a little later than 50 BC).

The letters Kritzas used to come to this conclusion are Π (with shorter right leg), Σ (with the two horizontal hastae not parallel), M (with the two side strokes not vertical, but at an angle), Υ (with a short vertical line), Z for zeta rather than Z, Ω for omega rather than Ω, B with uneven circles (the upper smaller than the lower), a very small O (omicron), Θ (theta with short line in the middle rather than a dot), Φ (with arc-like shape), and four-bar xi (Ξ).

Crowther (in Freeth 2014, Note S2) also accepts these letters for his stylistic criteria but argues instead that "It seems better, accordingly, to widen the palaeographical dating range for the Antikythera inscriptions to the end of the third to the beginning of the first century BC, *with a preference for the earlier half of this period*" [my italics], or in other words Crowther favors a

56 Freeth and Jones 2012.

57 *IG* XII,1 913 = Jones, 2006.

58 Arrian 2.24.5 says the siege of Tyre ended in Athenian Hekatombaion after seven months (Curtius is the only source that claims it took only six months). Arrian (2.220.3) also says that the Rhodians sent 9 triremes during the siege, probably around May of 332. Justin/Trogus (11.10) and Curtius (4.5.9), on the other hand, say Rhodes came over to Alexander immediately after Tyre was taken, and thus after July/August of 332. Curtius' wording is especially pointed: *Sed Rhodii urbem suam portusque debebant Alexandro.* The imperfect tense seems to be inceptive.

date *ca*. 210-150 BC rather than *ca*. 150-100 BC and apparently eliminates *ca*. 75-50 BC, the time of the shipwreck. Again, this critical move is undoubtedly made to cover the apparent start-up date of the Saros Eclipse Prediction Dial in 205 BC (and perhaps also to move it closer in time to Archimedes).

Moreover, Crowther goes on to single out parallels found on stone at Corinth to bolster his preference for an earlier date,[59] saying their lettering "is reminiscent of lettering on bronze, which seems to have been the regular medium for texts of this kind at Corinth." The irony here is that all the examples that Crowther cites as "Corinthian inscriptions" were certainly not, or almost certainly not, inscribed at Corinth, they were only found there. Crowther's most important example (Robertson 1976), for instance, employs the very peculiar and easily recognizable Elean dialect and was certainly inscribed at Elis, while the other examples cited employ the non-Doric koine and are probably dikastic decrees of foreign origin. Furthermore, no bronze examples of decrees or any other kind of document on bronze have been found at Corinth, nor do we have any literary references to a bronze-inscribing tradition at Corinth (making bronze yes, inscribing on bronze, no), making such claims very speculative.[60]

It is also worth testing these dating criteria with actual examples. In what follows I will compare what is claimed about the dating of these letter forms with Rhodian inscriptions that are securely dated to the first half of the first century BC, both because the chronology of Rhodian inscriptions of this era is quite secure, and to provide an example of how shaky these stylistic criteria are (and of course also to show that palaeographically that the Mechanism could also have been inscribed at Rhodes, the home of the Halieia, in the first half of the first century BC around the time of the shipwreck).

For instance, one can compare *IG* XII,1 730 (= *SER* p. 259, no. 5b), which I would argue covers the priests of Apollo Erethimios for the years 89/8 to 62/1 BC (certainly based on quite strong prosopograhical evidence it dates somewhere in the first half of the first century BC).[61] This inscription has pi with a shorter right leg (claimed to belong to the second half of the 2nd century BC), some sigmas without parallel horizontals (claimed to belong to the second half of second century BC, beginning of first),[62] short vertical for upsilon (claimed to belong to the second half of the second century BC), omega as Ω and not ω (claimed to belong to the second century BC), beta with smaller upper circle (claimed to be "old"), an arc-like phi (claimed to be "old"), and even a four-bar xi (Ξ) one time in line 9 (also claimed to be "old").

On another inscription (*ILindos* 2.334, which is dated securely on prosopographical grounds to around 50 BC), there is mu with side hastae that are not completely vertical (claimed to the second century BC), theta with short line (claimed to belong to the second century BC), a rela-

59 Crowther cites and gives a photo of a squeeze of Robertson 1976; he also cites *Corinth* VIII.3 46a-b (and here I will follow Robertson 1976, p. 257, n. 5 in arguing that fragment 46a while similar, belongs to another inscription); and the fragment I-77-13, which joins to *Corinth* VIII.3 46b and is to be published by me.

60 Crowther seems to be alluding to the hypothesis that Corinth's paucity of inscribed material on stone can be explained by positing that it had a rich tradition of inscribing on bronze presumably all lost to recycling. On why this thesis is unlikely to explain Corinth's dearth of inscribed material, see Dow 1942, pp. 113-119, especially p. 116 and a forthcoming article by me in *Hesperia* (probably in the first issue of 2021) on some unpublished inscriptions found on Corinth's Temple Hill.

61 For a good facsimile of this inscription, see Hiller von Gaertringen 1894, p. 18. For my new date of this inscription, see my forthcoming article in *Eulieme: Studies in Classical Archaeology, Epigraphy, Numismatics and Papyrology* scheduled to come out in the fall of 2020.

62 A better example of sigma can be found at *ILindos* 295, dated securely to, or shortly after 85/4 BC, when Damatrios (son of Aristogenes) was priest of Athena Lindia.

tively smaller omicron (claimed to be "old"), as well as pi with shorter right leg, upsilon with a short vertical hasta, the Ω shape, and probably also four-bar xi (but the stone is broken at the top and a bit to the left of the letter in col. I, line 1 to make the reading somewhat uncertain, although context-wise it is virtually certain it must be a xi, and the visible remnants strongly favor the four-bar xi).

It should be noted here that Crowther's (Freeth 2014, Note S2) preference for the earlier dating relies heavily upon the form Z for zeta. This form is found on Rhodes at *ILindos* 2.309 and 2.311, both securely dated to, or shortly after, Zenodotos' priesthood of Athena Lindia in 64/3 BC. Also on these two inscriptions the arc-like phi, upsilon with a short vertical, mu with two lines at an angle, and Ω-shaped omega also appear.

In short, all the letter forms that are claimed by both Kritzas and Crowther as belonging to the end of the third century to the beginning of the first are found on Rhodian inscriptions not only in those years, but also as late as *ca.* 50 BC. I would go even further and say that in my opinion, the only letter form that clearly suggests a preference for the earlier dating in Rhodian epigraphy is four-bar xi (Ξ), but this is thus far only attested on the numerals (which tend to be more conservative) on the Front Cover Inscription of the Mechanism. Even so, as we saw above this older xi is attested on stone at Rhodes as late as ca. 89/8 - 63/2 BC at Hiller 1894, line 9, and probably also as late as about 50 BC at *ILindos* 2.334, col. I, line 1.

This older xi on the Mechanism, however, is perhaps offset by the cursive omega now identified on the Back Plate Inscription[63] and the glyph-monogram for ὥρ(α), ⳨, that is composed of a ω-shaped omega crossed by a rho. This digraph, which is ignored by Kritzas and Crowther, is first found on papyri of the first century of our era or later. Furthermore, ω-shaped omega does not appear regularly on Greek inscriptions until the late first century BC and later,[64] although it becomes the regular form on papyri by the third century BC.[65] Another letter shape only suggested by Gregg Schwendner after 2014 and confirmed by Iversen and Jones in 2019 (p. 487) is the hooked-alpha, ⲁ, for which there are examples that appear on papyri from the late 3rd century to the middle of the first century BC.

Finally, there is the form Ϛ for stigma. As Jannaris (1907) notes, the symbol Ϛ for stigma probably originated in Alexandria in the first half of the third century BC where it is found on papyri. From here it spread to other Greek centers, where the evidence suggests it was first adopted in the first century BC.[66] The earliest inscribed example I have been able to find is found on an odd inscription found during construction of a new road between Yatağan and Milas on the south side of Stratonikeia (Karia) in front of the large nymphaeum there.[67] This inscription

63 See Iversen and Jones 2019, pp. 486-488.

64 A few rare earlier examples can be found on stone. For instance, Lougovaya 2015, 113 reports that *SEG* LIX 1767B (= Lougovaya 2015, 108, B), which comes from Ptolemaic Narmouthis (Egypt) and dates sometime from 117 to 115 BC, has a cursive omega in line 3, as well as cursive forms for pi and mu more typically found on papyri, mixed in with non-cursive forms. *SEG* LIV 1568, which is from Alexandria Arachosia and dates to the late second-century BC, also reports a cursive omega. Examples of cursive omegas on stamped bricks that date to the 20th and 33rd year of the reign of Attalos I (222/1 and 209/8 BC) can be found at Pergamon (*I. Pergamon* II, nos. 689-691).

65 For a useful chart of the development of letter shapes on papyri, see Kenyon 1899, p. 161.

66 Some inscribed examples include *CIG* 2655 = *RIG* 877 = *Syll.*^{3} 1020, line 29 (from Halikarnassos, dating to the first century BC); *IG* X.2,1 97, line 1 (from Thessalonike, erroneously corrected to 〈E〉 by Edson, dating 23/2 BC); *CIG* 1970 = *IG* X.2,1 526 (from Thessalonike, dating to AD 154/5 with Edson's misreading as zeta corrected by Daux, 1973, p. 593, no. 526).

67 See Şahin 2005, col. II,C = *IK Stratonikeia* 1508 = van Breman 2001 = *SEG* LXII 852.

lists those who have paid money that appears to have given them access to something "day and night" (τοῖς ἔχουσι τὰ *nomen patronym ethnicum* δεδωκότος (δρ.) -΄· ἡμέρα καὶ νύξ). It is noteworthy that many of these contributors came from Rhodes, which controlled the area of Stratonikeia at some point in the third century BC before they lost it to Makedonian control, after which they regained it after the Battle of Kynos Kephalai in 197 and held it until the Battle of Pydna in 168 BC, which concluded the Third Makedonian War, after which the Stratonikeians along with all of Karia were declared autonomous by the Roman Senate (but apparently the Rhodians continued to maintain a significant presence there).[68] R. van Breman dates it ca. 81 BC based on lettering that is very similar to *IK Stratonikeia* 505 (= *SEG* LII 1059), a Roman *Senatus Consultum* dating to 81 BC found at Lagina, which was under Stratonikeian control. She also suggests the block was built into the nymphaeum and that those who contributed to its construction had access rights to the water "day and night." At the end of this inscription (col. II,C), it appears more limited access rights on certain dates were given for those who lived outside the gates and did not contribute:

> τοῖς ἔξω πυλῶν Ἀρτεμεισι-
> ῶνος *vac.* η′ *vac.* ϛι′ *vac.* ζκ′
> Ἑκατησιῶνος β′ *vac.* ι′ *vac.* ηι′ εκ′
> Διοσθεῶνος ε′ *vac.* γι′ *vac.* ακ′ προτ(ριακάδι)[69]

As van Breman notes, to all appearances, this calendar seems to have employed the backward count of days in the last decade of the month between the 21st and the τριακάς; thus the days are Ἀρτεμεισιών 8, 16, 24, Ἑκατησιών 2, 10, 18, 26, and Διοσθεών 5, 13, 21, 29. Since all these dates are 8 days apart (assuming Ἀρτεμεισιών was a full month of 30 days and Ἑκατησιών was a hollow month of 29 days), it seems most likely that these months were consecutive, and possibly tied to water rights during the prime growing season in the spring. In any case, the separation of 8 days guarantees that ⊏I = ϛι´ = 16, not EI = ει′ = 15 as previous editors have supposed, a letter form found near in time to the Antikythera Shipwreck ca. 70-60 BC in a context that involved Rhodians.

Thus the Mechanism displays several letter forms found on stone and papyri that could date anytime from the late 3rd century to the middle of the first century BC, although the stigma is a special symbol thus far found outside of Alexandria on inscriptions dating from the first century BC (with one early example around 81 BC within the historic Rhodian Peraia), while the glyph-monogram for ὥρ(α) has thus far been found only on papyri beginning in the first century AD.

I have engaged in this exercise not only to demonstrate that the Mechanism could have been inscribed on Rhodes in the first half of the first century BC around the time of the shipwreck, but more importantly to show that pronouncements about dating by letter-style must be used with extreme caution and skepticism. The reality is that one can cherry-pick examples from just about anywhere in the Greek world to bolster an argument that relies upon letter style. In the end, it is extremely common for inscriptions to have mixed letter-forms in all periods (often even the same letter is inscribed in two different ways such as xi on Hiller 1894). In fact, many are the instances of dating of inscriptions by letter style (as opposed to individual inscribers'

68 See Livy 33.18; 33.30.10.

69 van Breman (2011) with help from P. Thonemann correctly identified the monogram at the end of the line as standing for προτριακάς (she read ΠΤΡ for π(ρο)τρ(ιακάδι)), but the photo in Şahin 2005 indicates clearly ΠΡ are in ligature with the top horizontal of Π extending far to the right with an O at its tip and a T underneath, thus προτ(ριακάδι).

hands such as the work Stephen V. Tracy does) that later were shown to be wildly off by a century or more,[70] and unless further securely dated examples of such tiny writing on bronze (where the inscriber used a burin and not a chisel) can be found, which is surely responsible for the necessity of having flaring-shaped letters that are more in keeping with smaller letters typically found before 150 BC, the most that can reliably be said is that the writing dates from the end of the third to the middle of the first century BC, and that it could have been inscribed just about anywhere in the Greek world.

The palaeography, therefore, is not a very helpful argument and must be combined with other evidence. To my mind, the appearance of the Halieia (with the spelling Ἁλίεια which is found almost exclusively on Rhodes and the Rhodian Peraia), combined with Rhodian material being on the ship, combined with the date of the shipwreck as being *ca.* 60 BC, combined with the likelihood that such a mechanism had a limited working life of about 30 years (according to Michael Wright), combined with the closest parallel interest in the astronomical theories modeled by the Mechanism being attested at Rhodes in the first century BC (i.e., Geminus, Book 8), combined with the earliest and closest literary parallel for such a device being built at Rhodes in the first half of the first century BC right around the time of the shipwreck (i.e., Poseidonios' *spheara*),[71] combined with the known tradition of the astronomers at Rhodes writing in the Attic-Ionic dialect (Hipparchus and Geminus),[72] combined with the closest parallel to the planet names on the Mechanism being found on an astronomical inscription from Rhodes that employs the Attic-Ionic dialect, combined with the evidence that the palaeography writing could come from the first half of the first century BC on Rhodes – all these in my mind outweigh any other current known evidence (such as the apparent start-up date for the Saros Dial in 205 BC that was evidently chosen because it was the best example of when the sun and moon were both close to their apogee at a full moon) to make the first half of the first century BC on Rhodes the most likely, if not the most attractive, candidate.

Finally, it has recently been argued, including by James Evans, the *honorandus* of this tome,[73] that the astronomical events on the Parapegma of the Antikythera Mechanism work best for latitudes in the range 33.3°N – 37.0°N. This range is too low for Epeiros, but does work for Rhodes, the northern tip of which sits at about 36.4°N.

Abbreviations

Agora XVIII = D. J. Geagan, *The Athenian Agora: Results of Excavations Conducted by the American School of American Studies at Athens*, vol. XVIII, *Inscriptions: The Dedicatory Monuments*, Princeton 2011.

BE = *Bulletin épigraphique* (Paris 1888-).

CIG = A. Boeckh and J. Franz, eds. *Corpus Inscriptionum Graecarum*, 4 vols. (1828-1877).

70 See for instance Iversen 2010. Based on the letter-forms, it was assumed this inscription (Corinth I 2649) dated before the destruction of Corinth in 146 BC (see Anderson 1967, p. 11), but the inscription was almost certainly inscribed at Athens and dates AD 165/6 to 168/9, or more than 300 years later than was first believed based on letter forms.

71 Cicero *De natura deorum* 2.88, published in 45 BC, but set in the 70s BC. The references to Archimedes' sphere (including in this passage) actually first appear in texts of the first century BC and look suspiciously anachronistic.

72 Note that Archimedes was famous for using his beloved Doric dialect.

73 M. Anastasiou, J. Seiradakis, J. Evans, S. Drogou and K. Efstathiou 2013.

CIGIME 1.2.2A = P. Cabanes and N. Ceka, eds., *Corpus des inscriptions grecques d'Illyrie méridionale et d'Épire, Inscriptions d'Apollonia d'Illyrie*, Athens 1997.
CIGIME 2.2 = P. Cabanes, and F. Drini, eds., *Corpus des inscriptions grecques d'Illyrie méridionale et d'Épire*, *Inscriptions de Bouthrôtos*, Athens 2007.
Clara Rhodos 2 = G. Jacopi, "Nuove epigrafi dalle Sporadi meridionali," *Clara Rhodos* 2 (Rhodes 1932), pp. 165-256.
Corinth VIII,3 J.H. Kent, ed., *The Inscriptions, 1926-1950*, Princeton, 1966.
IDelos = Inscriptions de Délos, Paris, 1926-1972.
IG = Inscriptiones Graecae. Berlin.
II^2 J. Kirchner, ed., *Inscriptiones Atticae Euclidis anno posteriores*, 2nd ed., Part II, 1-2, 1927-1931.
IV M. Fraenkel, ed., *Inscriptiones graecae Aeginae, Pityonesi, Cecryphaliae, Argolidis*. *Corpus Inscriptionum graecarum Peloponnesi et insularum vicinarum* 1, 1902.
V,2 F. Hiller von Gaertringen, ed., *Inscriptiones Arcadiae*, 1913.
IX,1^2 1 G. Klaffenbach, ed., *Inscriptiones Aetoliae*, 1932.
IX,1^2 4 K. Hallof, ed., *Inscriptiones insularum maris Ionii*, 2001.
X.2,1 = Ch. Edson, ed. *Inscriptiones Thessalonicae et viciniae*, 1972.
XII,1 F. Hiller von Gaertringen, ed., *Inscriptiones insularum maris Aegaei praeter Delum, fasc. 1. Inscriptiones Rhodi, Chalces, Carpathi cum Saro, Casi*, 1895.
XII,3 F. Hiller von Gaertringen, ed., *Inscriptiones insularum maris Aegaei praeter Delum, 3. Inscriptiones Symes, Teutlussae, Teli, Nisyri, Astypalaeae, Anaphes, Therae et Therasiae, Pholegandri, Meli, Cimoli*, 1898.
XIV G. Kaibel, ed., *Inscriptiones Siciliae et Italiae, additis Galliae, Hispaniae, Britanniae, Germaniae inscriptionibus*, 1890.
IK = Inschriften griechischer Städte aus Kleinasien. Bonn.
Iasos W. Blümel, ed., *Die Inschriften von Iasos*, 2 vols., (1985).
RhodPer W. Blümel, ed., *Die Inschriften der rhodischen Peraia* (1991).
Stratonikeia = M. C. Şahin, ed. *Die Inschriften von Stratonikeia*, 4 vols. (1981-2010).
ILindos = Ch. Blinkenberg, *Lindos. Fouilles et recherches, 1902-1914*, vol. II, *Inscriptions,* 2 tomes (Copenhagen and Berlin 1941).
IMagnesia = O. Kern, ed. *Die Inschriften von Magnesia am Maeander*, Berlin, 1900.
I. Pergamon II = M. Fränkel, E. Fabricius, C. Schuchhardt, *Die Inschriften von Pergamon*, vol.. II, *Römische Zeit - Inschriften aus Thon*, Berlin, 1895.
L'Épire = P. Cabanes, *L'Épire de la mort de Pyrrhos à la conquête romaine (272-167 av. J.C.).* Centre de Recherches d'Histoire Ancienne 19. Paris, 1976.
NSER = G. Pugliese Carratelli, "Nuovo supplemento epigrafico rodio," *ASAtene* 33-34 (1955-1956), pp. 157-181.
NSERC = A. Maiuri, *Nuova silloge epigraphica di Rodi e Cos* (Firenze 1925).
RIG = Ch. Michel, ed. *Recueil d'inscriptions grecques* (1900).
SEG = Supplementum Epigraphicum Graecum (Leiden 1923-).
SER = G. Pugliese Carratelli, "Supplemento epigrafico rodio," *ASAtene* 14-16 (1952-1954), pp. 247-316.
Syll.^{3} = W. Dittenberger, ed. *Sylloge Inscriptionum Graecarum*, third edition, 3 vols. (1915-1920).
Tit. Cam. = M. Segre and G. Pugliese Carratelli, "Tituli Camirenses," *ASAtene* 27-29, N.S. 11-13, (1949-1951), pp. 141-318.

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Is there a connection between the 1897 Cretan Revolt and the discovery of the Antikythera Shipwreck?

J. H. Seiradakis

Introduction

The Antikythera Mechanism was found by chance, in a shipwreck, close to the small Greek island of Antikythera, in April 1900, by sponge divers. The shipwreck was dated between 86 and 67 BC (based on coins from Pergamon). Later the Mechanism was stylistically dated, around the second half of the 2nd century BC (100 – 150 BC). About this time the great Greek astronomer Hipparchos (190 – 120 BC) lived in Rhodes.

It was a portable (laptop-size), geared mechanism which calculated and displayed, with high precision, the movement of the Sun and the Moon in the sky and the phase of the Moon for a given epoch. It could also calculate the dates of the four-year cycle of the Olympic Games. It had one dial on the front and two on the back. Its 30 precisely cut gears were driven by a manifold, with which the user could select, with the help of a pointer, any particular epoch (on the front dial). While this was done, several pointers were synchronously driven by the gears, to show the above mentioned celestial phenomena on several accurately marked annuli. It contained an extensive user's manual. The exact function of the gears has finally been decoded, and a large portion of the manual has been read after 2000 years by a major new investigation, using state of the art equipment.

Based on new surface photography and high resolution tomography data, a new model has been built at the Aristotle University, revealing the technological abilities of ancient Greeks.

No complicated geared instruments are known before the Antikythera Mechanism and for several centuries after. Therefore, this astronomical device stands out as an extraordinary proof of high tech in ancient times.

The Cretan revolt of 1897

The island of Crete was part of the Byzantine Empire until 1204 AD. In 1205 AD the island became an overseas colony of the Republic of Venice until its fall to the Ottoman Empire during the Cretan War (1645–1669). Since then there were significant rebellions against the Ottoman rule, particularly in Western Crete. One of the most famous revolts took place in 1770 AD, encouraged by the Russian Naval Forces. Toward the end of the 19th century, in particular after the liberation of the main land of Greece in 1821, there were continuous rebellions leading, eventually to an autonomous Cretan State (1898 – 1913).

In 1896 AD, while Crete was still under the Ottoman rule, tensions were aggravated, leading to a rebellion that soon covered most of the island[1]. Volunteers from the main land (but also

1 There are several books about the 1897 Cretan rebellion, see for example *Τα Σελινιώτικα Νέα* (*Ta Seliniotika Nea*), 27 February 2014 (in Greek).

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 39–43. Chapter DOI: 10.5281/zenodo.3975717.

Figure 1. The optical telegraph.

from several European countries) came to Crete and fought for the liberation of the island). In February 1897 the Greek government sent military forces to the island, under the royal adjutant Timoleon Bassos, provoking a war with the Ottoman Empire. Immediately the Ottoman administration, with the consent of the Great Powers (Italy, France, Austria-Hungary, Germany, Great Britain and Russia), blockaded Crete and restricted all communications between the island and Greece. The Greek Ministry of War reacted by establishing an *optical telegraph* between Cape Grambousa in NW Crete, Antikythera and the Port of Kapsali at Kythera, which was Greek territory. From there communications were transmitted to Athens by wire.

The message transmitted by optical telegraph

The optical telegraph (often called *heliograph* – Figure 1) was a simple but effective means of communication over long distances during the late 19th and early 20th century. Communication was achieved by transmitting optical flashes (usually Morse code), reflecting sunlight by mirrors. The flashes were produced either by tilting the mirror by a few degrees up or down, or by using a shatter. The optical telegraph used at Grambousa, Antikythera and Kythera was probably of the Mance Mark V type or a variant of it.

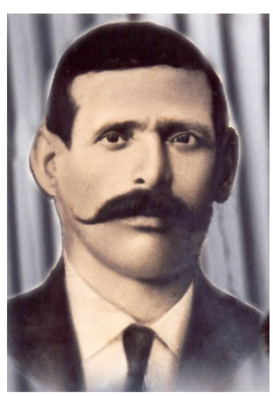

Figure 2. Elias Lykopantis or Stadiatis, who discovered the Antikythera shipwreck.

Figure 3. A suit used around 1900 by Simiot sponge divers (photographed by the author in 2008 at the Museum of Naval Art at Symi).

The war between Greece and the Ottoman Empire ended soon by the intervention of the Great Powers. By 1900, the Greek army was withdrawn from Crete and the heliograph operators left the islands. However, the operator of the Antikythera heliograph, escaping the military and financial bureaucracy of Greece, stayed at Potamos, the little port of Antikythera, enjoying the cheap and simple life of the island and his monthly salary!

The Antikythera shipwreck was accidentally discovered on Tuesday, 4 April 1900, five days before Easter by the Symiote sponge diver Elias Lykopantis (Figure 2),[2] while he was diving in a full diver's suit (Figure 3). Greece was still using the Julian calendar (until February 15, 1922), therefore this date would correspond to 17 April 1900, according to our modern Gregorian Calendar. The news spread among the inhabitants of the small island of Antikythera. However, the divers, having been hired and paid to collect sponges, had to continue their trip to their destination (the shores of North Africa), returning to Symi several months later. According to information provided by some old divers at the island of Symi in August 2007, the sponge expeditions usually started around April (when the weather improved) and ended several months later)[3].

Ten days later, on Friday after Easter (April 14, 1900), the residents at the Port of Kapsali (at Kythera) noted, much to their surprise that the Antikythera telegrapher was persistently requesting urgent communication. The communication was established and lasted about one hour, during which a message was relayed that a treasure shipwreck was discovered at Antikythera, insisting for immediate broadcasting to Athens[4]. In the Ministry of Education (responsible for cultural issues at the time) the message brought some justified activity, but after investigating the records of the Antikythera telegrapher it was rejected as the "*product of heavy alcohol use*"!

2 See Svoronos I.N. (1903, in Greek), *Ὁ Θησαυρὸς τῶν Ἀντικυθήρων*, Beck & Barth, Athens or Svoronos I.N. (1908, in German), *Die Funde von Antikythera*, Beck & Barth, Athens.

3 Price, D. de S. (1974), *Gears from the Greeks: The Antikythera Mechanism – A calendar computer from ca. 80 BC*, American Philosophical Society, Transactions, N.S. 64.7, Philadelphia, mentions that the divers sailed immediately back to Symi, which contradict the information provided by the old Symiote divers.

4 Most of what follows is mentioned in an article at the greek journal *Ἥλιος* (*Helios*) v. 345, p. 563 (1957), written by Stylianos Lykoudis, son of Emmanuel Lykoudis, who was the government observer during the marine excavations that took place between November 1900 and September 1901.

Δέκα μέρες πρίν, τὴν Μεγάλη Τρίτη τοῦ 1900, δυὸ σφουγγαράδικα μηχανοκάϊκα Συμιακά, ποὺ ἀνέβαιναν ἀπὸ τὴν Ἀφρική, τ' ἀνάγκασε φρέσκος ὀστριαγάρμπης (Ν Δ), νὰ καταφύγουν στὸν ὅρμο τοῦ Ποταμοῦ στ' Αντικύθηρα. Τὸ ἕνα ἀπὸ τὰ δύο ξεμύτησε ὥς στὸν δίπλα κάβο Γλυφάδια, καὶ σὲ 25 μέτρα μόνο ἀπὸ τὴν ἀκτή, κατέβασε μὲ σκάφανδρο δύτη, σὲ 35 ὀργυιὲς βάθος στὴ θέσι Πινακάκια, νὰ βγάλη θαλασσινὰ γιὰ τὴν νηστεία τῶν ἡμερῶν. Μὰ σὲ λίγο ἔκανε σινιάλο νὰ τὸν τραβήξουν ἀπάνω. Ἀντὶ γιὰ πίννες καὶ «γαϊδουρόποδα» ἀνέβασε ἕνα ὡραῖο χάλκινο χέρι, ποὺ ἀπέσπασε ἀπὸ ἀρχαῖο ἄγαλμα ποὺ βρίσκονταν μαζὶ μὲ ἄλλες ἀρχαιότητε- στὸν βυθὸ καὶ ἔπιαναν μάκρος 55 τοὐλάχιστον μέτρα. Οἱ δύο καπετάνιοι τῶν καϊκιῶν. Δημήτριος Ἐλευθερίου ἢ Κοντὸς καὶ Ἡλίας Σταδιώτης, ἐβούτηξαν οἱ ἴδιοι νὰ βεβαιωθοῦν· καὶ ἐπείσθηκαν, καὶ ἀνέβηκαν στὰ καΐκια τους κι' ἔκαναν ἀμέσως πανιὰ γιὰ τὴν Σύμη, μ' ὅλο τὸν φουρτουνιασμένο καιρό.

Ten days ago, on Good Tuesday before Easter of 1900, two sponge trawlers from Symi, coming from Africa, were forced by strong northeastern winds to resort to the bay of *Potamos* of Antikythera. One of the two boats got out of the bay as far as the next cape Glyfadia and 25 meters from the coast, let one of the divers, with a diving suit to descend at a depth of 35 fathoms in order to collect some seafood, useful for the Easter fasting days. Soon after, the diver gave signal to be drawn to the surface. Instead of muscles and clams he had brought up a nice bronze arm that he had extracted from an ancient statue he had found together with other antiquities at the bottom of the sea, covering an area of at least 55 feet in length. Both captains of the boats, Demetrios Eleftheriou or Kontos and Elias Stadiotis, dived themselves to make sure and be personally convinced of the finding and went back to their boats and immediately set sail for Symi, despite the stormy weather.

Figure 4. The Greek text and English translation of the 1957 "Helios" article, summarizing the contents of the message transmitted by the telegraph.

The summary of the contents of the telegraph is reproduced in Figure 4.

There are some contradictions in the above communication.[5] However it seems to be a vivid description of the discovery of the Antikythera shipwreck. The author of the 1957 article, Sylianos Lykoudis (1878 - 1958) was considered to be a trustworthy and reliable person. In 1939 was elected as a full member in the Academy of Athens.

The bronze arm that was brought up as evidence of the discovery of the shipwreck was the arm of the Philosopher of Antikythera, as was identified much later at the National Archaeological Museum of Athens.

Summary

The Antikythera shipwreck was discovered on Tuesday before Easter, i.e. on April 4, 1900 according to the then used Julian calendar. The first announcement of the discovery was transmitted ten days later, on April 14, 1900, by optical telegraph from Antikythera to Kythera and from there it was wired to Athens. The Ministry of Education in Athens rejected the announcement as unreliable. The optical telegraph had been used by the greek army, during the 1897 Cretan revolt and was inadvertently left on the island of Antikythera.

5 Small discrepancies which need further investigation: (a) As mentioned earlier it was too early for the boats to be on their way back from Africa. (b) The diver that first saw the Antikythera wreck and extracted and brought up the bronze arm was Elias Lykopantis with the nickname Stadiatis (not Stadiotis). This was confirmed by his niece when Xenophon Moussas and I met her in Rhodes in 2006. She also told us that after her uncle discovered the bronze arm he never allowed anybody else to touch it and that he slept with it in his bunk until they returned home. (c) The diver was wearing a diver's suit. It would have been extremely inconvenient to be searching for seafood in a suit. He was probably testing a new suit or showing its use to a novice. (d) Elias was the most experienced diver, but there is no evidence that he was actually the captain of the second boat. (e) As mentioned earlier the two boats did not set sail for Symi immediately after the discovery. They sailed to the coast of Africa first to collect sponges. This make sense, otherwise there would be a marked delay of seven months between the return of the boats to Symi (toward the end of April) and the announcement of the discovery to the (Kytherian) Minister of Education, Mr. Spyridon Stais on November 6, 1900, according to newspaper reports.

Acknowledgements

The author would like to thank several anonymous residents of the island of Symi that participated in a workshop about the discovery of the Antikythera shipwreck organized by the *Antikythera Mechanism Research Project*, the *Municipality of Symi* and the journal *Efoplistis* on August 31, 2008 for relating to us personal memories of members of their families. He would also like to thank the postgraduate student Magdalini Nikoli, for her thorough investigation of early newspaper (1900–1901), during her undergraduate Diploma Thesis.

Three examples of ancient “universal” portable sundials

Denis Savoie

Introduction

Ancient portable sundials (see Table 1) represent only 5% to 6% of the corpus of all ancient sundials, [1] which is at present estimated to be between 500 and 600 dials. Nevertheless they must have been in rather frequent use if one is to believe Vitruvius when he writes in his *De Architectura* (between 35 and 25 BC):[2] “Again, following these types, many authors have left notices for the construction of travelers’ sundials or portable sundials. If one wishes, one can find various kinds of projections in their works, so long as one is familiar with the drawings of the analemma.” Unfortunately, none of these notices has reached us. For the gnomonists of antiquity, these *horologia viatoria* lent themselves particularly to the development of original models, and to the pursuit of dials intended to be usable at practically all latitudes.

One can divide ancient portable sundials into two categories:

– those that are only usable for a given latitude. This is the case with the “Portici Ham,”[3] the dials found at Mainz and Ponteilla,[4] the medallion dials,[5] and generally the cylinder dials.[6]

– those that are usable at multiple latitudes or at all latitudes. These include the Berteaucourt and Merida disks, dials of the “Crêt Chatelard” type, and lastly the most perfect of all, the armillary dial.[7]

1 The standard catalogue is now that of J. Bonnin, *La mesure du temps dans l’Antiquité*, Les Belles Lettres, Paris, 2015, accompanied by an online database at https://syrte.obspm.fr/spip/actualites/article/en-ligne-la-base-de-donnees-la-mesure-du-temps-dans-l-antiquite-temoignages/ . The well known work of S. L. Gibbs, *Greek and Roman Sundials*, New Haven, 1976, is more or less silent about portable sundials. For a succinct discussion of ancient portable sundials, see K. Schaldach, *Römische Sonnenuhren*, Verlag Harri Deutsch, Frankfurt, 2001. See also R. J. A. Talbert, *Roman Portable Sundials: The Empire in Your Hand*, Oxford, 2017, which is especially significant for the geographical aspects.

2 Vitruve, *De l’Architecture*, Livre IX, texte établi, traduit et commenté par J. Soubiran, Les Belles Lettres, Paris, 1969, p. 31.

3 Accademia Ercolanese, *Le Pitture Antiche d’Ercolano e conformi incisi con qualche spiegazeione III,* Naples, 1762, p. V-XVII. See also J. Drecker, *Die Theorie der Sonnenuhren*, Berlin, 1925, p. 58-60.

4 K. Körber, *Die neuen römische Funde Inschriften des Mainser Museums*, 1900, n° 202, p. 119-121. See especially J. Drecker, *Die Theorie der Sonnenuhren, op. cit.*, p. 61-64. The Ponteilla fragment will be the subject of a forthcoming article.

5 For the medallion dial in the Musei Civici di Trieste see now R. J. A. Talbert, “A lost sundial found, and the role of the hour in Roman daily life,” *Indo-European Linguistics and Classical Philology* 23 (2), 2019, 971–988.

6 M. Arnaldi, K. Schaldach, “A roman cylinder dial : witness to a forgotten tradition,” *Journal for the History of Astronomy*, 28, 1997, p. 107-117.

7 G. Gounaris, “Anneau astronomique solaire portative antique découvert à Philippes,” *Annali dell’Istituto e Museo di Storia della Scienza di Firenze*, 5 (2), 1980, p. 3-18. This exceptional dial deserves a more extensive study.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 45–77. Chapter DOI: 10.5281/zenodo.3975719.

Fixed latitude	*Medium*	*Number of exemplars*	*Findspot, present location, language, date*
"Portici Ham"	bronze	1	Herculanum, Museo Nazionale di Napoli, Latin, 1st century
Mainz type	bone, stone	1	— Mainz, Landesmuseum Mainz, Latin, 2nd-5th century — Ponteilla, lost,
Cylindrical dial	bone, bronze	3	— Este, Museo Nazionale di Atestino, Latin, 1st century — Amiens, Musée de Picardie (France), Latin, 3rd century — Domjulien (Vosges), Musée d'Art Ancien et Contemporain d'Epinal (France), Latin, Roman imperial period
Medallion dial	bronze	6	— 1 at Rome, Museo Nazionale Romano, Latin, end of 2nd century — 2 at Aquileia (Italy), Kunsthistorisches Museum,Vienna, and Civici Musei di Trieste, Latin, 1st-4th century — 2 in Bithynia, 1 Latin, 1 Greek, AD 130 — 1 at Forbach, Hérapel, Musée de la Cour d'Or, Metz (France), Latin, 1st-4th century
Variable latitude			
Medallion dial	bronze	1	Exemplar of the Vienna Museum
Armillary	bronze	1	Philippi (Greece), Kavala Museum, Greek, 3rd-4th century
Portable meridian or latitude indicator	bronze	2	— Berteaucourt-les-Dames (France), Musée de Picardie, Latin, 2nd-3rd century — Merida (Spain), National Museum of Roman Art of Merida (Estremadura, Spain), Latin, 3rd century
"Crêt-Chatelard" type	bronze, brass, copper alloys	11	— Crêt-Chatelard (France), lost, 1st-4th century — Rome, lost, 1st-4th century — Trier (Germany), Rheinisches Landesmuseum Trier, 1st-4th century — Bratislava, Museum of the History of Science, Oxford, 1st-4th century — Unknown findspot, Science Museum, London, 2nd-6th century — Unknown findspot, ex-Time Museum, Rockford, USA, private collection, 5th century — Unknown findspot, British Museum, London, 4th-6th century — Aphrodisias, Aphrodisias Museum, 4th century — Bulgaria, private collection, replica in Römisch-Germanisches Zentralmuseum, Mainz, end of the 1st century to beginning of the 4th century — Memphis, Hermitage Museum, St. Petersburg, 4th century, lost? — Samos, Vathy Museum, Greece, 4th-6th century

Table 1. Summary of types and exemplars of ancient portable sundials known at present.

However, there exists an exemplar of a medallion dial that belongs simultaneously to the categories of fixed-latitude dials and multiple-latitude dials, as we will see below.

Certain ancient dials experienced a new glory during the Middle Ages and Renaissance, in particular the altitude cylinders, which in 19th century France were called "cadrans de berger." Conversely, other portable dials vanished, and it is a matter of some interest to understand why.

Medallion dials.

The term "mediallion dial" designates small altitude sundials 4-5 cm diameter, usable for a specific latitude, and resembling small pillboxes. The rim is raised, and on the principal face is a relief portrait, generally of an emperor. An eyehole or lateral orifice, situated in the raised rim, allows the Sun's rays to enter and fall upon a small mobile ruler in the shaded interior of the box. The apparatus is used in a vertical position oriented towards the Sun. At the base of the box is a grid in the form of an angular sector, made up of hour curves and date lines. One reads the seasonal hour at the intersection of the date line—on which is the ruler—and the hour curve. There are many variants of this altitude dial: for example in the Forbach dial each face bears the same dial and each ruler is at the center of the dial. In the Rome exemplar, the ruler is off-center, and the rear face of the medallion is decorated with a portrait of the emperor Commodus.

But it is the Vienna exemplar that is most remarkable (Fig. 1).[8] Not only is one face decorated with a portrait of the emperor Antoninus Pius (138-160) with the inscription *ANTONINVS AVG PIVS TR P COS III IMP II* (Fig. 2), but the first "box" component is like the *mater* of an astrolabe, having disks installed in it that are adapted to different latitudes. The base of this first box bears a—rather crude—tracing that unquestionably resembles the hour curves of a stereographic project (Fig. 3). These curves of course have no function in the dial; this is a "learned" ornamentation whose motivation is unknown. The other part of the box is equipped with a little stud for keeping in place the disks that are furnished with a grid applicable to several latitudes. Thus there are disks for the latitudes of Rome, Alexandria, Spain, Greece, etc. The hour grids of the disks are overall rather poor. The seasonal hour curves are represented by sometimes chaotic line segments; as for the point of convergence of the date lines, it often falls to the side of the hole for the mounting. On some of the disks (for example that for Alexandria, Fig. 4), one can still read below the date lines the names of the Roman months. This portable sundial is thus a very clever system in which the traveler places a grid-disk appropriate for the locality where he is in a kind of *mater*, rather as if one was traveling with an astrolabe equipped with multiple plates. There is a striking parallelism between these two instruments, where the astrolabe was the subject of efforts to develop a universal version that became workable in the 11th century with the *saphea* of Azarquiel.[9]

Let us study this dial from a modern viewpoint (Fig. 5). The most general case consists of a stud for fixing the disks and the ruler off-center. Let R be the radius of the box, whose center is O, and let C be the point of convergence of the date lines, and P the orifice through which the

8 This dial has been studied by E. Buchner, "Römische Medaillons als Sonnenuhren," *Chiron*, vol. 6, 1976, p. 329-348. The first mention and drawing of this dial appeared in F. Kenner, "Römische Medaillons," *Jahrbuch der Kunsthistorischen Sammlungen der Allerhöchsten Kaiserhauses*, vol. 1, Vienna, 1883, p. 84-85 ; one can clearly see in the drawing of F. Kenner that the dial bore a ruler as well as a pin, which have both vanished today. See also A. Schlieben, "Römische Reiseuhren," *Annalen des Vereins für Nassauische Alterthumskunde und Geschichtsforschung*, n ° 23, 1891, 115-128 and plate VI.

9 The principle was revived by Gemma Frisius in his *astrolabe catholique*. See R. D'Hollander, *L'Astrolabe*, *Histoire, Théorie, Pratique*, Institut Océanographique, Paris, 1999, p. 235-262.

Figure 1. This "box" sundial preserved at the Kunsthistorisches Museum, Vienna, came from Aquileia. Dating from the 2nd century of our era, it consists of two halves of a small bronze cylindrical box (top) containing four perforated disks (bottom). On the upper half of the box one clearly sees the small metal cylinder that enables a disk to be installed, as well as the notched hole on the side that allows the Sun's rays to pass through. The four disks bear on their recto and verso the horary drawing for the latitudes of the following cities: Alexandria Aegyptus / Africa Mauretania / Hellade Asia / Hispania Achaiia / Roma ? / Ancona Tuscia / Britannia Germania.

Figure 2. Outside face of the box, bearing a relief portrait of Antoninus Pius.

Figure 3. Inside face of the half of the box, bearing a drawing resembling a stereographic projection. The reason for its presence in this dial is unknown.

Figure 4. Disk showing the horary drawing for the latitude of Alexandria. One can clearly see that the hole is displaced relative to the drawing and that the latter is quite crude. Below the lines are the names of the months.

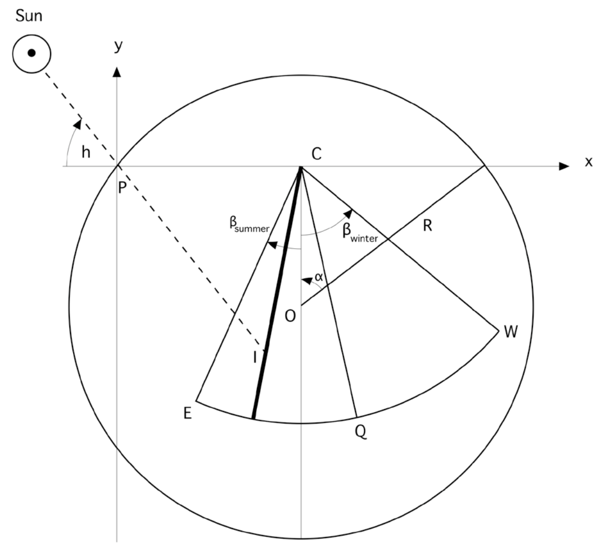

Figure 5. Diagram showing the principle of the "box" altitude dial: with it held vertically in the solar plane, the rays enter at P and strike the mobile ruler (which is very narrow); one reads off the hour at the intersection of the ruler and the drawing. The latter is contained within a triangular zone, with points E, Q, and W being the positions of the spot of light at solar noon (seasonal hour 6) respetively on the summer solstice, the equinoxes, and the winter solstice. The date lines converge on C, which is the mounting point of the ruler, while the hour arcs form a fan extending from the lowest arc (noon) to the center (sunrise/sunset).

Sun's rays enter. Let a system of axes pass through *P*, such that *x* tends rightward and *y* upward. We have:

(1) $OC = R \cos \alpha,$

where α designates the angle of aperture of the horary fan ($\alpha = 90°$ signifies that *C* coincides with *O*). One must fix the radius of the horary fan taking into account the eccentricity of C with a view to filling up the greatest possible surface in the box. Let *E*, *Q*, and *W* be the positions of the Sun at solar noon (seasonal hour 6) respectively on the summer solstice, the equinoxes, and the winter solstice. Hence we have

(2) $CE = CQ = CW = r$

This is also the radius of the mobile ruler revolving around *C*.

First of all, one aligns the ruler with the desired date. At a given moment, with the dial oriented in the plane of the Sun, a solar ray enters by the orifice *P* and strikes the ruler at *I*. The seasonal hour is read at the intersection of the date line and the hour curve. Let us seek the coordinates *x* and *y* of this intersection point.

Let *h'* be the noon altitude of the Sun:

(3) $h' = 90° - \phi + \delta$

where ϕ is the latitude of the locality and δ the declination of the Sun. The altitude *h* of the Sun for a given hour angle *H* is:

(4) $\sin h = \sin \phi \sin \delta + \cos \phi \cos \delta \cos H$

H_0 is the semidiurnal arc, which one obtains thus:

(5) $\cos H_0 = -\tan \phi \tan \delta$

The seasonal hour k indicated by the dial (such that 0 h corresponds to sunrise, 6 h to solar noon, and 12 h to sunset) is obtained thus:

(6) $$k = \frac{6 \times (H + H_0)}{H_0}$$

from which we deduce H.

Next we have:

(7) $$\sin \gamma' = \frac{R \sin \alpha \sin h'}{r}$$

(8) $\gamma = h' - h + \gamma'$

(9) $\beta = 90° - \gamma' - h'$

(10) $$PI = \frac{R \sin \alpha \cos \beta}{\sin \gamma}$$

(11) $x = PI \cos h$

(12) $y = -PI \sin h$

β here represents the angle between a date line and the vertical through O and the point of convergence. The total opening of the angular sector equals $|\beta_{summer}| + |\beta_{winter}|$.

Numerical example: let there be a medallion dial with radius $R = 7$ cm, calculated for a latitude $\phi = 38°$, obliquity $\varepsilon = 24°$. One chooses a radius for the horary fan $r = 8$ cm, setting $\alpha = 50°$. Let us calculate the coordinates for seasonal hour $k = 8$ on the summer solstice ($\delta = +24°$). We have $H = 36.785°$ and consequently:

(13) $h' = 76°$

(14) $h = 55.787°$

(15) $\gamma' = 40.570°$

(16) $\gamma = 60.783°$

(17) $\beta = -26.570°$

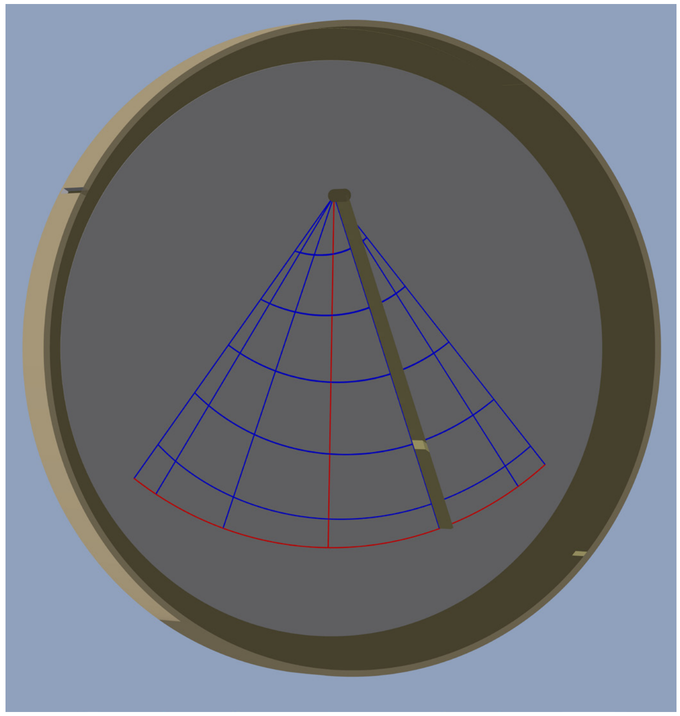

Figure 6. Modern reconstruction of the "box" altitude dial for a latitude of 38°. The rays enter by the lateral notch and strike the ruler, which is calibrated according to the month (here $\delta = -11.47°$, zodiacal signs Pisces/Scorpio). If one considers the time to be in the afternoon, it is seasonal 8 h. The red circular arc corresponds to solar noon; the red straight line in the middle corresponds to the equinoxes.

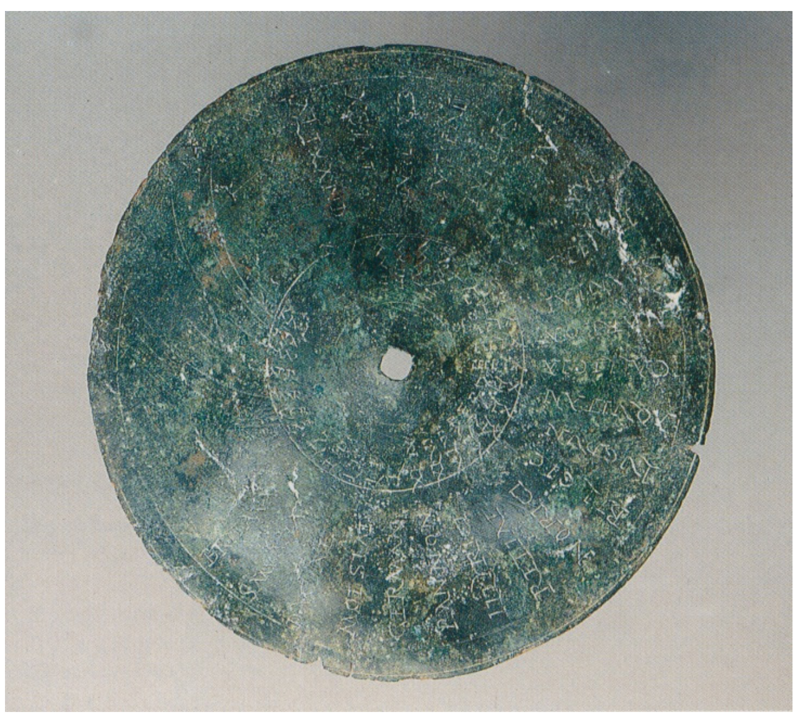

Figure 7. Bronze disk of diameter 10.4 cm, discovered in a Gallo-Roman villa at Berteaucourt-les-Dames, a locality in the north of France in Picardie, département de la Somme. The object dates from the 2nd century of our era.

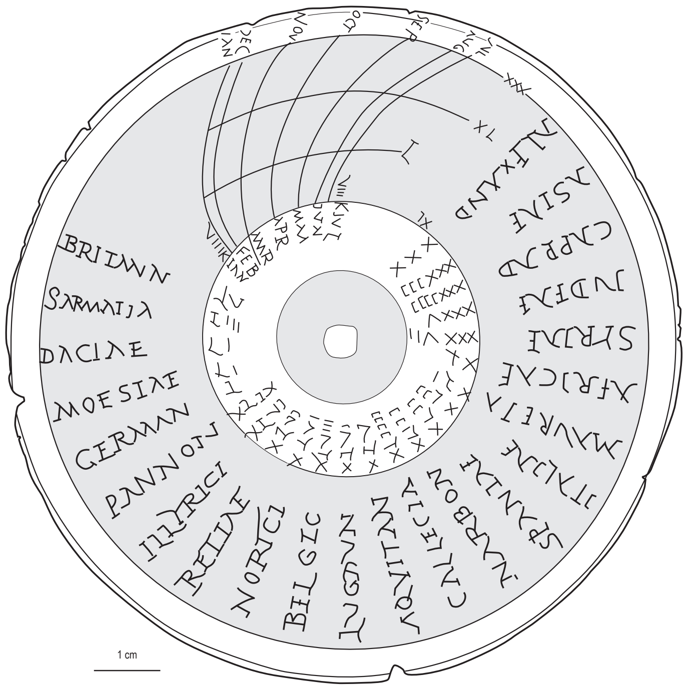

Figure 8. Drawing faithfully reproducing the inscriptions and drawing of the Berteaucourt-les-Dames disk. The upper part consists of a grid where one recognizes the names of the months; the rest of the disk is filled by a list of 23 Roman provinces, and occasionally cities, accompanied by their increasing latitudes. This progressive list begins with ALEXAND(riae) [latitude] XXX and ends with BRITANN(iae) [latitude] LX. The disk is perforated by a hole at the center.

(18) $PI = 5.495$ cm

(19) $x = 3.09$ cm

(20) $y = -4.544$ cm

(the superfluous decimals are given only for the sake of verification).

The geometrical construction is very simple. Once the radius *r* of the angular sector is defined, one obtains the intersection points of the hours on a date line while varying the solar altitude. Thus assumes that the maker has at his disposal a table giving the solar altitude as a function of the seasonal hour and the date. In fact, one can replace the orifice in the raised rim by a horizontal gnomon; the infinite shadow of the gnomon intersects the grid where one reads the hour according to the date (Fig. 6).

The grid that one obtains is quite harmonious. The hour curves are not excessively squeezed together as is the case if one draws the dial by true solar time.[10] It goes without saying that in the case of the Vienna dial, the tininess of the dials (to say nothing of their very crude execution), combined with the fact that one has to balance them vertically (which perhaps implies that there existed a suspension thread) renders them pure objects of prestige rather than real dials to indicate the time.

10 Such dials using equinoctial hours are apparently are unknown.

Portable meridians, indicators of latitude.

The two bronze disks found in the archeological excavations in 1985 at Berteaucourt-les-Dames in Picardie (France)[11] and in 1994 at Merida (Spain)[12] date respectively from the ends of the 2nd and of the 3rd centuries of our era. The archeologists at the time, contemplating their purpose, classified them as portable sundials of a new variety, without pursuing the question further. A recent study,[13] however, has made it possible to determine their functions more precisely and to bring new clarity to these little known objects.

As one can see in Fig. 7, which represents the dial of Berteaucourt-les-Dames, the object consists of a disk of about 10 cm in diameter perforated at the center by a hole (the Merida dial has diameter 13 cm). One can divide the principal face (Fig. 8) in two parts:

– the "geographical" part, with 23 names of provinces or cities sorted according to increasing latitude from *Alexandriae* (latitude 30°) to *Britanniae* (latitude 55°).[14]

– the strictly gnomonic part, which is a grid composed of four concentric curves, ascending from latitude 30° to 60° from outside to inside in steps of 10°. Seven other curves cross the latitude circles and bear labels for the twelve months of the Roman calendar, the two extreme curves being those for the solstices[15] with labels VIII K. IAN (VIII before the Kalends of January = winter solstice = December 25), et VIII K. IUL (VIII before the Kalends of July = summer solstice = June 24). The other dates doubtless correspond to the Sun's entries into the zodiacal signs.[16]

On the object's rear face is a straight line, whose significance we will see presently, which extends from the central hole to the periphery. A horizontal gnomon several centimeters long must have been lodged in this hole; it must have been long enough to cast a shadow on the disk and its diameter small enough to allow a sensitive reading.

11 J. L. Massy, *Gallia*, t. 43, fascicule 2, 1985, p. 481-482. This disk, discovered in a Gallo-Roman villa is preserved at the Musée de Picardie at Amiens. It is in much better condition than the Merida disk. See also C. Hoët-van Cauwenbergue, E. Binet, "Cadran solaire sur os à Amiens (Samarobriva)," *Cahiers Glotz*, XIX, 2008, p. 123-124.

12 J. Arce, "Viatoria pensilia. Un nuevo reloj portatil del s. III D. C. Procedente de Augusta Emerita (Mérida, Espana)," *Merida Tardorromana* (300-580 d. C.), 2002, p. 217-226. This disk is preserved at the National Museum of Roman Art of Merida (Estremadura). The latitude circles are effaced; six date arcs survive.

13 See D. Savoie and M. Goutaudier, "Les disques de Berteaucourt-les-Dames et de Mérida : méridiennes portatives ou indicateurs de latitude?", *Revue du Nord*, t. 94, n° 398, 2012, p. 115-119.

14 The Merida disk bears 19 names of provinces in contrast to 23 for that of Berteaucourt-les-Dames. On the latter, there are some exceptions to the increasing order of the latitudes. On the geographical labels inscribed on portable sundials, see C. Hoët-van Cauwenbergue, "Le disque de Berteaucourt-les-dames (cité des Ambiens) et les listes gravées sur les cadrans solaires portatifs pour voyageurs dans le monde romain," *Revue du Nord*, t. 94, n° 398, 2012, p. 97-114.

15 See F. K. Ginzel, *Handbuch der Mathematischen und Technischen Chronologie*, vol. 2, Leipzig, 1911, p. 179-181 et p. 282. The solstice dates given here are the classical dates in the Julian calendar that came into use in 45 BC; as for the equinoxes, they are fixed on March 25 (VIII before the Kalends of April) and September 24 (VIII before the Kalends of October). At the beginning of the 3rd century of our era, because of the shift of the Julian year relative to the tropical year, the astronomical seasons began on the following dates (see Table 5 below): vernal equinox on March 21, summer solstice on June 23, autumnal equinox on September 24, and winter solstice on December 22. Hence the user of the instrument would commit a slight error in retaining the common dates.

16 See on this point P. Brind'Amour, *Le calendrier romain*, Université d'Ottawa, 1983, p. 15-19.

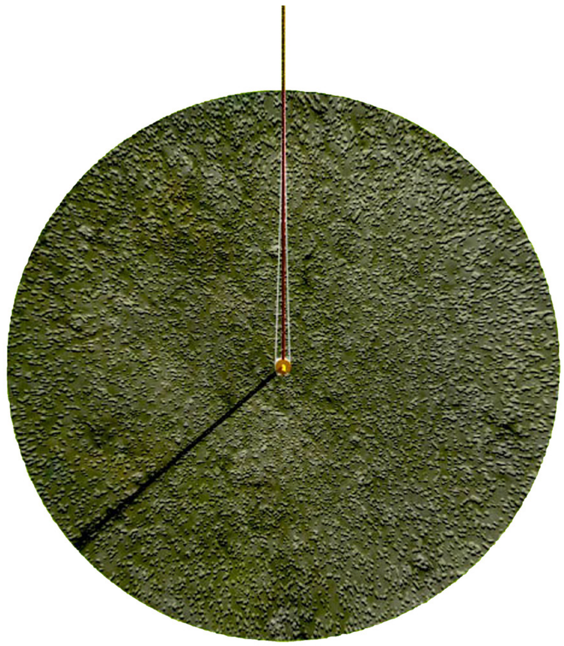

Figure 9. Photo of the rear face of the Berteaucourt-les-Dames disk. A gnomon was likely inserted in the hole to cast a shadow on the grid figuring on the other face of the disk. On the rear face shown here, a groove along the radius of the disk enables one to adjust it in the direction of the zenith.

There is no horary graduation on the grid, and this allows us to conclude that the purpose of this instrument was not to show the hour throughout the day (though it could indicate noon). What the grid does clearly show is the existence of a relation between the locality's latitude and the date; hence one deduces that this instrument could have two functions, possibly at the same time:

– to indicate the solar noon for the locality

– to indicate the latitude where one is

Let us recall that in altitude sundials, the principle is to determine the solar time as a function of the Sun's altitude above the horizon, and that this requires knowledge of two constants: the latitude of the locality and the date. This results from the fact that this kind of dial is a practical and technical application of a formula of spherical trigonometry known in antiquity that relates the time H to the Sun's altitude h:

$$\sin h = \sin \phi \sin \delta + \cos \phi \cos \delta \cos H \tag{21}$$

where δ is the Sun's declination (effectively the date) and ϕ is the latitude. This formula shows very clearly that the Sun's altitude depends on three components: the place, the date, and the time. Hence an altitude sundial intended to work throughout the Roman Empire, say over a range of 30° of latitude (about 3300 km) like the disks of Berteaucourt-les-Dames and Merida, would need either a network of curves so dense as to become illegible, or a mechanism to make it easy to use, such as for example the armillary.

But in the present case, since there is no question of showing the time throughout the day but only solar noon, one can take advantage of a simplification of formula (21), observing that at

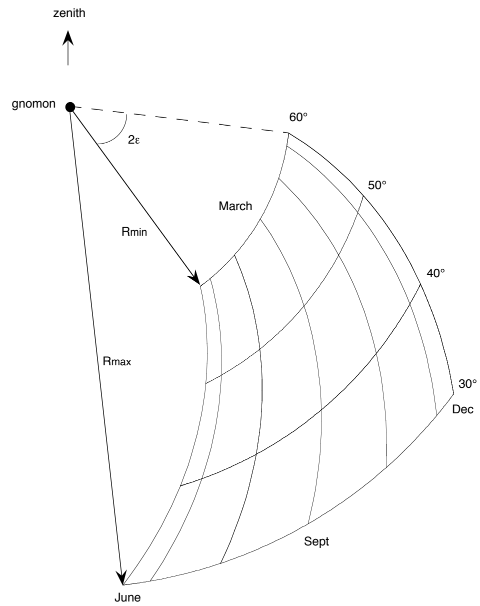

Figure 10. Modern representation of the grid. It comprises four concentric circles indicating the latitude (from 30° to 60°), crossed by seven date curves.

solar noon (H = 0°), the Sun's altitude becomes:

(22) $\sin h = \cos(\phi - \delta)$

hence

(23) $h = 90° - \phi + \delta$

The Sun's declination is obtained by the likewise classical formula:

(24) $\sin \delta = \sin \varepsilon \sin \lambda$

where λ is the Sun's ecliptic longitude, which is made to range from 0° to 360° in steps of 30°, so that one obtains the declination of the Sun at its entry into each zodiacal sign.

The grid under investigation here is a clever application of formula (23), in which one makes the Sun's declination vary as the "abscissa" and the latitude as the "ordinate." The read-off, whether of noon, or of latitude, takes place at the intersection of the shadow of a horizontal gnomon and a declination arc, while the instrument is suspended vertically and aligned with the plane of the Sun. This is corroborated by the straight line on the back of the instrument, which is perfectly aligned with respect to the grid, and which serves to orient the disk to the zenith, doubtless with the help of a suspension thread (Fig. 9). The graduated face is the one that is directed to the east.

The calculation of such an instrument is quite simple. One sets, starting from the hole, radius R_{max}, which corresponds to the minimum latitude (here 30°) and radius R_{min}, which corresponds to the maximum latitude (here 60°). Radius R for an intermediate latitude is thus obtained by a simple rule of three:

(25) $R = R_{max} - [\Delta R(\phi - 30)/30]$ where $\Delta R = R_{max} - R_{min}$

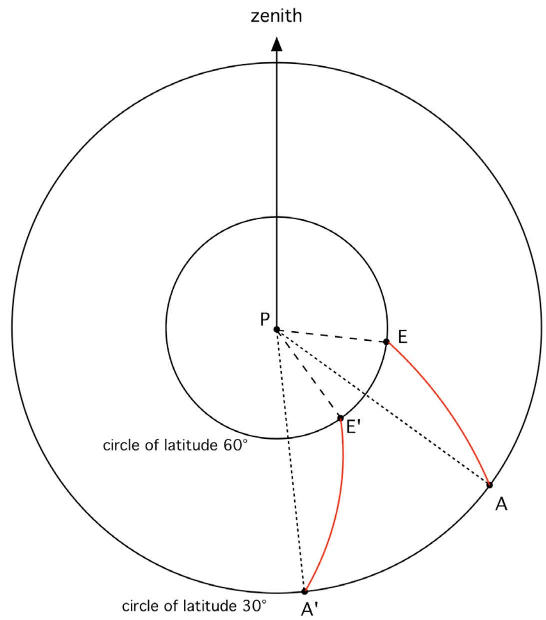

Figure 11. Geometrical drawing of the grid. Arcs EE' and AA' are respectively the arcs for latitude 60° and 30°. Arcs EA (the curve for the winter solstice) and E'A' (for the summer solstice) are in red.

Making a system of axes pass through the disk's center, with x tending northward and y towards the zenith, one draws a declination arc or a circle of latitude thus:

(26) $x = R \cos h = R \sin (\phi - \delta)$

(27) $y = - R \sin h = - R \cos (\phi - \delta)$

If one treats the latitude as variable for a given solar declination, one gets a declination arc. If one treats the declination as variable for a given latitude, one gets a latitude circle. The declination should range from $-\varepsilon$ to $+\varepsilon$ where ε is the obliquity of the ecliptic; in antiquity one generally uses $\varepsilon = 24°$. In any case one can verify on the Berteaucourt-les-Dames disk that the angle at the vertex (Fig. 10) is indeed equal to $2\,\varepsilon$, which corresponds to the difference in solar altitude between the winter and summer solstice at latitude 60°; the same angle is found again for the circle for latitude 30°.

In practice, such a grid does not require recourse to trigonometrical calculation; pure geometry allows one to draw it. One starts by establishing, with the help of the elementary formula (2), the values of the Sun's noon altitude for different latitudes. After drawing two concentric circles with a compass (Fig. 11), one uses a protractor centered on the center P of the circles, which is also where the gnomon will be, to trace the Sun's altitude in winter for the maximum latitude (point E) and then the altitude in summer for this same latitude (E'). One carries out the same operation for the outermost latitude circle, obtaining points A and A'. To obtain the date curves between the extremal circles, one must of course generate the altitude points for the intermediate latitudes and then join up the points by a curve.

Let us say at once that one should not look for precision instruments in these two disks: their dimensions are in the first place too slight and in the second place one should regard them more as objects of curiosity or prestige, or maybe astronomical amusement, even if they are

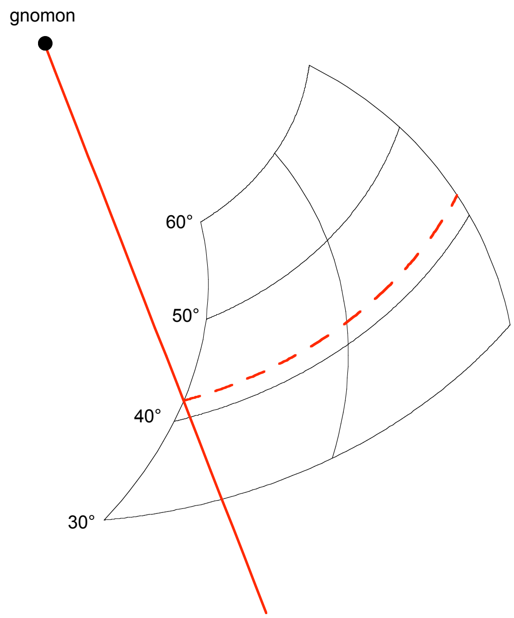

Figure 12. The gnomon's shadow (red) cuts the summer solstice curve at latitude 42° (broken circle).

scientifically quite sophisticated. In fact, what one has here are instruments that give orders of magnitude and that assume a nontrivial level of knowledge on the part of the user.

Consider first of all the case of a traveler who wants to use this indicator to determine solar noon, thus as a portable meridian. He absolutely has to know the date and the place where he is. Let us imagine that the date is the 8th day before the Kalends of July, that is, the summer solstice, and that the traveler as at Rome at a latitude of 42° as the disk indicates (Fig. 12). After a preliminary identification of the latitude circle for 42°, the traveler has to estimate *grosso modo* the moment when the gnomon's shadow cuts the latitude circle and the declination curve. One can imagine that to find such a circle a thread was provided, attached to the disk's center and furnished with a sliding bead.

It is appropriate here to note that the Sun's altitude around its culmination has little variation, especially around the solstices, so that one could easily be wrong by ±15 minutes or more, and that is without taking account of the fact that the shadow of a horizontal gnomon has a certain width. In any event it is an inconvenience that one has to underline and that affects all altitude sundials: their accuracy is mediocre at noon.

Now let us put ourselves in the hypothetical situation of a traveler who uses the disk to have an idea of the region where he is. Again he knows the date, but he also has to find a means of determining solar noon, for example by means of a gnomon, waiting for its shadow to be shortest (Fig. 13). Let us go back to the preceding example of a Roman traveler; at solar noon he has to find the intersection of the shadow with the date curve so that he can then read off the latitude. Here again, one can imagine that a thread furnished with a bead allows one better to estimate the place, or rather the region. But one should add right away that the instrument does not yield the longitude of the region or place. Hence one has to have already an idea of the place where one is, without which one might, for example, establish a latitude of 42° without knowing

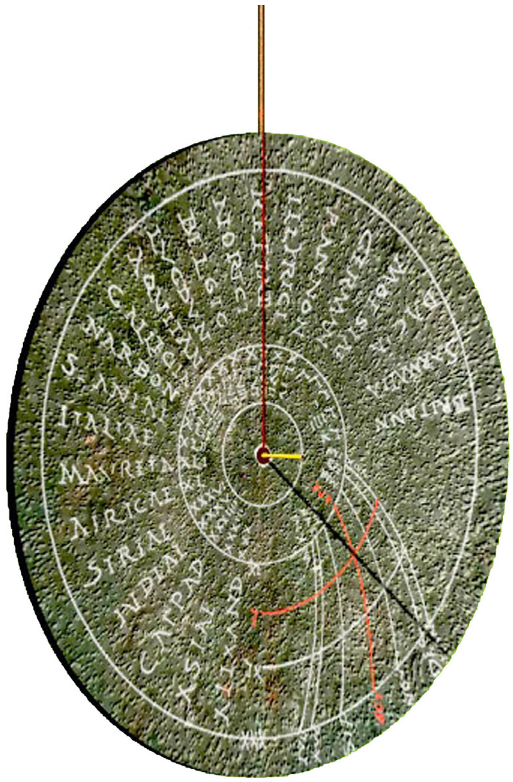

Figure 13. Method of use of the Berteaucourt-les-Dames disk. The disk is suspended in the plane of the Sun. One can here see that the horizontal gnomon casts a shadow upon the grid, cutting the curve for latitude 50° at the equinoxes (red).

whether one is in Spain or in Italy!

An attempt at a universal portable dial.

Among all the ancient portable sundials, the most numerous at present are the eleven Greek and Roman exemplars, constructed between the 1st and the end of the 6th century, of a very particular type that has tripped up many authors with regard to its functioning and hence to its theory, a bit like the case of another portable dial, in this case medieval, the *Navicula de Venetiis*.[17] These two sundials additionally have points in common, such as that of being usable at practically all latitudes and that of not being rigorously exact except on certain dates while being theoretically very accurate throughout the year. One of these dials, lost today, was discovered in France in the 19th century, at a place called Crêt-Chatelard (département de la Loire).[18]

The first to understand the working of these sundials, which had been known since the 18th century and which exhibit great variations among themselves, was the German gnomonist J.

17 See C. Eagleton, *Monks, Manuscripts and Sundials, The Navicula in Medieval England*, Brill, Boston, 2010. The calculation of the error in read-off on the Navicula was made by J. Kragten, *The Little Ship of Venise – Navicula de Venetiis*, Eindhoven, 1989.

18 This dial was described by V. Durand, followed by a restoration and—erroneous—instructions for use by general De La Noë : “Cadran solaire portatif trouvé au Crêt-Chatelard,” *Bulletin et Mémoire de la Société Nationale des Antiquaires de France*, t. 7, mémoires 1896, Paris, 1897, p. 1–38. De La Noë, who does not hesitate to characterize as “crude” an earlier attempt at explication by Baldini (*cf. infra*), had absolutely no understanding of this dial, which he placed horizontally.

Figure 14. Roman exemplar found near Bratislava, dating from the 3rd century. The instrument is complete. Clearly visible are the mobile and curved read-off scale with its hour strokes, at the end of which is the gnomon. At the extremities of the date scale are the inscriptions VIII K. IAN and VIII K. IVL, which correspond to December 25 and June 24, the dates considered to be the winter and summer solstices in the Julian calendar.

Figure 15. Byzantine exemplar dating from the 5th-6th century, diameter 110 mm. On the gnomonic face can be seen the cursor, which enables the dial to be set according to the latitude, and the pendant ring for keeping the instrument vertical in a chosen direction. On the angular sectors appear abbreviations in Greek letters of the Latin names of the months for each half year, January to June on the upper part, July to December on the lower. The read-off scale is lost. On the back are the latitudes of 36 cities and provinces. [credit: Trustees of the British Museum].

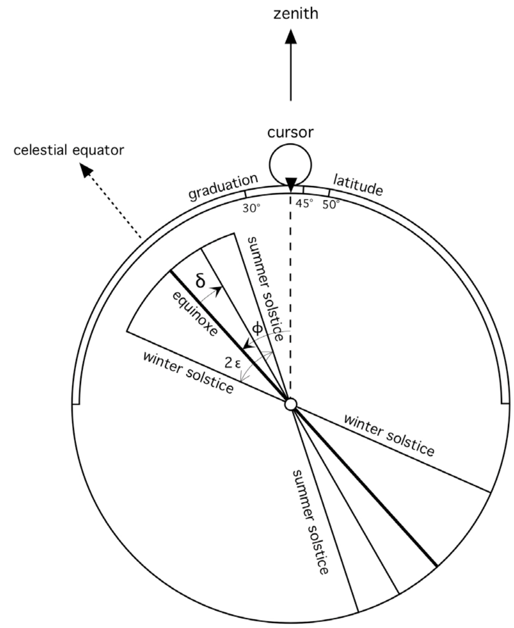

Figure 16. Astronomical indications of the gnomonic face of the portable dial, required for setting it.

Drecker at the beginning of the 20th century.[19] Several studies have been published since on the subject, the most thorough to date being that of M. T. Wright in 2000.[20]

These dials are quite simple in appearance (Fig. 14 and 15). They are disks of between 5 and 12 cm diameter. On one face names of cities or provinces are inscribed with their latitudes. On the other face are engraved labels pertaining to months (notably equinoxes and solstices). The mobile scale for reading off the time is not inscribed with numbers; it is curved, and the gnomon is at its extremity. On the disk’s periphery, a sliding rail assemblage, under which the latitudes to which the instrument can be set are marked, allows a cursor or pendant ring to be slid to calibrate the dial for the place where it is to be used.

19 J. Drecker, *Die Theorie der Sonnenuhren*, *op. cit*., p. 64-66. Drecker was also the first to estimate the error in the read-off of the time as a function of the date and latitude. Two earlier authors (Baldini and Woepcke) had already studied this dial but they were completely misguided about its operation: G. Baldini, “Sopra un’antica piastra di bronzo che si suppone un orologio da sole,” *Saggi di dissertazioni accademiche publicamente lette nella nobile Academia Etrusca di Cortona* , vol. III, Rome, 1741, p. 185-194 ; F. Woepcke, *Disquisitiones archaeologio-mathematicae circa solaria vetrum*, (Diss. Inaug.), Berlin, 1842, p. 14-19. In their defense, one should note that the model of dial on which they based their arguments was incomplete, the gnomon situated at the end of the read-off scale having been lost.

20 M. T. Wright, “Greek and Roman Portable Sundials – An Ancient Essay in Approximation,” *Archive for History of Exact Sciences*, vol. 55, 2000, p. 177-187. Wright, who curiously does not cite Drecker, employs a Vitruvian analemma in his trigonometrical demonstrations, which is admirable. We may also cite the brief study by F. A. Stebbins, “A Roman Sundial,” *Journal of the Royal Astronomical Society of Canada*, vol. 52, 1958, p. 250-254, who has the merit of having explained clearly how one used these portable dials. See also the classic and very good article by D. J. de Solla Price, “Portable Sundials in Antiquity, including an Account of a New Example from Aphrodisias,” *Centaurus*, 1969, vol. 14, p. 242-266. Many authors, more specialized in archeology, have concentrated on the geographical lists that appear on the backs of the dials, which in some cases give as many as 36 places. Attention should also be drawn to an inscribed marble slab found at Aquincum, Hungary, and sometimes miscalled *tabula gromatici*, which appears to have been a set of templates for constructing this type of dial; see P. Albèri-Auber, “The Aquincum fragment,” *The Compendium: Journal of the North American Sundial Society* 25 (2), 2018, 13–24.

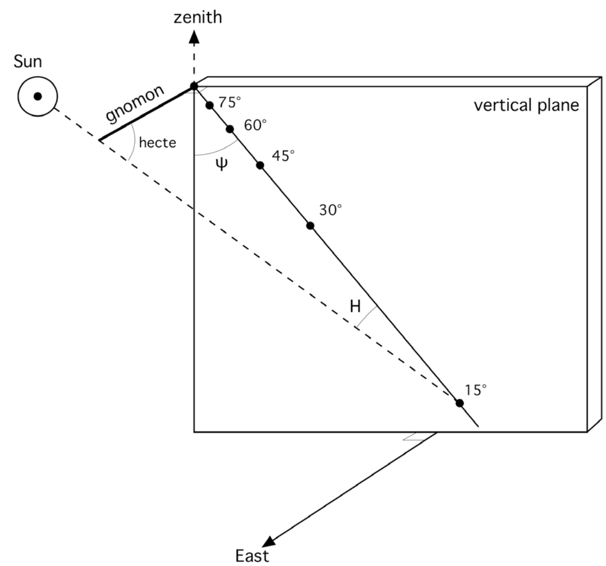

Figure 17. The portable dial is here reduced to a vertical plane oriented due east and furnished with a horizontal gnomon casting a shadow on a graduated equinoctial scale.

Let us describe with greater precision the "gnomonic" face of this dial, which, let us specify at the outset, is always used in a vertical position (Fig. 16). Its layout is in the first instance an instantiation of the celestial equator; this makes an angle with the zenith equal to the latitude of the locality. This explains why a sliding rail assemblage makes it possible to calibrate the dial in accordance with the latitude ϕ. On each side of the equator, one traces the Sun's position right to the solstices. That is, the angle between the equator and the positions specified by the calendar corresponds to the declination δ of the Sun, whose absolute value is equal to the obliquity at the two solstices.

In antiquity one uses $\varepsilon = 24°$ (*cf. infra*); one gets the Sun's declination as a function of the date, which is represented by the ecliptic longitude λ, varying by steps of 30°:

$$\sin \delta = \sin \varepsilon \sin \lambda \qquad (28)$$

The dial can be reduced initially (Fig. 17) to an equinoctial straight line drawn on a vertical plane exactly oriented towards the east, being mobile around its gnomon, and having the points for the hours drawn on it at intervals of 15° (tangent law: the distance of an hour point to the foot of a gnomon of length a is obtained as ($a/\tan H$), where H is the Sun's hour angle). This equinoctial line likewise makes an angle ψ with the vertical passing through the gnomon which is precisely equal, on the equinoxes, to the locality's latitude. As it is drawn for the moment, the dial resembles an oriental vertical sundial with gnomonic declination[21] $D = -90°$, functioning only on the equinoxes.

Let us remark at once that to draw such a dial, an ancient gnomonist in possession of Ptolemy's *Treatise on the Analemma*[22] would have noted that the horary angle H of the Sun is the

21 The gnomonic declination D is the azimuth of the line perpendicular to the plane.

22 The edition of Heiberg de 1907 is the revised edition of the Latin text and the Greek fragments that he had already published in 1895 : *Claudii Ptolemaei Opera quae exstant omnia*, Vol. II, *Opera Astronomica Minora*, ed. J. L. Heiberg, Leipzig, Teubner, 1907, p. 187-223 et *Praefatio*, p. XI-XII. This edition is the standard at present. One can find

H	H'	$90° - hecte$
−90°	−90°	−90°
−75°	−75°	−75°
−60°	−60°	−60°
−45°	−45°	−45°
−30°	−30°	−30°
−15°	−15°	−15°
0°	0°	0°

Table 2. Comparison of equinoctial and seasonal horary angles with the complement of the hectemoros for equinoxes.

complementary angle of the *hectemoros*, which last is the angle between the direction to the Sun and the cardinal east (or west) point. This *hectemoros* is independent of the latitude, since it is calculated as:

(29) $\cos hecte = \sin H$

It is interesting to make a tabular comparison of the equinoctial horary angle H, the Sun's seasonal hour angle H' (as in all ancient sundials, this portable dial shows the seasonal hour), and the complement of the *hectemoros*, since one observes a certain similarity, in fact a strict equivalence that—perhaps—was the origin of this portable sundial's mode of operation. On the equinoxes, the equivalence is perfect (Table 2).

Let us recall that H' is obtained thus:

(30) $$H' = \frac{H.90°}{H_0}$$

Where H_0 is the semidiurnal arc obtained as:

(31) $\cos H_0 = - \tan \phi \tan \delta$

The seasonal hour[23] k indicated by the dial (with 0 h corresponding to sunrise, 12 h to sunset) is:

(32) $$k = \frac{6 \times (H + H_0)}{H_0}$$

or

(33) $$k = \frac{6 \times (H' + 90)}{90}$$

a modern Italian translation facing the Latin in R. Sinisgalli, S. Vastola, *L'analemma di Tolomeo*, Edizioni Cadmo, Firenze, 1992. See O. Neugebauer, *HAMA*, Springer-Verlag, Berlin-Heidelberg-New York, 1975, t. II, p. 840-841 and D. Savoie, *Recherches sur les cadrans solaires*, Brépols, Turnhout, 2014, chap. IV.

23 If one keeps to the classical definition, the seasonal hour has no meaning beyond the polar circles (latitude 90· ± obliquity) at certain times of year because the Sun can be circumpolar.

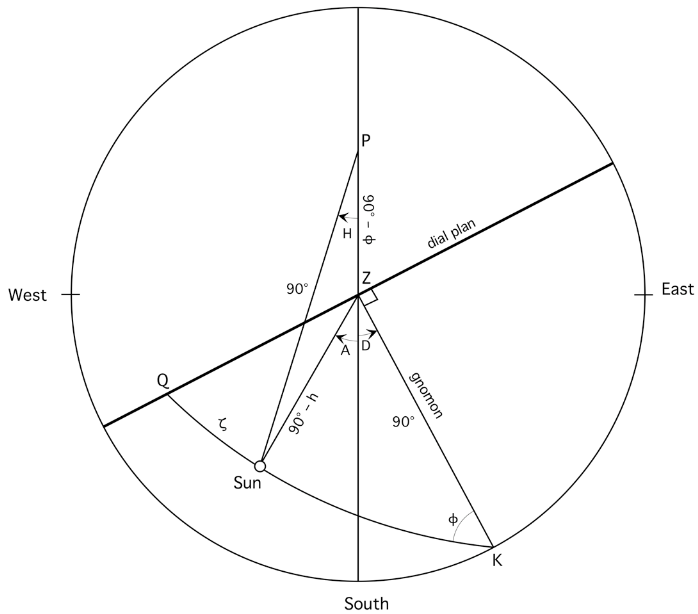

Figure 18. Representation by spherical trigonometry (viewed from the zenith) of the portable dial in use on the equinoxes. P is the north celestial pole, Z the zenith, K the tip of the gnomon.

On the equinoxes, at our latitudes, the dial at sunrise has its gnomon pointing exactly eastward; then the shadow shifts progressively during the morning along the equinoctial line until noon, when the Sun, now in the direction of geographic south, produces an infinite gnomon shadow. Throughout the morning, the plane of the sundial has stayed perfectly fixed, with the gnomon always having a gnomonic declination of 90°. But for the dial to continue to work in the afternoon, one has to pivot its vertical plane around an axis passing through the zenith. Otherwise put, the gnomon will now point towards the south-east horizon, then the south-west horizon, finally pointing exactly westward at the moment of sunset. The plane of the dial will thus have turned 180°.

One could imagine a turning about of the dial, such that the east face becomes, after solar noon, the west face. But this operation would call for a rotation equal to 2ψ of the scale and the gnomon and hence a new calibration according to the date and latitude. Now none of the dials known at present bears a double graduation in latitude and in the scale of dates that would make this kind of setting possible. Moreover the user does not know *a priori* which part of the day—morning or afternoon—he is in.

It is well to emphasize here that right from the moment that one makes the plane of the dial pivot, thus changing its gnomonic declination, the angle between the direction to the Sun and the direction in which the gnomon points is no longer the *hectemoros* but another angle that we will call ζ: this is the angle that one reads on the dial. This angle ζ can be shown to be calculated thus (Fig. 18 and 19):

$$(34) \qquad \cos\zeta = \frac{\sin h}{\cos\phi}$$

where h is the Sun's altitude. Since we are at the equinoxes, one has:

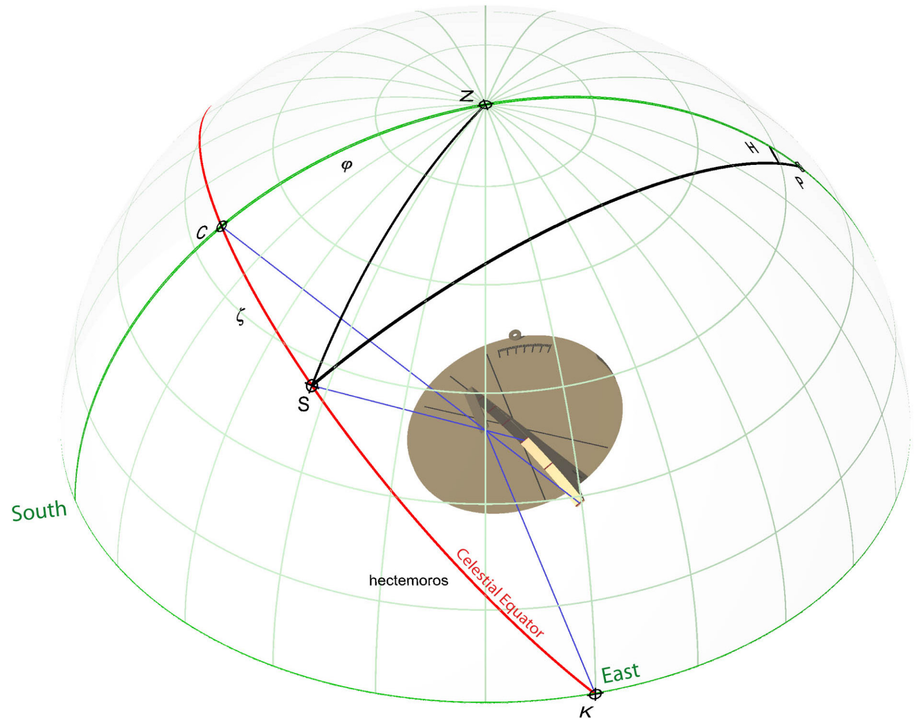

Figure 19. Representation in the celestial sphere of the dial in use in the morning on the equinoxes. The Sun S, situated on the celestial equator, illuminates the portable dial, which remains immobile. On the dial, the shadow falls upon the graduation for seasonal hour 4.

(35) $\sin h = \cos \phi \cos H,$

from which it follows that $\zeta = H$.

To sum up, on the equinoxes the operation of the dial can be divided into two phases: in the morning it is a dial with fixed horary angle is if it was a classical sundial drawn on a wall facing due east. After the Sun crosses the meridian, the user "forces" the dial by turning it in such a way that the gnomon's shadow lines up with the equinoctial line.

The relation established previously between the equinoctial hour and the *hectemoros* is only one hypothesis among others for explaining the principle of this portable dial; one could also

H	H'	90° – *hecte*
−105°	−84.422°	−61.935°
−90°	−72.362°	−66.0°
−75°	−60.302°	−61.935°
−60°	−48.241°	−52.293°
−45°	−36.181°	−40.239°
−30°	−24.121°	−27.179°
−15°	−12.060°	−13.677°
0°	0°	0°

Table 3. Comparison of horary angles with the complement of the hectemoros for summer solstice.

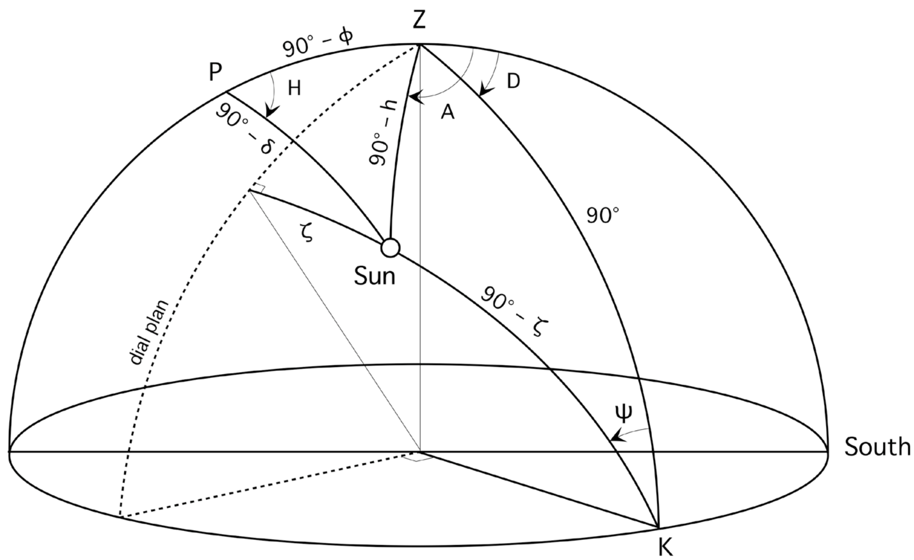

Figure 20. Representation by spherical trigonometry of the principle of the portable dial. The dial measures angle ζ whereas it is set as a function of angle ψ.

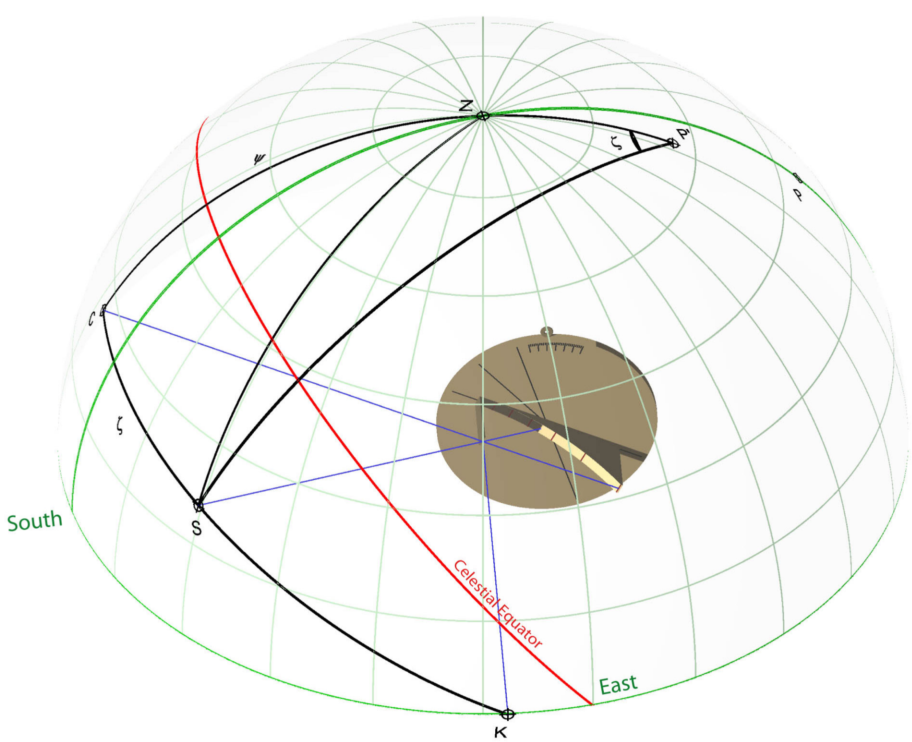

Figure 21. Representation in the celestial sphere of the portable dial in use in the morning on the winter solstice. The dial measures angle ζ which approximates a division by 6 of the semidiurnal arc described by the Sun. The Sun (at *S*) illuminates the dial, which is oriented to the southwest. The gnomon's shadow falls upon the graduation for seasonal hour 3.5.

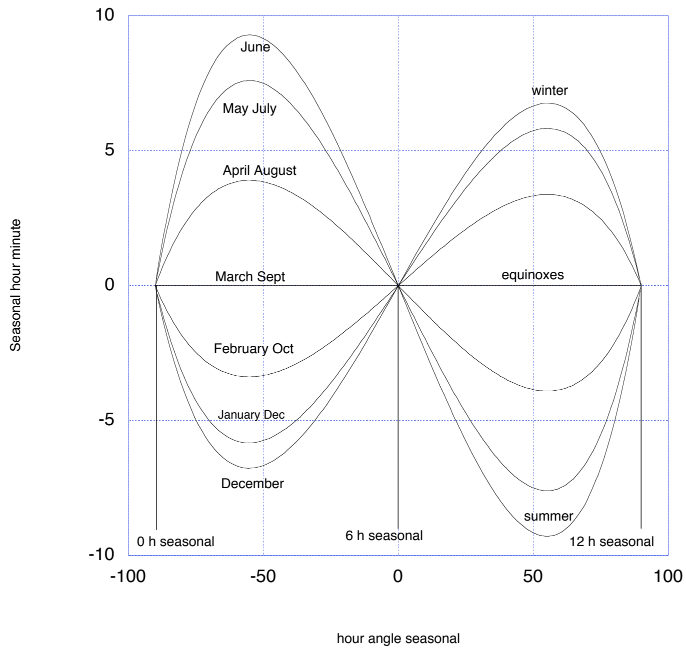

Figure 22. Error in reading the time for latitude 40°.

imagine a purely empirical approach. For that matter, we will see that a recognition in antiquity of the intrinsic error in the principle of this dial is not very probable.

If we resume the analogy between the angles but for an arbitrary date, for example for the summer solstice, for a latitude of 40° and a solar declination of +24°, we see that there is no longer an equivalence among the three aforesaid angles. One gets the following quantities for the morning, keeping in mind that cos *hecte* = sin H cos δ (Table 3).

Additionally, the gnomon's shadow no longer falls on the equinoctial line, but below it. The inventor who conceived of this dial imagined that one should modify the inclination of the equinoctial line, and that this inclination, for a given day, was equal to the noon altitude of the Sun, namely $90° - \phi + \delta$. This implies that angle ψ which the equinoctial line makes with the vertical passing through the gnomon equals $(\phi - \delta)$. The purpose of this constraint is to indicate on the dial, to the extent that this is possible, the seasonal horary angle H'. Now we have seen that the dial measures the angle ζ, and in the general case where the Sun's declination is not zero, it can be shown (Fig. 20) that:

H	H'	90° – *hecte*
−105°	−84.422°	−85.207°
−90°	−72.362°	−74.218°
−75°	−60.302°	−62.587°
−60°	−48.241°	−50.506°
−45°	−36.181°	−38.115°
−30°	−24.121°	−25.516°
−15°	−12.060°	−12.789°
0°	0°	0°

Table 4. Comparison of horary angles with the angle measured on the universal portable dial.

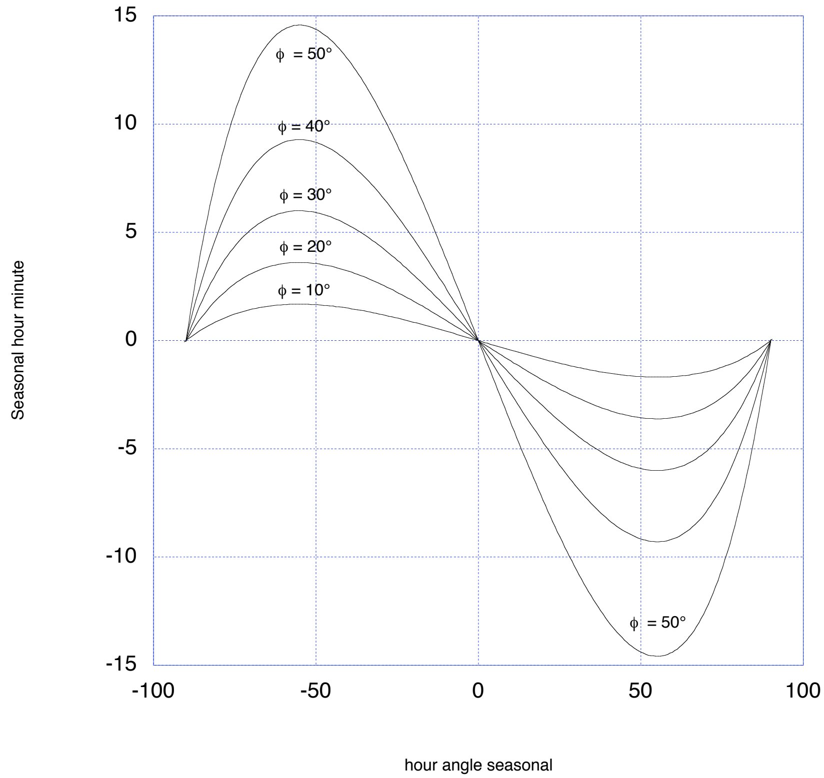

Figure 23. Deviation (H' – ζ) for different latitudes on the summer solstice.

(36) $$\cos\zeta = \frac{\sin\ h}{\cos\psi}$$

or, expressed in terms of the primary parameters:

(37) $$\cos\zeta = \frac{\sin\phi\sin\delta + \cos\phi\cos\delta\cos H}{\cos(\phi-\delta)}$$

such that ζ has the same sign as *H*.

The whole principle of this dial thus rests on the relationship between ζ and *H'*, using as its starting hypothesis the assumption that the deviation between these two angles (expressed on hours) would be acceptable in civil use (*cf. infra).* Let us observe that the principle of its operation is not transferable to a sundial that is supposed to show the equinoctial hour (Fig. 21). In other words, this dial is only useful in a society in which the civil practice is seasonal hours. This could explain why it fell into desuetude, unlike the other kinds of portable sundials which can indicate the seasonal or the equinoctial hour indifferently, such as the cylinder or the armillary, which continued to be used right through the Middle Ages and the Renaissance, and even beyond in the case of the cylinder. This fundamental point seems not to have been noticed: while the majority of ancient sundial types could have survived through the centuries despite the change in usage of hours, the portable dial studied here experienced an abrupt halt because it could not be adapted to equal hours.[24]

24 An example of a truly universal portable dial is that of Regiomontanus.

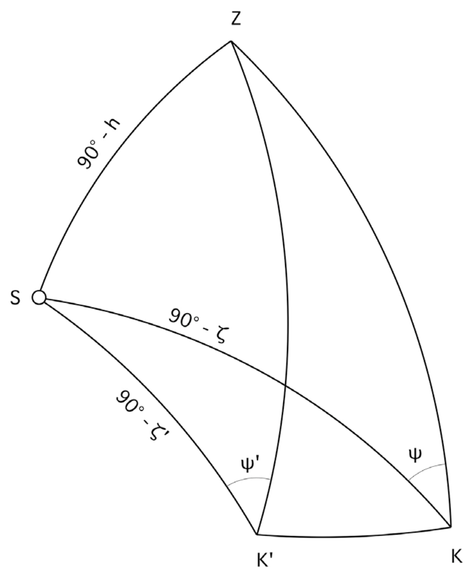

Figure 24. Principle of the calculation of error in latitude, represented in spherical trigonometry. A bad setting of the read-off arm of the dial introduces a false angle ψ', so that instead of measuring angle ζ, the dial measures ζ'.

Resuming the preceding table (for the summer solstice), we can see that the deviation between *H'* and ζ is minimized by the solution tabulated in Table 4. The greatest deviation here between *H'* and ζ (for latitude 40°) reaches a little more than 9 minutes of a seasonal hour (Fig. 22).

One should underline the great subtlety of this portable sundial, which achieves the feat of showing, with a very acceptable error, the seasonal hour, measuring it on a scale graduated in equal angles of 15°, that is, an equinoctial scale. The considerable deviation between the equinoctial horary angle *H* and angle ζ is particularly apparent in this table; for example for *H* = –75° (7h), the deviation almost reaches 50 equinoctial minutes.

When one plots the deviations (H' – ζ) as a function of latitude and as a function of declination (Fig. 23), one finds on the one hand that the greatest deviations are at the summer solstice, and on the other hand, that the deviation increases with higher latitudes.[25] But rather curiously, the deviation always seems to reach its maximum for the same seasonal horary angle in absolute value.

To highlight this feature, it is necessary to study the derivative $^{d(H'-\zeta')}/_{dh}$ and set it equal to zero to obtain the precise seasonal horary angle corresponding to the maximum deviation. We obtain a rather long expression that nevertheless depends only on the semidiurnal arc:

$$H' = \pm 90 \frac{\arcsin\left(\dfrac{180\sqrt{\cos H_0\left[-H_0^2 + 90^2(1-\cos H_0)\right]}}{90^2 - H_0^2}\right)}{H_0} \tag{38}$$

In all rigor, *H'* undergoes a gentle fluctuation of a few minutes as a function of the Sun's declination and the latitude, but always remains very close to a seasonal horary angle of ±55° (except

25 Around the polar circles, the dial becomes unusable for the most part because of the huge error caused by the latitude.

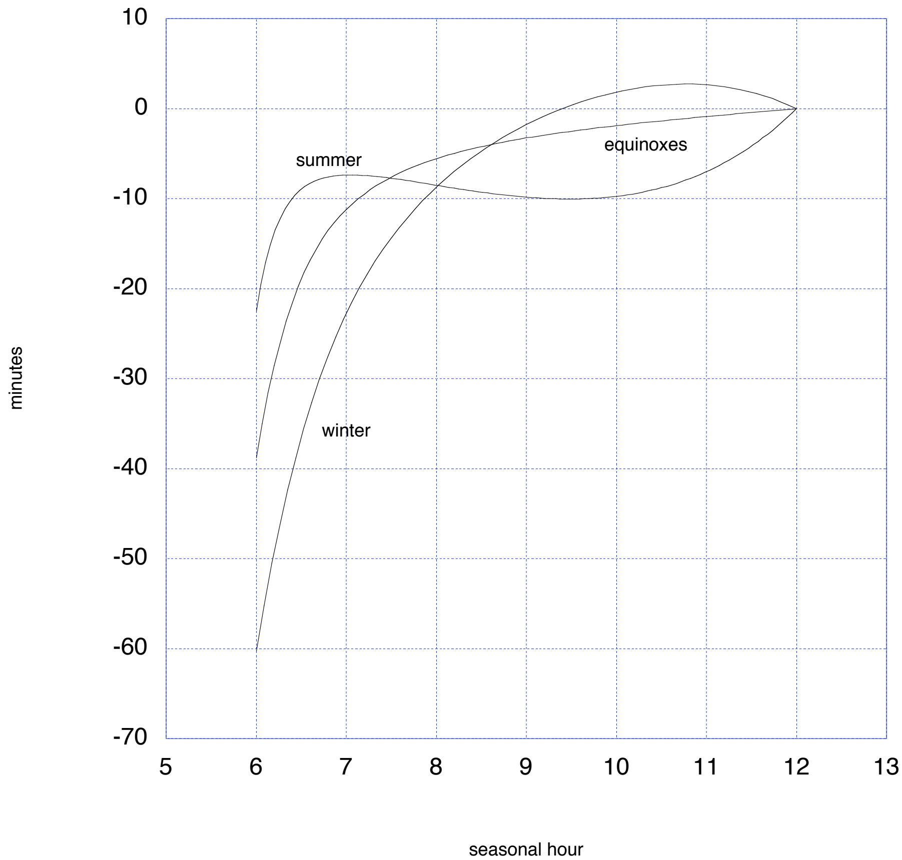

Figure 25. Error in reading the time for φ = 40°, φ' = 39°.

for high latitudes, where one observes a drop starting at ϕ = 62°). For a latitude of 40°, this corresponds to seasonal times of 2 h 19 m and 9 h 40 m in the summer, supposing one could read the time with such precision.

When the user decides to read the time on this dial, he first has to set it in latitude by positioning the cursor on the circular scale; then he must calibrate the mobile arm according to the date (this presupposes a clamp to keep the arm fixed). The user then causes the dial to turn until the shadow of the gnomon falls upon the read-off scale. If the user is uncertain about whether it is before or after solar noon, he has to make a first reading of the time and then wait before making a second reading; if the shadow shifts away from the gnomon, it is morning, while if it shifts towards the gnomon, it is afternoon.

In addition to the intrinsic error of this dial (*cf. supra*), many causes can perturb the accuracy of the indicated time and even make it impossible to read off. The first cause of error is incorrect setting of the latitude: on all the portable dials that have been found, the latitudes of cities or provinces appear on the back. If broadly speaking the latitudes correspond to reality for the cities, the same does not go for the provinces, for which the latitudinal extension can be considerable.[26]

26 There are additionally errors in the latitudes of cities, and one can find variants between the exemplars. Constantinople, for example, is places at 41° de latitude in the Aphrodisias exemplar but at 43° in the Samos, Memphis, and Rockford exemplars. See the table provided by J. V. Field and M. T. Wright, "Gears from the Byzantines : A Portable Sundial with Calendrical Gearing," *Annals of Science*, 42, 1985, p. 109-110.

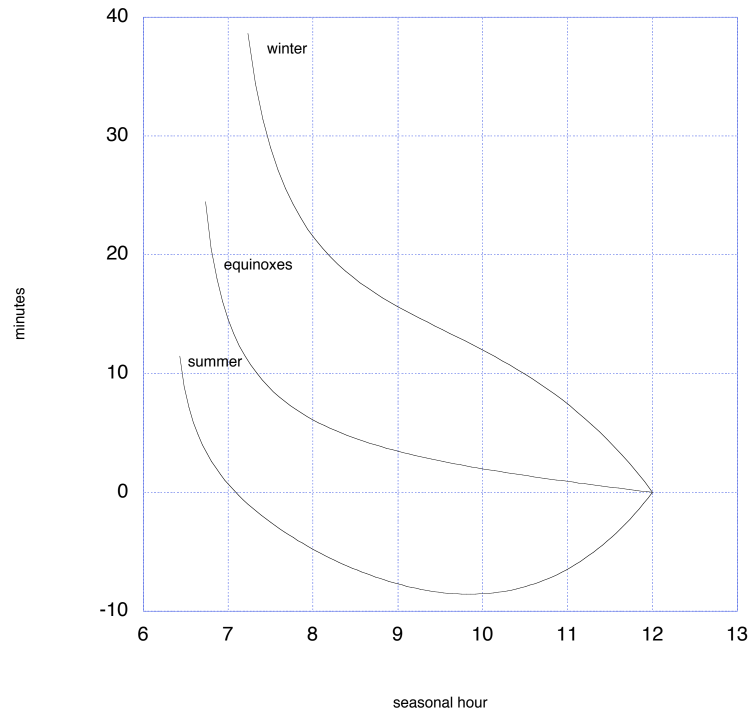

Figure 26. Error in reading the time for φ = 40°, φ' = 41°.

Getting the latitude wrong amounts to getting angle ψ wrong, hence to inclining the mobile arm at an incorrect angle ψ' such that ψ' = (ϕ' – δ), where ϕ' is the supposed latitude and ϕ the correct latitude. In Fig. 24 we see that the incorrect inclination brings with it a displacement of the direction in which the gnomon points from *K* to *K'*. As a result one will read off a false angle ζ' on the read-off scale, and this angle has to be compared to the Sun's seasonal horary angle *H'* for the locality ϕ. This angle is:

$$(39) \qquad \cos\zeta' = \frac{\sin\phi\sin\delta + \cos\phi\cos\delta\cos H}{\cos\psi'}$$

In this formula, the denominator corresponds to the part set manually by the user. There are two kinds of configuration for latitude error: either the supposed latitude ϕ' is greater than the correct latitude ϕ, or it is less.

(1) ϕ > ϕ'. It is in the neighborhood of the meridian that the error is most important—in fact considerable—and the *maximum maximorum* takes place on the winter solstice (graph 3). Right at the meridian the false angle ζ' becomes:

$$(40) \qquad \cos\zeta' = \frac{\cos(\phi - \delta)}{\cos(\phi' - \delta)}$$

Fig. 25 displays the error H' – ζ' for the afternoon (the error for the morning is symmetrical but with opposite sign) when one is off by 1° in latitude (in this case the user is at 40° latitude but sets his dial for 39°). One can clearly see that between seasonal 6 h and 7 h (and hence also between 5 h and 6 h) the error is huge but decreases very quickly in such a way that one soon

Mean dates	*1st century*	*2nd century*	*3rd century*	*4th century*	*5th century*	*6th century*
Vernal equinox	March 22	March 21	March 21	March 20	March 19	March 18
Summer solstice	June 24	June 23	June 22	June 22	June 21	June 20
Autumnal equinox	Sept. 25	Sept. 24	Sept. 23	Sept. 22	Sept. 22	Sept. 21
Winter solstice	Dec. 22	Dec. 22	Dec. 21	Dec. 20	Dec. 19	Dec. 19

Table 5. Shift of dates of solstices and equinoxes in the Julian calendar.

is within the zone where the error is in the neighborhood of −10 minutes. One should note that the user can never observe seasonal 6 h (solar noon) on the dial, even by placing it in the plane of the Sun; at this moment, instead of pointing due east ($D = -90°$), the gnomon is off by several degrees (in summer the deviation can significantly exceed 20°). But since the shadow covers the read-off scale completely, the user cannot realize that he is committing an error in believing that he is before or after seasonal 6 h.

(2) $\phi < \phi'$. In the case where the latitude to which the sundial is set is greater than the correct latitude, there results a zone of hiatus in the readability of the time since it becomes impossible to use the dial in the neighborhood of seasonal 6 h (graph 4). In fact, if the mobile arm of the dial is set such that its inclination is less than the actual noon altitude, one cannot read off the time when the Sun has an altitude greater than $(90° - \psi')$, that is, when $(\cos \zeta' > 1)$.

In practice, this means that at a certain moment during the day, the user, no matter what how he turns his dial about, will never be able to bring the gnomon's shadow upon the read-off scale. This impossibility of making a reading, unlike the preceding case, allows him to grasp that there is a problem with the setting of the mobile arm.

Here again (see Fig. 26, where the error in latitude is 1°), it is at the winter solstice that the greatest deviations result between H' et ζ', and one notes that the error decreases more slowly than in the case where $\phi > \phi'$. Moreover, the dial is unusable for more than two seasonal hours during the day for a latitude around 40°.

One can take consideration of the error that one commits by using an incorrect orientation of the dial; but since the user is required to orient his dial so that the gnomon's shadow falls on the scale of hours—otherwise one cannot read off the time—the orientation is not a significant cause of error.

On the other hand, there is another source of error that has to be taken into account, namely the error in the date. The majority of examplars of the universal portable sundial bear a scale of dates, bounded by the two solstices, with the intermediate graduations corresponding to the entries of the Sun into the zodiacal signs.[27] Let us note in passing that the sole function of the symmetrical scale of dates that one finds on the exemplars is to refine the setting by means of a point situated at the tip of the read-off scale.

The solstices are generally placed on the 8th days before the Kalends of January and July, that is, respectively December 25 and June 24.[28] The indicated solstice dates are the classical dates according to the Julian calendar, which came into use in 45 BC. As for the equinoxes, they are set on

27 On this point see P. Brind'Amour, *Le calendrier romain*, Université d'Ottawa, 1983, p. 15-19.

28 F. K. Ginzel, *Handbuch der Mathematischen und Technischen Chronologie*, *op. cit*., p. 179-181 et p. 282.

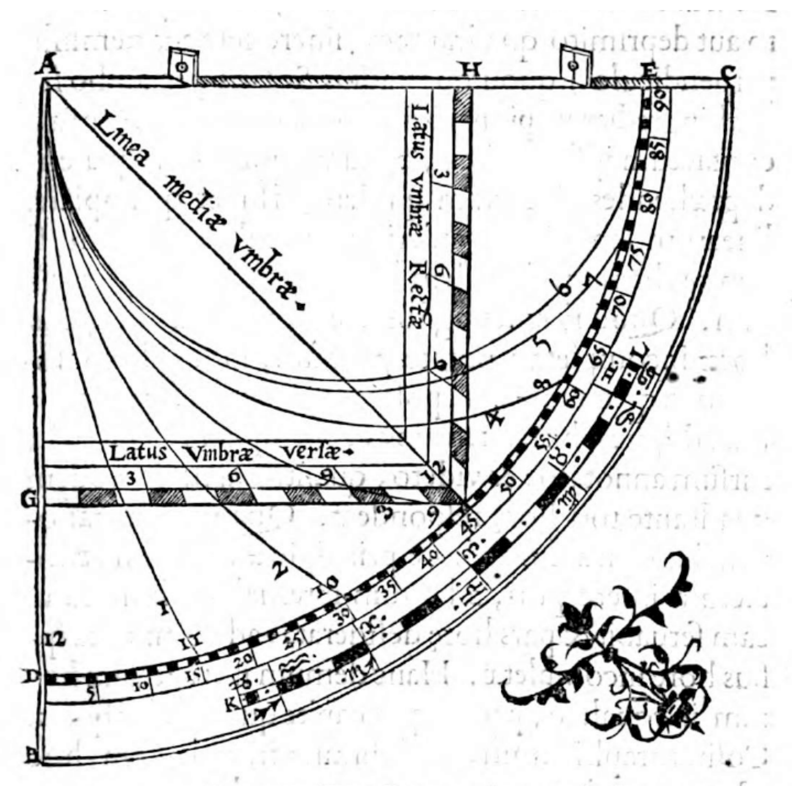

Figure 27. Illustration of the quadrans vetus from Orontius Finnaeus, *De solaribus horologiis et quadrantibus*, Paris, 1560, Book II, p. 143. On this ancient quadrant can be seen the sighting system consisting of pinnules, and the limb graduated in 90° with a dentate scale. Below the limb, the author has added a zodiacal calendar establishing the correspondence between the days of the year and the degrees of the zodiac occupied by the Sun on each day. Six circular arcs meeting at the top represent the hour curves.

March 25 (8th day before the Kalends of April) and September 24 (8th day before the Kalends of October). But because of the drift of the Julian year relative to the tropical year, the astronomical seasons underwent a shift as tabulated in Table 5 for the period from the 1st century through the end of the 6th century[29]

In addition to this shift of the seasons there is a slow diminution of the obliquity of the ecliptic, which changed from 23° 41' in the 1st century to 23° 37' in the 6th century. This has the effect of significantly modifying the inclination of the mobile arm at the solstices, and given that the dials were engraved with an obliquity of 24°, the error ends up being a little over 0° 20' in the 6th century. Making a bad estimate of the date (or of the obliquity) on this type of portable dial amounts to applying an erroneous solar declination, and it is easy to see that this reduces to the same kind of error as introducing a bad latitude: the angle ψ' here becomes $(\phi - \delta')$ where δ' is the incorrect solar declination. Since the declination varies very little around the solstices, getting the date wrong by several days is inconsequential, in particular since the graduations of the dials do not allow a setting to the precise day! For example, a 6th century user who thinks that the summer solstice falls always on June 24 is committing an error of 0° 3', which is not

29 D. Savoie, “Les dates des quatre saisons,” *Observations et Travaux*, n° 19, 1989, p. 3-6. The mean dates (largest number of occurrences of the date by century) in the present table were calculated in UT according to the algorithms given by J. Meeus, *Astronomical Algorithms*, Willmann-Bell, Richmond, USA, 1998.

perceptible. On the contrary, the same user, if he thinks that the vernal equinox falls on March 25 rather than March 18, commits an error of close to 2° 30' in the declination, and this does become perceptible.

As has already been said, the dial is perfectly correct only on the equinoxes, and its error is absolutely imperceptible for at least a month before and after the equinox. Once again, our modern conception of the idea of precision for such a dial needs to be put in perspective. It would appear logical to compare the seasonal time indicated by the portable dial to that which would be indicated on an ancient dial that has been perfectly drawn and is in a functioning state, which plays the role of a kind of control-clock. In reality this appears unrealistic for many reasons. The first is tied to the physical difficulty of making such an error apparent, since ancient Greco-Roman sundials, with rare exceptions, lack any subdivision of the hour. This means that to know the time when the gnomon's shadow falls between two hour lines on the control-clock, one has to make an estimation that one cannot guarantee to be accurate to within 5 or 10 minutes. Hence unless the maximum error of the portable dial falls, by a felicitous stroke of luck, on a round hour number, it is practically certain that an error of 7 or 9 minutes is impossible to make visible. What is more, how can one be certain that the error does not arise from a bad setting in latitude or date rather than from the conceptual basis of the dial? In fact it is legitimate to ask whether the inventor or inventors of this portable dial were conscious that it was, in an ideal sense, false.

We should thus seriously consider the possibility that this dial was considered in antiquity to be perfectly exact. For lack of documents, one cannot know whether its invention resulted from a theoretical investigation or by chance or from a combination of the two. But if one recalls that it is not a precise instrument for time measurement, one has to conclude that it is doubtless the best candidate for being the famous πρὸς πᾶν κλίμα of which Vitruvius speaks,[30] and that it fulfilled in a very satisfactory way its mission of giving a good notion of the time while traveling over a vast extent of latitude. It constitutes one of the very are examples of sundials that sank into obvlivion when unequal hours progressively gave way to equinoctial hours.

The dial can be compared—without presuming *a priori* a line of descent—to another later instrument of Arabic or rather Persian origin,[31] the "ancient quadrant" (*quadrans vetus*), one of the earliest references to which goes back to Hermann le Boiteux (1013-1054),[32] and which was diffused in Europe especially thanks to Master Robert Anglès (*Robertus Anglicus*)[33] and Sacrobosco[34] (Fig. 27). This consists simply of an adaptation of a diagram of unequal hours—a grid that was in

30 Vitruvius, *De Architectura*, Book IX, chap. VIII, 1, gives a description of sundials, attributing them to inventors, and speaks of a dial "for all latitudes" (in Greek in the text). See the translation by J. Soubiran, Les Belles Lettres, Paris, 1969, p. 30.

31 See D. King, "A *Vetustissimus* Arabic treatise on the *Quadrans vetus*," *Journal for the History of Astronomy*, xxxiii, 2002, p. 237-255 et F. Charette, *Mathematical instrumentation in fourteenth-century Egypt and Syria*, Brill, Leiden-Boston, 2003, p. 211-215.

32 Study of the texts of this period, including the one allegedly by Hermann le Boiteux, enabled J.–M. Millas Vallicrosa to demonstrate that there actually existed two types of *quadrans* : the *vetustissimus*, which was older than the *vetus*. See the important study by J.–M. Millas Vallicrosa, "La introduccion del cuadrante con cursor en Europa," *Isis*, t. XVII, 1932, p. 218-258.

33 The Latin and Greek text were edited by P. Tannery, *Le Quadrant de Maître Robert Anglès*, (Montpellier, XIII$^{\text{è}}$ siècle), *Notices et extraits des manuscrits de la Bibliothèque Nationale*, Paris, t. 35, 1897, 2$^{\text{è}}$ partie, p. 561-640. See, however, the remarks and criticisms by W. R. Knorr (*cf. infra*).

34 One should consult the fundamental study by W. R. Knorr, "The Latin Sources of *Quadrans vetus* and What They Imply for Its Authorship and Date," *Texts and Contexts in Ancient and Medieval Science : studies on the occasion of John E. Murdoch's seventieth birthday*, E. Sylla and M. McVaugh, Leiden, New York, Köln, Brill, 1997, p. 23-67.

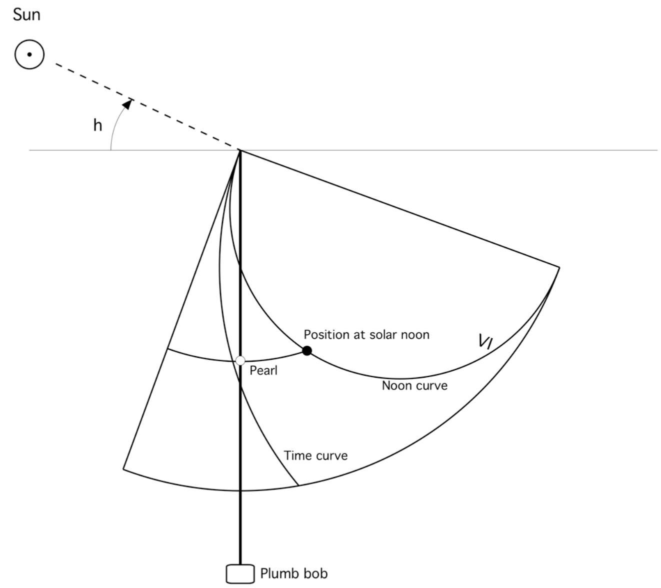

Figure 28. Principle of setting and use of the *quadrans vetus*. Knowing the latitude of the locality and the Sun's declination, one calculates the Sun's noon altitude (90° – latitude + declination), which one locates on the graduated limb. One now extends the plumb line, which cuts the circle for unequal hour VI at a point that one finds by means of a sliding bead. When this setting is complete, one sights the Sun; the position of the bead enables one to read off the unequal hour on the network of curves.

wide use notably on the backs of planispheric astrolabes—to a sighting system complemented by a weighted thread. Otherwise put, the ancient quadrant is a "universal" altitude sundial since from observation of the altitude of the Sun one deduces the unequal hour, no matter what date and latitude of the locality.

This *quadrans vetus* presents remarkable properties which call to mind those of the ancient universal portable dial: it is not rigorously exact except at the equinoxes,[35] it has to be adjusted to the noon altitude of the day, its equinoctial path is valid at (nearly) all latitudes, and it indicates the seasonal hour subject to exactly the same approximation as the ancient universal portable dial.

Let us briefly describe this sundial. It is a quadrant of a circle bearing a sighting system (pinnules on one of its sides, a plumb line fixed to the point of convergence of the hour lines, graduation of the limb in 90°), six circular arcs meeting at the instrument's top and constituting hour lines that divide the limb into six sectors of 15°. In the 12th century, a zodiacal calendar was added in the form of a mobile circular sector along the limb, allowing the solar noon altitude to be obtained as a function of the date; this is what was named the cursor. It served as a kind of table of solar declination.

On the day of observation, one calculates the altitude of the Sun's culmination at the place of observation, which one finds (for example by means of a bead sliding along the plumb line) on the line for seasonal hour VI; then one sights the Sun by holding the quadrant vertically. The entire dial is tilted, except of course for the plumb line. The sliding bead will now show the seasonal hour (Fig. 28).

35 This point did not escape J.–B. Delambre, *Histoire de l'Astronomie du Moyen Age*, Paris, 1819, p. 243-247. J. Drecker approaches the problem but in a more obscure manner in *Die Theorie der Sonnenuhren*, *op. cit.*, p. 86-89. See also R. D'Hollander, *L'Astrolabe*, *Histoire, Théorie, Pratique*, *op. cit.*, p. 213-216.

H	*T*	*T'*	*Error in minutes T – T'*
0°	0°	0°	0
15°	11.443°	12.364°	3.7
30°	22.886°	24.659°	7.1
45°	34.329°	36.810°	9.9
60°	45.773°	48.727°	11.8
75°	57.216°	60.299°	12.3
90°	68.659°	71.379°	10.9
105°	80.102°	81.771°	6.7

Table 6. Calculation of error of time determinations on the *quadrans vetus* on the summer solstice, latitude 47° 15'.

It is easy to show by plane trigonometry that the angle that one reads off on the dial is not the seasonal horary angle *T* but a different angle—let us call it *T'*—whose formula is:

$$(41) \qquad \cos T' = \frac{\sin h}{\sin h_m}$$

where *h* is the Sun's altitude and h_m is its noon altitude, that is, 90° – ϕ + δ. Hence we have:

$$(42) \qquad \cos T' = \frac{\sin h}{\cos(\phi - \delta)}$$

or, again,

$$(43) \qquad \cos T' = \frac{\sin\phi \sin\delta + \cos\phi \cos\delta \cos H}{\cos(\phi - \delta)}$$

One sees at once that the expression for *T'* is the same as that for ζ in the ancient universal portable sundial. If for example one calculates the error committed on the summer solstice (δ = +23° 26') on a *quadrans vetus* calculated for the University of Puget Sound (ϕ = 47° 15' 44'') one obtains the data tabulated in Table 6.

It goes without saying that, by way of contrast, the difference (*H* – *T'*), that is, the equinoctial hour angle minus the "hour angle" of the dial yields considerable deviations: one cannot read off the equinoctial hour on such a sundial. Here one meets again one of the particularities of the ancient universal portable dial, with the difference that one could have adapted the *quadrans vetus* to equinoctial hours, but at the cost of suppressing its "universality," since such a sundial depends on the locality's latitude;[36] the hour lines remain circular arcs but they do not converge at the angle of the quadrant. Moreover, the cursor no longer has real meaning since its purpose is to render the dial usable universally.

36 The procedure for constructing a quadrant for equal hours is given, for example, by Jean Fusoris (1365-1436) : see E. Poulle, *Un constructeur d'instruments astronomiques au XVè siècle : Jean Fusoris*, H. Champion, Paris, 1963, p. 71-73. A good example of a quadrant for equal hours (and unequal hours) may be found in Orontius Finnaeus, *De solaribus horologiis et quadrantibus*, Paris, 1560, Book II, p. 151.

While we wait for future archeological excavations to provide us with new portable sundials from antiquity—perhaps of a new type—we should recall the intent of these "universal" portable dials. They are witnesses first of all of remarkable originality and imagination in gnomonics, even if the principle on which they are based is relatively simple: to determine the time from the Sun's altitude. There are few scientific instruments that made it possible to deploy so many clever devices, for which there was need of a collaboratoin of applied technology, trigonometry, and geometry. The Middle Ages and the Renaissance would continue along this route opened by the gnomonists of antiquity, with dials in which esthetics assumed a major role, as in the *Navicula*, the *Capucin*, and the "Universal" of Regiomontanus. More surprisingly, one finds again even in the Age of Enlightenment this tendency to conceive of new universal portable sundials; it suffices to consult the *Supplément à l'Encyclopédie* [of Diderot and D'Alembert] and its superb plates,[37] where the author describes unpublished portable dials[38] some of which were due to the German mathematician Johann Heinrich Lambert (1728-1777).

Did these ancient universal dials serve in the "real world" to determine the time? Emmanuel Poulle, connoisseur of medieval astronomy, concluded bluntly that astronomical instruments (astrolabes, *quadrans vetus*, ...) constituted a pedagogical resource and incidentally a tool for calculation, but in no way a resource for observation. I absolutely share this view and I think that the same applies to the majority of ancient portable dials: they are objects of prestige and curiosity. As I have said above, their small dimensions make the read-off of the hour very difficult, not to say impossible; the crudeness of the drawing in the case of some of them, and in the case of others the uncertainness or inexactness of the locality where they were used and the shift in the dates of the seasons in the Julian calendar—all this makes highly improbable any precise determination of the time. Doubtless there would be much to say about this concept of "precision" in antiquity and even in the Middle Ages, and it is a safe bet that we impose modern concepts on these portable sundials that are totally anachronistic so far as concern the results that the ancients expected to obtain.[39]

37 *Supplément à l'Encyclopédie ou Dictionnaire Raisonné des Sciences, des Arts et des Métiers*, t. 2, Amsterdam, 1776, p. 97-106.

38 See Y. Massé, *De l'analemme aux cadrans de hauteur*, 2009, available from the author: 2 ruelle de la Ravine 95300 Pontoise, France.

39 Having executed an exemplar of the ancient universal sundial of 19 cm diameter in wood, I can confirm that it would yield the unequal hour with very good accuracy. However, none of the dials that have come down to us reach these dimensions; of the eleven known examples, half are 6 cm in diameter and the other half 11 cm.

Buchner's findings at the Horologium[1]

Karlheinz Schaldach

My considerations are to be understood as a throw-in into the last debate on the Horologium Augusti initiated by L. Haselberger, and I would like to limit myself here to only two aspects.[2] First, I ask the question, which has hardly been touched upon in the debate, namely how great the Augustan involvement was in the Flavian meridian. Finally, I will present a surprising hypothesis, which is based on the reflection that for the Augustan period the northern Campus Martius is not to be regarded as level, just as E. Buchner has always supposed. The assumption of a flat Campus Martius has led to misinterpretations in my opinion.

How great is the Augustan involvement in the Flavian meridian line?

It is undisputed that the Flavian instrument was a large meridian of which only fragments have survived. By comparison, with regard to the Augustan Horologium, despite recent contradictory views concerning it, Buchner has espoused that it was not a meridian, but a colossal sundial.[3] This is necessary to understand Buchner's arguments presented below.

In Haselberger's debate, possible correlations between the Augustan and the Flavian instrument were mentioned almost not at all. The reason is a view not doubted by scholars, as Haselberger writes, that "This Flavian clock (hitherto unknown) cannot be identical with the instrument described by Pliny."[4] M. Schütz, however, confesses at least that one must assume certain resemblances to the Augustan Horologium without pushing this further, and only for G. Alföldy is it quite undoubted that the find is related to the Solarium Augusti.[5]

Apparently P. Heslin presented his theory of a complete new Flavian meridian so convincingly that it is not challenged in the debate.[6] Before I turn to his reflections, I would like to sketch out briefly the initial situation and Buchner's arguments.

1 I am glad to deliver for Jim Evans this small contribution which was written in 2012 and formed one part of my talk "Three exceptional Horologia" given at 29 March 2013 in Lille at a conference entitled "Horologia et Solaria. Instrumentaliser le temps à l'époque romaine". Another part was published under "The Globe Dial of Prosymna," Bulletin of the British Sundial Society 25(iii) (2013), 6–12 (together with Ortwin Feustel), and a third was issued in H. Kienast, *Der Turm der Winde in Athen*, Wiesbaden 2014, 197–226.

2 See L. Haselberger, "A debate on the Horologium of Augustus: controversy and clarifications", *JRA* 24, 2011, 47–73 with responses by P. J. Heslin, 74–77, and M. Schütz, 78–86, and additional remarks by R. Hannah, 87–95, and G. Alföldy, 96–98. I thank Robert Hannah and Lothar Haselberger for debates about the issues raised here, though Haselberger, as I must confess, did not value some of my arguments presented in that paper.

3 Horologium was understood in Roman antiquity as a generic term for all kinds of time-measuring instruments. For this reason, I share the view of Heslin 2011, 77, that the term implies that the instrument was a clock, but also, that it was a meridian line.

4 Haselberger 2011, 56.

5 Schütz 2011, 78; Alföldy 2011, 96, who mentions the lettering of the Solarium Augusti as if it is identical with the lettering found on the meridian line.

6 P. J. Heslin, "Augustus, Domitian and the so-called Horologium Augusti", *JRS* 97, 2007, 1-20, here 9–10.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 79–86. Chapter DOI: 10.5281/zenodo.3975721.

The most important evidence that the meridian comes from the Flavian period is the layer in which it was found.[7] So it is clear that in the Flavian period renovations were carried out on the Augustan meridian. But how far did these go? The question is of importance for this reason, because Buchner, despite extensive digging and coring, did not obtain a reliable result from the original installation.

There are some possible solutions for the riddle of the disappeared Augustus meridian. One is that the Augustan Horologium still lies underground, but somewhere else. Buchner, but none of the other scholars, has assumed that. Heslin has pointed out that one is able to explain the Flavian renovation measure against the backdrop that an emperor of that time, especially Domitian, was perceived as guardian and renovator of the Augustan building projects.[8] So with the renovation of the meridian they will not have interfered with the iconic nature of the Augustan building program and therefore will have left the meridian on the spot where Augustus put it and linked it with the other buildings of the Campus Martius.[9] That means that the Augustan meridian lays where Buchner supposed it did and at no other place underground, only probably deeper.[10]

According to Buchner, the Augustan tiles were removed in order to reuse them with the new Horologium. Buchner wrote, however, not explicitly of a reuse, therefore the passage in which he points it out is easy to overlook, but he spoke of a "new layout" of the Augustan Horologium and that "also the obelisk" had a "new position on a level of 10.80 NN".[11] This *also* includes with it the meridian line and the "new position" means lifting it up without an alteration or even destruction.[12]

He expressly distanced himself here from his original version, in which he had also foreseen a reuse of the tiles, but in the reverse position, whereupon bronze lines had been placed with intervals decreasing a little, because through the obelisk remaining in its original position and by the uplifting of the tiles the working height of the gnomon would have become smaller.[13] The

7 Haselberger 2011, 56.

8 For Vespasian speaks the cippus from AD 75 found west of the Horologium, which was almost at the same height level as the renovated meridian line, see E. Buchner, *LTUR ITT* (Rome), 35–37, and Haselberger 2011, 57, while E. Buchner, "Solarium Augusti and Ara Pacis," *RömMitt* 83, 1976, 319–365, here 362, and Heslin 2007, 16–19, favoured Domitian and his connection to the Augustan program of power. Above all, it was Domitian who wanted to distinguish himself as a calendar reformer and he renamed September and October Germanicus and Domitianus (Sueton 13.3).

9 See in particular Heslin, 2007, 16, n. 79.

10 Probably, because Buchner states that the levels for the Augustan foundation are between 8.65 NN (1996) and 8.91 NN ("Horologium Solarium Augusti. Bericht über die Ausgrabungen 1979/80", *RömMitt* 87, 1980, 355–373, here 361), without providing any further details. NN means something like MSL (Mean Sea Level), so 8.65 NN means 8.65 m above MSL.

11 Buchner 1994, 81, and 1996a, 36.

12 The addition of "without an alteration or destruction" is a compelling conclusion, which Buchner himself did not draw. It is pointless to ask why he did not. It is typical that he, in his articles that have been published after the essay by Schütz on the question of the maintenance or destruction, communicated his ideas only shortly, withdrawed from any conclusion and left it to the reader. Another example is Buchner 1996, 36, where he made the important statement that the Flavian Horologium was a meridian line only in a subordinate clause and with the words "reduced to a stripe." I should add, however, that Buchners text is always in German, and the translation given is always mine.

13 Compare Buchner 1980, 364 for the decreasing, 368 for the uplifting and 372 for keeping the obelisk in its original position.

new solution now offered the advantage that not only the tiles but the meridian line could be re-used in their entirety.[14]

Buchner's new suggestion, however, is unlikely because laying down the obelisk and setting it up again at almost the same place would mean a complicated and considerable expenditure at that time.[15] Such a measure is only then entered upon, if no other simpler solution is offered.

Is there such a solution? Was it actually simpler to carry away the Augustan stones, to dispose of them and to set up a complete new layout, with new stones and a new meridian line? Was it absolutely essential or was one not able to reuse Augustus' layout differently, perhaps even preserve it differently? These and other questions are raised, when one rejects Buchner's explanations.

But Heslin when he comments Buchner's ideas does not enter into such questions at all, perhaps because he wrongly understands Buchner's intention. He writes namely that "destroying the Augustan pavement and relaying the instrument in a reduced form… is the approach Buchner now takes."[16] One can in fact interpret Buchner's statements with regard to the area right and left of the meridian, though Buchner did not speak of destruction at any point, but it is incorrect to apply his approach to the meridian line for the reasons mentioned above.[17]

But Heslin, acting as if he agreed with Buchner on this issue, turns Buchner's intention into its opposite, when he writes that the Augustan tiles have not survived because they were destroyed, or that in the Flavian period they constructed an entirely new meridian.[18]

But suppose it was as Heslin claims that the old layout was destroyed under Domitian and as an innovator he probably "remodelled" it. What then is one to ask, what idea, what feature of the new instrument was actually introduced by Domitian? Heslin thinks that Domitian "did not dramatically change its nature,"[19] without however elaborating further. I would like, therefore, to draw attention to a few points about why the instrument misses any special Flavian influence.

First, from the inscription no definite date has been derived. It could have belonged to the Flavian period as well as the Augustan.[20] It is, as already pointed out, a Greek and not a Latin inscription, as one might have expected.[21] But that a Flavian ruler should have decorated a monument with a Greek inscription is strange. Besides the scientific orientation of the instrument, which was helpful for Augustus and his calendar reform, there is a second subject to mention: the meridian and obelisk are to be seen as uniform booty from Alexandria, at that time the

14 In the supplement of E. Buchner, *Die Sonnenuhr des Augustus* (Mainz 1982) 80, he writes of the Augustan bronze letters that they had been separated out and re-used. With the new solution the letters could be reused alongside the bronze lines without alteration.

15 See the difficult erection of the Vatican's obelisk, described by D. Fontana, *Della Trasportatione dell'Obelisca Vaticano et delle Fabriche di nostro Signore Papa Sisto V* (Roma 1590). Add to that the establishment of a new foundation of our obelisk next to the old one, in order to realise exactly Buchner's reasoning, although, Pliny *NH* 36.73 wrote that "the foundation reached so deep in the soil, as the obelisk was high", see also Heslin 2007, 13.

16 Heslin 2007, 9.

17 The misunderstanding continues when Heslin writes quite generally and without a differentiation thereof that Buchner, after he had initially considered a reuse of the Augustan tiles, later described the pavement of the meridian as Flavian, see Heslin 2007, 8.

18 Heslin 2007, 16: "A new pavement was therefore laid that made the readings accurate once more."

19 Heslin 2007, 10.

20 On the position in time of the letters, see Buchner 1982, 80.

21 In A. M. Bandini, *De obelisco Caesaris Augusti e Campi Martii ruderibus nuper eruto,* Rome 1750, the meridian line (Tab. IV Fig. I) was labelled in Latin.

centre of Greek science.[22] Moreover, the meridian displays no Flavian influence: Domitian, for instance, could have made clear his calendar efforts by adding at least his new month to the calendar, but this did not happen.[23]

What is to be made of all this: a missing Augustan meridian and a Flavian one, which looks as if it were Augustan? Was Buchner perhaps right with his assertion that the whole layout had been raised by about 1.5 metres? Actually there is a much simpler solution, which has so far not been considered at all.

A reevaluation of the architectural layout of Augustus Horologium and the Flavian renovation

Buchner estimates a horizontal surface of 9.10 to 9.45 NN for the Augustan layout, because it runs out from the foundation height of the Ara Pacis and he treats the Campus Martius as a horizontal surface, on which they were able to install a huge horizontal dial. But now it is doubted if one should really proceed from a level Campus Martius. Buchner himself recently worked from this idea and said from it that one must consider a lowering of the Augustan line network compared with the level of the Ara Pacis by 40 cm.[24]

But I would like to go further. The northern Campus Martius for its part was only loosely deposited and had unstable alluvial soil which was difficult to secure and to strengthen permanently. Any effort in this respect would have been nullified again with the next major flood. This would have also been taken into consideration in the planning of Augustan buildings: the elevated position of the Ara Pacis, to which one had to climb up a staircase from the Campus Martius, provides an example of how they wanted to protect themselves against flooding.[25] This makes an unsecured, large flat sundial unlikely.[26]

But if one frees himself from the idea of a colossal sundial, a new possibility comes into focus, which is illustrated in Fig. 1. The proposition is based on Haselberger's value for an elevated

22 The booty is comparable with the first sundial that Valerius Messala brought to Rome in 262 BC and had set up in the forum (Pliny *NH* 7.213–215; Cens. 22, 7); T. Gesztelyi, "Alexandria in Rom," *ActaClDebrec* 38–39 (2002-3), 65–70, 68, has accepted that "even the network of lines, or at least its most important parts" were taken from Alexandria to Rome, for him an explanation of why the device was not correct. I think that one can rule that out, but in order to prevent reading errors as happened with the establishment of the first sundial in Rome, one corrected the division of the meridian in accordance with the local latitude. Pliny is clear that at the latest since the year 164 BC one was familiar with the local latitude function of- dials. Consistent with that, Pliny did not now give human errors as a reason for why the reading of the obelisk was not at all exact, but the influences of nature, see A. Wolkenhauer, *Sonne und Mond, Kalender und Uhr* (Berlin 2011), 255–6.

23 The excavation revealed 11° of Virgo, which includes the first days of September. But a reference to the month Germanicus for September is absent.

24 Buchner 1996, 36; in E. Buchner, "Neues zur Sonnenuhr des Augustus," *Nürnberger Blätter zur Archäologie* 10, 1994, 77-84, here 81, it is "ca. 30 cm."

25 Buchner 1994, 83, speaks only of safeguarding the obelisk "against sinking into the soft alluvial land." But just as important were measures against inundation. Such a simple measure at the obelisk could be a higher setting of the bench; with the Ara Pacis , however, only a 2 metre high retaining wall helped, see F. Rakob, "Die Urbanisierung des nördlichen Marsfeldes. Neue Forschungen im Areal des Horologium Augusti," *L'Urbs. Espace urbain et histoire* (CoIIEFR 98, 1987) 687-712, here 700.

26 Rakob 1987, 696, note 29, therefore, says also, at least with the Flavian Horologium, that possibly because of changes to the terrain as a result of flooding, this must have been reduced in its largeness. Also not to be ruled out should be "a sink area west of the Augustan square and its shift to the east."

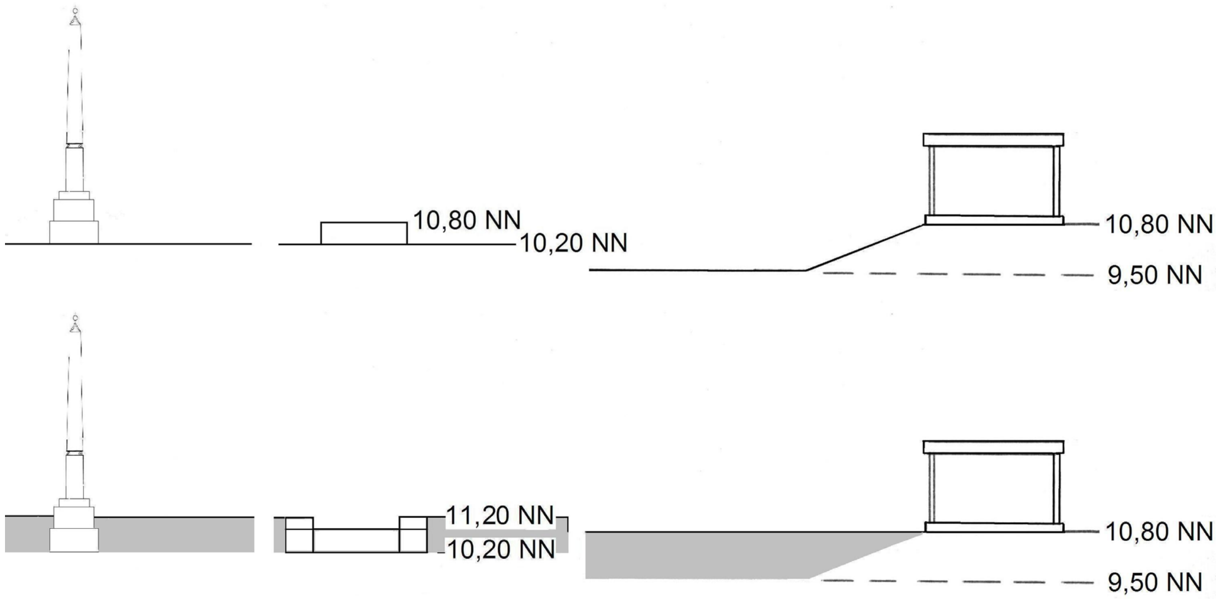

Figure 1. Meridian line in Augustan (top) and in Flavian time (below). Author's drawings, not to scale.

Augustan foundation in the vicinity of the obelisk (10.20 NN).[27] The data on the Ara Pacis comes from F. Rakob.[28] A peculiarity is that the Augustan meridian lies on the ground up to a level of 10.80 NN (Fig. 1, top). The height difference between the Augustan and the Flavian horizon of about 60 centimetres agrees with the statement in Buchner, when he says, "the blocks of the 'Domitian' dial are 40 centimetres thick and rest on a foundation of 20-30 centimetres."[29] This obtains a striking agreement with the horizontal surface of the Ara Pacis, which suggests that both measures form the basis of a common design concept.

Not only the elevation data, but also the Horologia themselves, suggest this interpretation. For in antiquity they were usually independent, self-standing objects and only in the late Roman period and even later then in Byzantine times or the Islamic Golden Age they were integrated also into the ground, a wall or a general building structure.[30] Typical examples are shown in Fig. 2 and 3: a Roman raised dial on a column at Palmyra and an integrated column dial from the Islamic Age.[31] In addition, a raised position of the meridian had a particular signal effect and helped to emphasise the midday shadow casting.

How must one now consider the Flavian renovation? Important in this context is a comment by Buchner, which was made clear for the first time by Haselberger: "As for the meridian's

27 Haselberger 2011, 61-62 and Fig. 10.

28 Rakob 1987, Fig. 7.

29 Buchner 1982, 78, or 1980, 358: "57 cm between the top edge of the foundation and the Augustan level."

30 The first sundials were, with the Tower of the Winds, drawn or respectively painted on a wall, as is usual today. The peculiarity of the Tower is comprehensible if one recognizes it not as a building but as a huge multi-faced sundial.

31 Also the two hitherto known vertical meridian lines in Miletus and Chios, see K. Schaldach, "Eine seltene Form antiker Sonnenuhren: Der Meridian von Chios," *ArchKorr* 41 (2011), 73-83, have not been worked into walls or floors, but into free standing stones. The large horizontal dials worked into the pavement of Thamugadi, today Timgad, and in Lambaesis (Tazzoult-Lambèse) , the headquartes of the Legio Tertia Augusta under Hadrian, see A. Guerbabi, *Atti del x Convegno di Studio Sassari* 1, 1994, 359-402, both dating from a later period.

Figure 2. Sundial in Palmyra, at a column erected in 64 for Shalamallat, son of Yarhibola (author's photograph).

original setting, the different surface treatment of the pavement is telling: while the broader middle stripe with its bronze insertions shows a smooth surface, the two lateral stripes feature roughly-carved surfaces and open-lying clamps, a clear testimony that these lateral stripes were originally covered by at least one more layer of stone, about 1.50 metres in width. *Thus in its original state the pavement was flanked by broad (and shadow casting) boarder walls*" (Italics mine).[32] Buchner shies away from the consequences that result therefrom. He therefore formulates this in the subjunctive: "stone blocks would have actually been on both sides of a cultured, elevated stripe, the shadow edges were cast out to the west of it in the morning and to the east side in the afternoon; only at the moment of day's highest solar altitude, at midday, would a shadow have been cast out on neither side."[33]

One must know that Buchner, when he writes this, is still proceeding from a colossal Flavian sundial. A layer of blocks on either side of the meridian would fit poorly with this, for the midday line would be isolated from the other parts of the sundial and lines would have been obscured or shaded. All the same, he admits that the meridian was nestled between blocks, which he dismisses as seating.[34] But they were present *as broad boarder walls*, for it is not credible that the structure framing the meridian had the sole task to serve as a bench.[35] More practical considerations are suspected: especially that the wall helped as a barrier against further masses of earth. Moreover, the play of light—as with the elevated meridian—was in addition carried, when at midday only the shadow of the obelisk with its sphere fell in the pool between the tiles, whereas at other times of day the curb stones cast a shadow on the midday line.[36]

The Flavian wall was presumably not a boundary layer, which separated the meridian from the rest of the Campus Martius, but part of an earth structure, which bordered it basin-shaped,

32 Haselberger 2011, 55, see also Fig. 7.

33 Buchner 1980, 365.

34 Buchner 1980, 365.

35 See Haselberger 2011, 55.

36 Schütz 2011, 80, therefore agrees when he says: "Thus, true noon could be ascertained with high accuracy."

so that it was lowered compared to the level of the surroundings. This is supported by the remnants of a water basin, which dates from the time of Hadrian or even later.[37] This basin, which was built up over the meridian, follows its lines closely. But, we must ask, why one came exactly here, to build such a pool, unless the previous structure had suggested such an idea?[38]

The height of the Flavian wall can only be estimated. The top of the meridian lies at 10.80 NN, in addition taking into account a wall, the view to the meridian from the altitude of the bench at the obelisk might possibly have been obscured. It could therefore be that the second seat at the so-called Hadrian height of 11.66 NN dates from the Flavian period, with which then a wall height of up to 0.86 metres is conceivable. On the other hand, Rakob gives block heights of 30 centimetres, which can be regarded as a lower limit for the height of the wall.[39] All values between 30 centimetres and 86 centimetres are thus feasible, but a higher wall is to be preferred, should it have endured for several years. In the drawing (Fig. 1, below) the lower limit value was chosen.

A look at the unglazed verges on both sides of the meridian line confirms this solution: they look like they were pushed out, for the transverse edges show no relation to the corresponding

Figure 3. Sundial of Ahmad al-Halabi (†1455), chiselled into a column of the Dome of the Treasury in the Umayyad Mosque of Damascus (author's photograph).

37 For summary, Haselberger 2011, 55.

38 Thus already Buchner 1994, 81.

39 Rakob 1987, n. 19.

edges of the meridian.[40] The overhanging blocks must have then, in the time of Hadrian, in the course of the rearrangement and enlargement of a water basin, been removed and were probably incorporated into the new wall.[41]

Conclusion

These reflections show that the findings published by Buchner can also be interpreted differently. If it was a meridian line that Augustus set up on the Northern Campus Martius and if the whole area was not as plain as Buchner claimed we have to take into consideration that the piece which was laid open is nothing else than a part of the original Augustan Horologium just as Pliny described it, what means as well that there was no other lay-out at a lower level.

The interpretation is consistent with definite data of the excavation, with the formulation of Pliny and it always takes into consideration that a mass of earth was held off from the line.[42] With it Buchner's reconstruction regarding the complex new laying of the meridian and the obelisk is obsolete. But above all it means nothing more than that Buchner actually found the Augustan meridian line, but he therefore did not want to admit it, because he had started out from a colossal horizontal dial.

Against the hypothesis of the unaltered meridian is only the recovery of post-Augustan pottery underneath of the find. From the data no indication is given, whether the pottery was found underneath of the midday line or in the area just beside. But due to my new interpretation, the stones of that area were added only in the Flavian period.

Unfortunately we have for the present no possibility to decide if the new interpretation weighs the conditions more precise than the common view as there is a conflict of law at the moment between the owner of the house where the meridian line is situated and the German Archaeological Institute in Rome. Therefore it is not possible now to make available new informations, for instance a check if my new interpretation is right. This would be the case if the lateral faces of the meridian stone covered by the edges stripes were so finely smoothed as its upper surface with the calendar.

40 See Haselberger 1911, 54 fig. 7.

41 Buchner 1996, 36 speaks of a "travertine-balustrade (above ground level)" which were possibly built by travertine stones of the Flavian renovation.

42 Pliny *NH* 36.72 wrote of *strato lapide*, which Buchner 1976, 323 translates with *spread out stone*, in order to interpret it readily—like others—as pavement or stone slabs. But if one assumes that here a long line of adjoining stones was spread out on the Campus Martius, Pliny's choice of words makes almost more sense.

Putting the astronomy back into Greek calendrics: the *parapegma* of Euctemon

Robert Hannah

It is a pleasure to be able to offer a paper to our honorand. Many years ago James Evans established himself as a great teacher of the history of ancient Greek astronomy to many beyond the confines of his own lecture room through his book, *The History and Practice of Ancient Astronomy*. While in more recent years he has provided us with sophisticated papers on the more technical aspects of astronomy, especially as they pertain to the Antikythera Mechanism, it is to that earlier monograph, and its impact on myself and my own students, that I wish to pay homage in this small offering on ancient "observational" astronomy.

Parapegmata

Calendars across all cultures in the world have traditionally relied on the observed and/or measured motions of the celestial bodies: the sun, the moon, and the stars. From an early stage in historical Greek society the motions of the stars in particular played a significant part in time-measurement and the development of calendars. Alongside the phases of the moon (the fundamental basis of all Greek civil calendars) and the apparent movement of the sun (which provided seasonal markers), the Greeks also used the appearance or disappearance of stars at dawn or dusk to establish schedules for timekeeping.[1]

While we are aware of these uses of astronomy from the poems of Homer and Hesiod, from the fifth century BCE onwards there is evidence of an increase by Greek astronomers in the number of first and last star risings and star settings, and a formalisation of these data-sets into what were called *parapegmat*a. These survive from the Hellenistic and Roman periods as stone tablets inscribed with day-by-day entries for the appearance or disappearance of stars.[2] We may regard these as "star schedules," or more loosely as "star calendars." These were set up in public spaces in the cities, and therefore presumably had a civic significance beyond the narrowly astronomical, much as clocks did in Medieval and Early Modern Europe. Many of the leading astronomers of antiquity were credited with *parapegmata*—the list includes, from the fifth century, Democritus (more a philosopher than an astronomer, but who wrote a work called *Parapegma*)[3], Meton (associated with the 19-year "Metonic Cycle," which still governs the placement of Christian Easter in the western calendar, and the Jewish calendar as a whole), and Euctemon; and, from the fourth century, Eudoxus and Callippus.[4] *Parapegmata* continued in use until the Medieval period.[5] In literary form, they were combined into compilations and published either in their own

1 Hannah 2009, 2005, 2001.

2 Hannah 2002: figs. 6.1, 6.2.

3 Diogenes Laertius 9.48.

4 Dicks 1970: 81, 84–85.

5 McCluskey 1998.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 87–108. Chapter DOI: 10.5281/zenodo.3975723.

right by astronomers (e.g. in Geminus, *Eisagoge*; Ptolemy, *Phaseis*), or subsumed into agricultural "handbooks" by literary authors, especially by the Romans (e.g. Columella, *De re rustica*; Varro, *De re rustica*). Star lore of this kind pervades every aspect of Greek and Roman literature: it can be found in all the major authors, from Aeschylus to Euripides and Aristophanes, through Aratus to Plautus, Vergil and Ovid and beyond. Julius Caesar was credited with a "star calendar," which survives in later quotations (notably by Pliny, *Historia Naturalis* 18). In all, about 60 *parapegmata* survive in epigraphical and literary form.[6]

A generation before Democritus (b. ca. 470 BCE), the earliest of these authors, the philosopher Anaxagoras was already writing scientific treatises in prose, rather than in the poetic form that his predecessors had regularly used, so it seems to me likely that these early *parapegmata* appeared in prose form too. Whether these works were lists, as the term *parapegma* can signify and as they have come down to us through later compilations organised according to some temporal system (day-count, zodiacal months, calendars) or were embedded in treatises on broader topics from which the data have later been excerpted, it is impossible to tell now.[7] Equally whether there were more data than have survived in the compilations, or whether some data have been misattributed to these astronomers, is also impossible to know. Source criticism demands that such caveats be kept in mind, but they do not invalidate work based on what has survived; they simply make certainty impossible in the current state of affairs. In what follows, the "*parapegma* of Euctemon" signifies what authors in antiquity thought it meant, namely a list of star risings and settings, sometimes allied with other astronomical or meteorological data. Such a *parapegma* may have been a stand-alone entity (written in more or less perishable form), or it may have been derived from some larger work by Euctemon.

The *parapegma* was first dealt with in any significant manner in a series of publications by Rehm in the first half of the 20th century.[8] Several papers by van der Waerden treated the topic in the second half of the century,[9] but because he based his work on Rehm's, it is still the latter's views which lie behind his scholarship. Unfortunately, as others have noted, Rehm mixed fact and hypothesis indiscriminately in his conclusions,[10] and a rigorous critique of his work shows that there is a need to re-lay the astronomical foundations for work on the subject. Furthermore, Rehm and van der Waerden were primarily interested in simply reconstructing the *parapegmata* as lists of observations, rather than situating them into a wider context. The broader scientific and cultural context is what the most recent, and now fundamental, book specifically on *parapegmata* aims to provide, as it also presents all *parapegma*-style texts in full.[11] My own approach in this paper is to interrogate the accuracy of the *parapegma* as a time-marking instrument that once pervaded many aspects of Greek and Roman culture.

Accuracy

Despite the pervasiveness of data about star phenomena in the broader culture of Classical antiquity, the information that can be derived from *parapegmata* has been regarded as inaccurate

6 Lehoux 2007.

7 On the question of lists and literacy with regard to the *parapegmata*, see Hannah 2001.

8 Diels and Rehm 1904, Rehm 1913, Rehm 1949.

9 der Waerden 1960, Pritchett and van der Waerden 1961, van der Waerden 1984a, 1984b, 1984c, 1985.

10 Neugebauer 1975: 595 n.17, Lehoux 2007: 89, 2005: 125–26.

11 Lehoux 2007; cf. a similar approach for Babylonian astronomical texts in Rochberg 2004.

in itself or in the uses to which it was applied. This issue goes back to antiquity—Pliny the Elder (*Historia Naturalis* 18. 210–213) complained about the different dates given in his sources for the same star phenomena when observed in the same country. The complaint is picked up in modern scholarship.[12] This has been especially the case with regard to the dates for the phenomena given by the Roman poet Ovid in his calendar-poem, the *Fasti*.[13] But recent studies suggest that the astronomical basis of the star data relayed by Ovid has been misconstrued, and that modern parameters for accuracy have been imposed anachronistically on the ancient data.[14]

A *parapegma* afforded the facility to measure time through the year via the stars by pinpointing in the year when a star first became visible at dawn or dusk, or was last seen at dawn or dusk. For a *parapegma* to be able to state when a star would be first visible or last seen in the dawn or dusk sky, it has generally been assumed that ancient observations, like all modern ones, took the magnitude of each star into account, and therefore required the sun to be at different distances below the horizon, depending on the apparent brightness of the star. To be sure, Ptolemy himself says as much when discussing the methods by which one could calculate first and last visibility,[15] but this ignores the earlier practice in antiquity of calculating the times of star-rise and star-set by assuming a fixed angle for the sun below the horizon (on which, see below).

In the early 19th century Ideler calculated the angles for the solar depression on the basis of the 1st- and 2nd- magnitude stars, and the dates for first and last visibilty at the five different latitudes (*climata*) that were presented by Ptolemy in his *Phaseis*. Ideler's final angles were then the result of his averaging these calculations based on Ptolemy's dates.[16] In the early 20th century Boll, Ginzel, Vogt and Neugebauer presented the dates for star-rise and star-set in tables, again based on the assumption that solar angular depression was dependent on the magnitude of the star.[17] But even these tables depended on Ptolemy, as Ginzel noted. Thus, for morning risings and evening settings of 1st magnitude stars, the angle for the sun below the horizon was 11°–12°; for 2nd magnitude, 13°–14°, for 3rd magnitude, 14°–16°, and for fainter stars, 15°–17°. For evening risings and morning settings, the respective limits were 7°, 8.5°, 10°, and 14°. Ginzel added that the calculations agreed with naked-eye observations, with the angle of the sun below the horizon for (presumably) the morning rising and evening setting of Sirius being about 10°, for Aldebaran (1st magnitude) 10°–11.5°, for Regulus (magnitude 1.3) 12°, for α Arietis (2nd magnitude) 11°, and for the Pleiades (η Tauri, 3rd magnitude) 15°–16°. Neugebauer simplified these parameters for morning risings and evening settings to 11° for stars brighter than magnitude 1.5, 14° for magnitudes 1.5–2.5, 16° for magnitudes 2.5–3.5, 17° for magnitudes 3.5–4.5, and 17° for stars

12 The observations of the *parapegmata* are explicitly called "rough" by Bowen and Goldstein 1988: 56. More importantly, their accuracy is questioned in one of the standard modern editions of the work in which the *parapegma* of Euctemon is embedded (Aujac 1975), where one will find notes drawing the reader's attention to differences between the dates of observations given in Geminus's compilation and those calculated in modern times. Aujac (1975: 158) expressly includes among the possible causes for these discrepancies "manque de précision dans l'observation."

13 Ideler 1825: 137–69. J. G. Frazer's influential commentary (Frazer 1929) set Ideler's view in concrete for generations; his views are repeated and embellished in recent treatments of the poem, e.g. Bömer 1957–58, Herbert-Brown 1994, Nagle 1995, Newlands 1995, Fantham 1998, Gee 2000.

14 Hannah 1997a, 1997b, Fox 2004; Robinson 2009, Lewis 2014.

15 Ptolemy, *Almagest* H198–204 (Toomer 1998: 413–17).

16 Ideler 1819.

17 Boll 1909: 2429–30; Ginzel 1906–14: 2. 517–22 (on which Bickerman 1980: 112–14 was based); Vogt 1920; Neugebauer 1925.

fainter than magnitude 4.5; and for evening risings and morning settings to 7˚ for stars brighter than magnitude 1.5, 8.5˚ for magnitudes 1.5–2.5, 10˚ for magnitudes 2.5–3.5, 14˚ for magnitudes 3.5–4.5, and 17˚ for stars fainter than magnitude 4.5.[18] Later Neugebauer would update his parameters on the basis of naked-eye observations made by Schoch, and indeed Schoch's work is important in at least three respects in our present context: he made actual observations of first and last star phases to arrive at his angles of solar depression; he noted that previous calculations traditionally had ignored the effects of refraction and therefore assumed the stars were at the horizon rather than some degrees above it after clearing the atmosphere; and he was aware that reliance on Ptolemy's data was permissible only in Alexandria, where Ptolemy was based, because in places like Athens and Babylon the atmosphere was clearer and observations would reach markedly different dates for morning firsts and evening lasts[19]. Schoch's measurements "for places with a very clear sky (such as Babylon and Athens)" for first morning rising and last evening setting for stars near the ecliptic were: magnitude -1: (morning rise) 9.5˚, and (evening set) 8.5˚; magnitude 0: 10.5˚, and 9.5˚; magnitude +1.0: 11.5˚, and 10.5˚; magnitude +2: 13.5˚, and 12.5˚; magnitude +3: 16.0˚, and 15.0˚.

Later work addressed some of the difficulties inherent in naked-eye observations of these star phases, which Schoch had hinted at, in particular on calculating the "extinction angle" of a star at the horizon, i.e. "the smallest apparent altitude at which, in perfectly clear weather, it [the star] can be seen."[20] Matthew Robinson has provided a concise and lucid of history of some of these investigations, notably those of the astrophysicist Bradley Schaefer, so there is no need to enter into great detail here.[21] Let me instead note the practical approaches that stem from the work of Alexander Thom, who worked on the henges of Britain. Thom developed a rule of thumb according to which a star's extinction angle was "roughly equal to the magnitude of the star, so that a third-magnitude star cannot be seen below 3˚."[22] Mann, having noted that Thom's Rule was based on having "perfectly clear weather" and that such conditions were not common in Britain where both he and Thom worked, has suggested a more practical estimation would be "Thom's Rule +1," i.e. an angle computed from the value of the magnitude of the star plus an extra degree.[23] Interestingly, this revised rule may produce results not dissimilar from those derived from calculations using Schaefer's formulae for poor visibility conditions.[24]

18 Neugebauer 1925: 60 Tafel 28. I am taking Evans' point that the modern terminology of heliacal rising (for morning rising), heliacal setting (evening setting), achronychal rising (evening rising) and cosmical setting (morning setting) can be unhelpful and is largely anachronistic: Evans 1998: 197; see also Fox 2004: 104, Robinson 2009: 356–58.

19 Schoch 1924: 731–32. He notes that "in Alexandria there was always a layer of mist covering the lower part of the horizon," a comment that will resonate with anyone who has been near the Nile. For more recent "in the field" observations, but in a different context, in modern Australia, see Leaman, Hamacher and Carter 2016.

20 Thom 1967: 15.

21 Robinson 2009. See Schaefer 1985, 1986; cf. Ruggles 1999: 52. For an instance in Greece in which Schaefer's approach is taken into account, see Salt and Boutsikas 2005.

22 Thom 1967: 15. For a recent instance of "Thom's Rule" in action, see Hannah, Magli and Orlando 2017: 7 (in relation to the Temple of Juno at Agrigento).

23 Mann 2011: 252–53 n.11.

24 Using "Thom's Rule + 1" for the morning rising of Arcturus in Rome, 5 CE, I produce a date of 23 September if Arcturus was 1˚ above the horizon and the sun 11˚ below, which matches Robinson's calculation, based on Schaefer's criteria for bad visibilty with a limiting magnitude of 5 and an extinction factor of 0.3: Robinson 2009: 307 Table 15.

A different approach

However, in contrast to these approaches that depend on the magnitude of stars, Matthew Fox has used Pliny, *Historia Naturalis* 18. 218 to posit that ancient observations of the stars took the sun to be at a certain distance below the horizon, regardless of the brightness of the star being observed on the horizon.[25] Pliny was not referring here to an angular distance, such as the 15˚, or half a zodiacal sign, that we know Autolycus used at the end of the 4th century BCE.[26] Bowen and Goldstein once speculated that Eudoxus, as an older contemporary of Autolycus, "probably assumed in his *parapegma* that phenomena will be visible if the sun is ½ zodiacal sign below the horizon,"[27] but it seems unlikely that they would agree entirely with this now, given their later affirmation that artificial zodiacal signs of 30˚ are not attested in Greek texts before the third century BCE.[28]

As Neugebauer pointed out, a scheme based on a fixed visibility limit like that of 15˚ can produce symmetries between the phases, such as those produced by Autolycus:

(last) evening setting → (first) morning rising: 30 days

(first) morning rising → (last) evening rising: 5 months

(last) evening rising → (first) morning setting: 30 days

(first) morning setting → (last) evening setting: 5 months.[29]

Tannery had discerned elements of a similar symmetry in the *parapegma* associated with Eudoxus (for morning rising → evening rising, and for morning setting → evening setting) among those included in the *parapegma* of Geminus, but not in those attributed to Euctemon, Democritus or the little that is provided from Callippus.[30] This suggests that Eudoxus, like Autolycus, assumed a fixed visibility limit for the star phases,[31] whereas Euctemon did not, but whether Eudoxus used 15˚, however equivalently expressed, is not discussed. Tannery took it, anyway, that Euctemon did not subject the results of his star observations to a similar theory.[32]

The method adopted by Autolycus, and arguably by Eudoxus if Tannery was correct to discern a pattern in his *parapegma*, of using half a zodiacal sign as a guage of the sun's depression

25 Fox 2004.

26 E.g. Autolycus, *On Risings and Settings* 2.6. For examples derived from Autolycus's *Risings and Settings*, see Evans 1998: 190–7. Cf. Neugebauer 1975: 760–1: "From an astronomical viewpoint a universal 15° visibility limit is a rather crude simplification of facts which obviously are much more complex. It cannot have escaped notice that not all stars appear or vanish simultaneously or that the eastern and the western parts of the horizon are not the same in darkness near sunrise or sunset. Nevertheless the 15° limit—or the equivalent 15-day limit for the solar motion—was generally accepted."

27 Bowen and Goldstein 1988: 56.

28 Bowen and Goldstein 1991.

29 Autolycus, *On Risings and Settings* 2.6; Neugebauer 1975: 761.

30 Tannery 1912: 234.

31 As noted by Neugebauer 1975: 761, while dismissing Tannery's deduction that it also suggested that there was a (now lost) work by Eudoxus on spherics.

32 Tannery 1912: 234.

below the horizon, relies on an awareness of the sun's apparent path along the ecliptic. In Greece the discovery of the ecliptic as the sun's oblique pathway through the zodiac is accredited to Oenopides of Chios, who lived in the late fifth century BCE.[33] The use of its constituent constellations (rather than the artifically equal zodiacal signs, which were formulated later) as temporal stepping stones through the solar year is not impossible at this time, seeing as Oenopides' older contemporary, Cleostratus, is said by Pliny to have distinguished the constellations along the ecliptic, starting with Aries and Sagittarius.[34] Indeed Dicks went so far as to surmise that to judge from their activities Meton and Euctemon were also familiar with the ecliptic.[35] Whatever the case, we might keep this method in mind for future investigations of the star data not only of Euctemon but of Eudoxus too.

Two other options suggest themselves to me for how Euctemon produced his data: either he conducted simply naked-eye observations, and/or he used a different artificial method. The former has little to recommend it, as we shall see when I investigate a few of the data from Euctemon. However, one possibility for a different artificial method is provided by Pliny in the passage already referred to: a depression of the sun below the horizon that was measured by time. Pliny says that the sun must be "at least three-quarters of an hour" below the horizon. He clarifies at *Historia Naturalis* 18. 221 that he is talking of equinoctial hours, not the more usual seasonal hours used in sundials and daily life. Le Bonniec and Le Boeuffle translated this "three-quarters of an hour" into angular terms, suggesting that this corresponds to about 12° below the horizon.[36] This angle, which equates to modern "nautical twilight," is the minimum workable angle for the sun's depression below the horizon that would enable stars down to and including magnitude 2 to be observed, which would then allow the majority of the stars in the *parapegmata* to be seen. Pliny's modifier "at least" would permit the longer period needed to include the fainter Pleiades, Delphinus (the Dolphin) and Sagitta (the Arrow), assuming the last two have been correctly identified.

While Pliny's use of a period of "at least three-quarters of an hour" is primarily a time-related criterion, it is not impossible that this still relates, in a secondary fashion, to the brightness of the stars. Through the sequences of predawn twilight (we would say from "astronomical" through "nautical" to "civil"), the sun's depression below the horizon rises from about 18° to zero, with the result that stars of diminishing brightness are gradually lost to sight. But the fact that a time period is chosen as the criterion draws us away from magnitude and into the realm of timing mechanisms. This in turn suggests the prioritisation of an external timing device over naked-eye observation of the star.

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33 Theon of Smyrna, *Expositio rerum mathematicarum ad legendum Platonem utilium* [*Aspects of Mathematics Useful for the Reading of Plato*] 198.14–15 (2nd century CE) says that "Eudemus [late fourth century BCE] reported in his *Astronomies* that Oenopides was the first to discover the belt of the zodiac." Diodorus Siculus 1.98.3 (first century BCE) says that "Oenopides, while also spending time with the priests and astronomers [of Egypt], learned other things and especially about the circle of the sun, that it has a slanting route, and makes its movement opposite to the other stars." For an in-depth of analysis of what we might know, or not know, about Oenopides' discovery of the obliquity of the ecliptic, see Bodnár 2007: 4–8 (I am grateful to Professor Anthony Spalinger and Dr Dougal Blyth for this reference).

34 Pliny, *Historia Naturalis* 2.31: "Then [following Anaximander's discovery of the ecliptic] Cleostratus distinguished the signs in it, first those of Aries and Sagittarius." Bodnár 2007: 6 interprets Anaximander's "discovery" attested by Pliny here as nothing to do with the ecliptic *per se* but instead as concerning the slant of the paths of the stars and planets against the horizon.

35 Dicks 1970: 172.

36 Le Bonniec and Le Boeuffle 1972: 264 n. 3.

Timing

One possible explanation for the use of “three-quarters of an hour” as a basis for timing is that it reflects fairly well a whole “winter” hour for Athens or Rome. If we work with seasonal, rather than equinoctial, hours, then an hour in mid-winter in Rome—i.e. one twelfth of the time between sunset and sunrise—amounts to 45 minutes, or three-quarters of an equinoctial hour, while such an hour in Athens amounts to 47 minutes. Pliny’s criterion might therefore signal that the night was measured by astronomers in “winter hours” regardless of the actual season, and that this could readily be equated with three-quarters of an equinoctial hour. Such a methodology is found elsewhere in Athenian culture, making it culturally plausible in the case of the star data associated with Euctemon: the whole of the legal day, regardless of season, was made to correspond to the length of the shortest days of the year, those of the mid-winter month Poseideon.[37]

Another possible explanation for the “three-quarters of an hour” is that it corresponds quite closely to twice the value of 24 minutes, which is a sixtieth of 24 hours. This is a fraction of the full day that was recognised in Babylonian (and later Indian) astronomy. In Babylonia one 24-minute measure was six UŠ, or one *mina*, and by the sixth century BCE waterclocks of the outflow type were being made that could measure this time-period or multiples of it.[38] The same measure of 24 minutes arises much later in Indian astronomy in historical times, and was measurable via simple holed bowls that would sink into buckets of water at this given rate of 24 minutes.[39] Such a bowl, made of copper and dating to perhaps the ninth century BCE, though of what measure is unknown, has been identified from the material from Nimrud.[40]

It may be that some similar type of bowl, made of metal or pottery, was used in fifth century Athens. Waterclocks were certainly used there. Ideally we would want one of the type that noted the Greek equivalent of the number of *minas* flowing out through a whole night, whose total measure in the equivalent of *minas* was known season by season.[41] The astronomer would just need to note the start of the last two *minas* to catch any observations in the crucial period of “three-quarters of an hour” before sunrise. The waterclocks used in Greece by soldiers on night watches might have offered such a means for measuring star-rise and star-set through the seasons. We read from a fourth century BCE source that, in order to ensure that the night watches were equal, the volume of water that the clocks held could be adjusted by coating the inner surface of the waterclock with different thicknesses of a layer of wax, and by changing the waterclock every ten days. The interior was waxed with a thinner coating to allow more water to be held when the nights were longer, and with a thicker layer to make the clock hold less water when the nights were shorter.[42] Alternatively, a fifth century BCE clay bucket excavated in the Athenian Agora was found to contain a volume of two *choes*, or 6.4 litres, which would empty

37 [Aristotle], *Athenian Constitution* 67.4; Harpokration, s.v. *hemera diamemetremene*; Hannah 2009: 102.

38 Fermor and Steele 2000. For short periods of time in Babylonia, see now Steele 2020.

39 Sarma 1994; Sarma 2015: 2008–2009; Sarma 2018: 3645–711. I am grateful to Professor Clemency Montelle for advice on the Indian material.

40 London, British Museum 91283; Brown 2000: 119–20.

41 Such measurements are mentioned in Babylonian tablets: Hunger and Pingree 1989: 163–64.

42 Aeneas Tacticus 22.24–25.

out in six minutes.[43] Seven *choes* could therefore empty out in 21 minutes, and two such periods would make 42 minutes, a span of time close to Pliny's criterion.

If appropriate artificial mechanisms were unavailable, we could still posit the use of natural time signals specifically for the predawn period, such as cockcrow. In his play, *Ekklesiazousai* (30–31, 82–85, 390–91), produced in 391 BCE, Aristophanes has characters noting the "second cockcrow" as a specific time signal that occurs before sunrise when the stars are still visible. This implies a first cockcrow earlier in the predawn period, and quite likely a third or more, to judge from cross-cultural examples.[44]

From the time of Eudoxus in the fourth century BCE, we might suppose use of some form of celestial globe.[45] Even some small time before then, however, Plato's description of the creation of the cosmos in his *Timaeus* suggests that something like the more skeletal armillary sphere was familiar to him.[46] We have, unfortunately, no physical evidence of such instruments from this period.

Euctemon's parapegma

So what happens if we apply the criterion of time rather than magnitude to the star data preserved from Euctemon? In what follows I deal only with a small section of the data set attributed to Euctemon by Geminus, namely the first part of the summer section from the morning rising of the Pleiades to the morning rise of Sirius. It is enough to give a flavour of the type of work which might be pursued further, and the relative strength of the argument regarding a time-limit rather than purely ocular observation. The Greek text followed here is Aujac's; the translation is my own.[47] However, I have not followed Aujac's Julian calendar equivalences for the zodiacal-month dates in Geminus. These dates were those supplied by Manitius, an earlier editor of Geminus,[48] and suited a date of ca. 45 BCE. Instead I have reset the Julian dates to suit the Julian equivalent of the date of the summer solstice used by Euctemon's colleague, Meton, namely 27 June in 432 BCE (in reality, the date was 28 June for that period). It is from the summer solstice and the entry of the sun into Cancer that the Geminus *parapegma* is calibrated.

I am not being mathematically precise about the figure of "three-quarters of an hour," but am allowing some leeway either side of it, given the likely imprecise method of measurement for a unit of this scale. If on a given date for a star phase it is found that the sun was around three-quarters of an hour below the horizon, then I regard this as an accurate datum. This does not necessarily mean, however, that the phase was "visible" in the ordinary sense of the word, any more than a star phase could be seen when rain was forecast.[49] For comparison I have also taken the astronomical data (RA, Dec, magnitude) from the computer planetarium programme,

43 Young 1939; Hannah 2013: 350–52, with Figure 23.2

44 For sequential cockcrows as a time signal for encroaching dawn in Roman, Medieval and modern cultures, see Birth 2011; he notes, for example, the first cockcrow in a Filipino context at 4 a.m., which would be about two hours before sunrise.

45 Evans 1998: 249.

46 Hannah 2009: 116–18.

47 Recent versions of the Geminus *parapegma* are those of Aujac 1975, Evans and Berggren 2006, Lehoux 2007.

48 Aujac 1975: 158.

49 On the disjunction in the astrometeorological *parapegmata* between weather events and star phases, see Lehoux 2007: 59.

Voyager 4.5.[50] Dates for the various stellar phases are then derived from my own calculations based on the trigonometrical formulae that underlie the examples in Neugebauer 1925; I have added here in Appendix 1 my own derivation of Neugebauer's method. I have entered the data derived from these calculations to the Voyager planetarium program to gain a visual impression. These readings have been set in the program against an horizon of Athens as seen from the excavated Pnyx, the site where Meton, Euctemon's colleague, was said to have set up a *heliotropion*—which might have been a means of identifying the solstices—and which has largely not changed since antiquity.[51] As it turns out, however, the readings suggest that this was not an observation point for the data that we have. I wonder whether Mount Lykabettos, at 300m high and on the outskirts of Athens, would have been a better observation site, epsecially as this is where Meton's teacher, Phaeinus, was said to have observed the solstices.[52]

> [Τὸν δὲ Ταῦρον διαπορεύεται ὁ ἥλιος ἐν ἡμέραις λβ.
>
> *The sun passes through Taurus in 32 days.]*

This could not be in Euctemon's *parapegma*, but the zodiac is used in Geminus as the organising principle; the usage stems probably from Callippus's *parapegma*.[53]

> Ἐν δὲ τῇ ιγ^{η} Εὐκτήμονι Πλειὰς ἐπιτέλλει· θέρους ἀρχή· καὶ ἐπισημαίνει.
>
> *Day 13 (May 6), according to Euctemon Pleiades rise; beginning of summer; and there is sign of weather.*[54]

This is a difficult observation to start off with for several reasons, as will be seen shortly. It signals the first visible morning rising of the Pleiades. The date of 6 May puts the sun 52 minutes short of rising, which suits Pliny's criterion. But on 6 May when η Tau was rising, the sun was only 9°16' below the horizon. The rising of the Pleiades was therefore technically not visible by modern calculations—with a flat horizon first visibility would occur ca. 23 May, with the sun 16° below the horizon, more than an hour and a half before sunrise.[55] That being said, however, Ginzel, quoting calculations by Hartwig, has 15–19 May for 431 BCE, which presumably signals a smaller angle of solar depression than 16°.[56] Schoch was aware of the discrepancy between modern calculations and ancient records: while allowing for a solar arc of 15.5° for the morning rising of the Pleiades, he also noted, "The magnitude of η Tauri is only +3.0. But Greek and Babylonian observations seem to imply a greater brilliance for the Pleiades. Perhaps Alcyone was brighter

50 Voyager 4.5, Carina Software, 865 Ackerman Drive, Danville, CA 94526, USA.

51 On Meton's *heliotropion*, see Kourouniotes and Thompson 1932: 207–11, Bowen and Goldstein 1988: 72–77, and recently Jones 2017: 77–8.

52 Theophrastus, *De Signis* 4.

53 See Hannah 2002: 120–21.

54 On the meaning of ἐπισημαίνει, which I have translated as "there is sign of weather," see Evans and Berggren 2006: 230 n.1. They translate it as "it signifies"; Lehoux 2007: e.g. 233 proposes "there is a change in the weather." All understand the usage to refer to a meteorological event.

55 Ginzel 1906–14: 1.517; Neugebauer 1925: 60 Tafel 28 allows the solar arc to be 16° for stars of magnitude 2.5–3.5. Schoch 1924b: 4 has an arc of 15.5° for the Pleiades; I calculate this would result in a date of 21 May in Athens.

56 Ginzel 1906–14: 1. 27

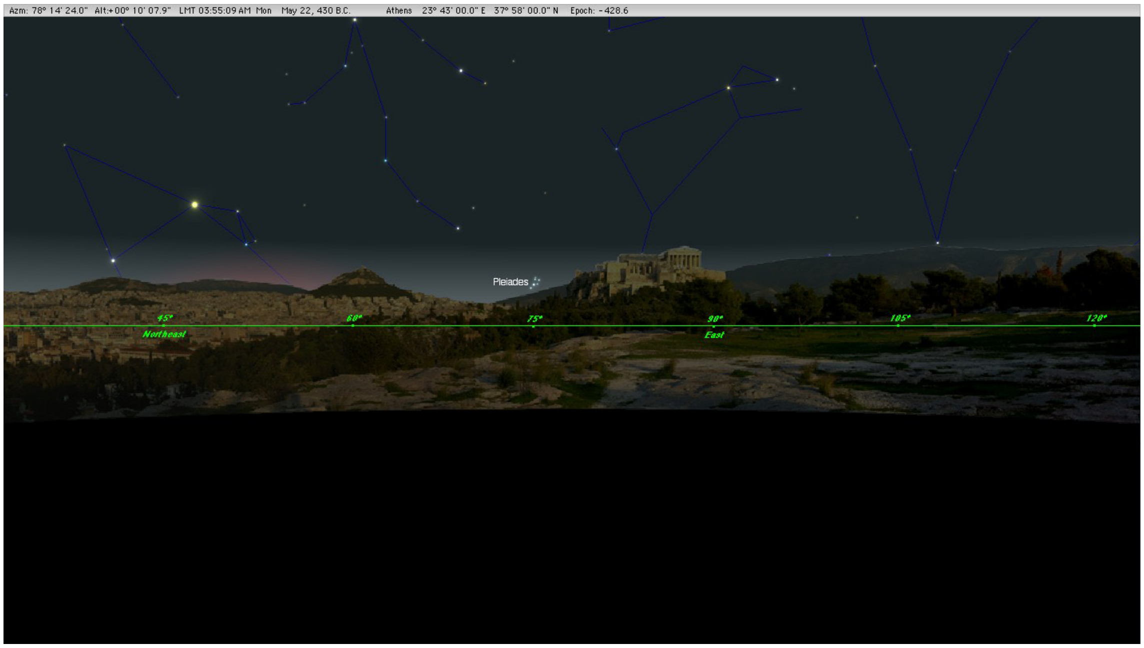

Figure 1. View from the Pnyx, Athens, of the first visibility of the rising Pleiades between the Acropolis and Mount Lykabettos, 22 May 430 BC.

2,500 years ago than now, or else the Pleiades, as a cluster, occupying an appreciable space on the sky, are more readily seen than an individual star of the same magnitude."[57] From my own observations I think there is truth in what Schoch says: the Pleiades seem to benefit from being a distinctive, twinkling cluster in a dark area of sky, that is most readily found by not looking directly at it at first (and this applies even to my home latitude at 46°S, where the Pleiades do not rise high in the sky as they do in the northern hemispehere). Ovid, *Fasti* 5.599–602 has the rising of the Pleiades in Rome on 13 May. Fox noted that for 5 CE, around the time Ovid composed his poem, "On May 13 in Rome the sun rises at 4:49 A .M . and the Pleiades at 3:43 A .M., probably enough time for them to be high enough in the eastern morning sky for an observer to see them before sunrise."[58] Fox is presumably allowing that the Pleiades, being so faint, would be some degrees above the horizon, to allow them to be visible in the predawn sky. But, although there is just over an hour between the rising of the Pleiades and that of the sun in Rome, and this suits the criterion of "at least three quarters of an hour" before sunrise, the sun lay only 10° below the horizon at the time of the rising of the Pleiades, and that seems to me too close to the horizon for the Pleiades to be visible. All the same, this arc is close to the one that I have calculated for 6 May 432 BCE, so perhaps the visibility of the Pleiades even with this small arc is within the bounds of possibility.

If the viewpoint was the Pnyx, then η Tau rose between the Acropolis and Mt Lykabettos, at an altitude of about 3°26' and an azimuth of 75°, and was not visible until about 22 May, with the sun 12.5° below a hypothetical flat horizon, but about 16° below the actual hilly horizon.[59] This late date would seem to count against viewing from the Pnyx. See Figure 1.

57 Schoch 1924b: 3.

58 Fox 2004: 120.

59 Cf. Salt and Boutsikas (2005) for a similar method of calculation for the much higher horizon at Delphi.

Ἐν δὲ τῇ λαῃ Εὐκτήμονι Ἀετὸς ἑσπέριος ἐπιτέλλει.

Day 31 (May 24), according to Euctemon Eagle rises in the evening.[60]

This signals the evening rising of Aquila. The date of 24 May put the sun 44 minutes after setting, which suits Pliny's criterion. On 24 May when α Aql was rising, the sun was 7°26' below the horizon. The rising of Aquila would therefore have been visible and accurate—with a flat horizon last visibility would occur on 25 May, with the sun 7° below the horizon.

Ἐν δὲ τῇ λαῃ Εὐκτήμονι Ἀρκτοῦρος ἑῷος δύνει· ἐπισημαίνει. ... Εὐκτήμονι Ὑάδες ἑῷαι ἐπιτέλλουσιν· ἐπισημαίνει.

Day 32 (May 25), according to Euctemon Arcturus sets at dawn; there is sign of weather. ... Hyades rise at dawn; there is sign of weather.

This signals the last visible morning setting of Arcturus. The date of 25 May puts the sun only 10 minutes short of rising, which does not suit Pliny's criterion. On 25 May when α Boo was setting, the sun was 1°52' below the horizon. The setting of Arcturus is therefore technically not visible—with a flat horizon first visibility would occur ca. 4 June, with the sun 7° below the horizon.[61] Indeed, this counts as a true, and therefore invisible, morning setting. As we shall see elsewhere, visible morning settings (by the criteria of time and optical visibility) are characteristically not this *parapegma*'s forte. This is curious, since last visible morning settings should have been relatively easy to note, because of the previous nights' observations of the star setting over the same horizon before dawn. This in turn stands in contrast to first morning risings, which could not be notified by previous morning observations and should therefore have been harder to determine, if one was actually physically observing the stars.[62]

This entry also signals the first visible morning rising of the Hyades. In this case, the date of 25 May put the sun 36 minutes short of rising, which practically suits Pliny's criterion. On 25 May when α Tau was rising, the sun was 6°23' below the horizon. The rising of the Hyades was therefore technically not visible—with a flat horizon first visibility would occur about 4 June, with the sun 11° below the horizon.[63]

[Τοὺς δὲ Διδύμους ὁ ἥλιος διαπορεύεται ἐν ἡμέραις λβ.]

[The sun passes through Gemini in 32 days.]

Ἐν δὲ τῇ κδῃ Εὐκτήμονι Ὠρίονος ὦμος ἐπιτέλλει.

Day 24 (June 18), according to Euctemon shoulder of Orion rises.

This signals the morning rising of Orion. Orion's shoulders are γ Ori (upper, so seen first) and α Ori (lower, so second).[64] According to calculation, the visible morning rising of both is on the

60 Aujac omits the manuscript reading, Ἐν δὲ τῇ κεῃ Εὐκτήμονι Ἀετός ἑσπέριος ἐπιτέλλει [Day 25 (May 19), *according to Euktemon Eagle sets in the evening*], because Ἀετός (Eagle) makes no sense astronomically. Ἄιξ (Goat) would be better, but for the sake of consistency, as I have chosen to follow Aujac's text, I have omitted the line.

61 Vogt 1920: 55 gives angle of 8.5°, instead of Neugebauer's 7°, for the morning setting of Arcturus. This would push the date to 6 June or later.

62 As noted by Schoch 1924a: 733.

63 Ginzel 1906–14: 1.27, quoting Hartwig's calculations, has 3–7 June for 431 BC.

64 Kidd 1997: 303.

same date, 30 June. The date of 18 June puts the sun 40 minutes short of rising, which suits Pliny's criterion. On 18 June when γ Ori was rising, the sun was 6°43' below the horizon. The rising of the shoulders of Orion is therefore technically not visible—with a flat horizon first visibility would occur around 30 June, with the sun 14° below the horizon.

[Καρκίνον διαπορεύεται ὁ ἥλιος ἐν ἡμέραις λα.]

[The sun passes through Cancer in 31 days]

[<Ἐν μὲν οὖν τῇ> α$^{\eta}$ ἡμέρᾳ Καλλίππῳ Καρκίνος ἄρχεται ἀνατέλλειν· τροπαὶ θεριναί· καὶ ἐπισημαίναι.]

[Day 1 (June 27). according to Callippus, Cancer begins to rise; summer solstice; and there is sign of weather.]

<Ἐν δὲ τῇ> ιγ$^{\eta}$ ἡμέρᾳ Εὐκτήμονι Ὠρίων ὅλος ἐπιτέλλει.

Day 13 (July 9), according to Euctemon all of Orion rises.

This signals the morning rising of all of Orion—I assume that "all of Orion" means not just β Ori but also κ Ori, as these are the "two feet" of Orion in Aratus (*Phaenomena* 338). The date of 9 July puts the sun 46 minutes short of rising, which suits Pliny's criterion. On 9 July when κ Ori was rising, the sun was 7°34' below the horizon. The rising of Orion is therefore technically not visible—with a flat horizon first visibility would occur around 20 July, with the sun 14° below the horizon.

<Ἐν δὲ τῇ> κζ$^{\eta}$ Εὐκτήμονι Κύων ἐπιτέλλει.

Day 27 (July 23), according to Euctemon Dog rises.

This signals the morning rising of Sirius. I assume "Dog" means α CanMaj, not β CanMaj. The date of 23 July puts the sun 36 minutes short of rising, which practically suits Pliny's criterion. On 23 July when α CanMaj was rising, the sun was 6°03' below the horizon. The rising of Sirius is therefore technically not visible—with a flat horizon first visibility would occur around 31 July, with the sun 11° below the horizon.[65]

<Ἐν δὲ τῇ> κη$^{\eta}$ Εὐκτήμονι Ἀετὸς ἑῷος δύνει· χειμὼν κατὰ θάλασσαν ἐπιγίνεται.

Day 28 (July 24), according to Euctemon Eagle sets at dawn; storm at sea comes on.

This signals the morning setting of Aquila. I assume "Eagle" means α Aql (the last part of Aquila to set), not λ Aql (the first part), whose setting date is much earlier. The date of 24 July puts the sun 16 minutes short of rising, which does not suit Pliny's criterion. On 24 July when α Aql was setting, the sun was 3°14' below the horizon. The setting of Aquila is therefore technically not visible—with a flat horizon last visibility would occur around 30 July, with the sun 7° below the horizon, and the sun would be 42 minutes short of rising, which would suit Pliny's criterion. As we have seen already, however, morning settings are a weak point in this *parapegma*.

65 Ginzel 1906–14: 1. 27, quoting Hartwig's calculations, has 27–31 June for 431 BC, but this must be a misprint for 27–31 July.

[Τὸν δὲ Λέοντα διαπορεύεται ὁ ἥλιος ἐν ἡμέραις λα.]

[The sun passes through Leo in 31 days.]

Ἐν δὲ τῇ α$^{\eta}$ ἡμέρᾳ Εὐκτήμονι Κύων μὲν ἐκφανής, πνῖγος δὲ ἐπιγίνεται· ἐπισημαίνει.

Day 1 (July 29), according to Euctemon Dog is visible, and stifling heat comes on; there are signs of weather.

This signals the morning rising of Sirius. I assume "Dog" means α CanMaj: Evans and Berggren suggest that the two "observations" for the Dog signify a first fleeting visibility (27 Cancer / 23 July) and then an easy visibility (this present date).[66] This date of 28 July puts the sun 60 minutes short of rising, which suits Pliny's criterion. On 28 July when α CanMaj is rising, the sun is 9°57' below the horizon. The rising of Sirius is therefore technically just visible—with a flat horizon first visibility would occur around 31 July, with the sun 11° below the horizon.

Conclusions—and speculations

While acknowledging the caveats that inevitably surround the construct called "the *parapegma* of Euctemon," I have used a sample of the star phases attributed to Euctemon in the compilation attached to Geminus's *Eisagoge* so as to test the criterion provided by Pliny for the visibility of first and last risings and settings of stars. This criterion prioritises time relating to the sun over magnitude relating to the stars, requiring that the sun be "at least three-quarters of an hour" before rising or after setting, regardless of the magnitude of the star. This is not to say that magnitude does not matter, only that it is secondary, and in this I stand in contrast to previous scholarship from the 19th century on, which has focussed on magnitude as the only criterion.

By modern standards of calculation only two of the nine star phases attributed to Euctemon investigated in this paper appear to have been physically visible at the time assigned to them. On the other hand, of these nine star phases seven seem to suggest that a time-based measure lies behind the "observations." I have treated the time criterion loosely, since "three quarters of an hour" is anachronistic for fifth century BCE Greece—although hours, in the guise of "the twelve parts of the day," are among the things that Herodotus, in the fifth century BCE, says the Greeks learned from the Babylonians,[67] and therefore were presumably available to Euctemon and his colleagues, nonetheless part-hours do not appear until the late fourth century BCE.[68] The measure of "three quarters of an hour" may also be a later translation not of time but of quantity, i.e. of the amount of water that flowed out of a waterclock of some kind, or through a holed vessel, for a duration before or after the astronomical event being recorded. Or, probably less likely but suggested by developments under Eudoxus and Autolycus, the measure might reflect early attempts to measure time via the passage of the sun through the zodiacal constellations, which seem to have been distinguished by the Greeks in the fifth century BCE before Euctemon's time.

That the star phases themselves may not have been physically observed need not bother us for the period in question. The situation may be similar to that identified by Bowen and Goldstein with regard to the "observation" of the summer solstice by Meton and Euctemon in 432 BCE. In that case they deduced that the solstice was not fixed by a series of observations, since

66 Evans and Berggren 2006: 233 n.7.

67 Herodotus 2.109.3.

68 Hannah 2009: 82–3, 125–6.

there is no evidence of such a series, but rather by the placement in the Athenian civil calendar of a date for the solstice already identified by Babylonian astronomers.[69]

Nor does this tentative result of my investigation necessarily make Euctemon's data "wrong." We might imagine that the data were still regarded as correct, in the same sense that a medieval "scratch-dial" told the time "accurately" because everyone agreed that when it said "one o'clock" it was one o'clock for those who were to do something at that agreed time. Or perhaps it is like when people synchronize their watches but to the same wrong time: the "time" may be inaccurate according to an objective standard, yet it will be "correct" according to an internal agreement and therefore perfectly adequate for internally coordinating an activity.[70] Is it the same here with Euctemon's data, that the star phases, in some way, linked in with some external purpose that benefitted from the notices of the annual regularity not only of the weather signs but also of the star phases?

Was the Athenian calendar the point of the exercise? And—as I often come back to, in dealing with the Athenian *parapegma*—who was interested in these data? I usually propose the civic/religious authorities, intertwined as they were then, not the farmers and sailors, who did not really need to watch the stars to know when to sow and plough, or when to sail.[71] Nor, for that matter, did the officials, but they did want a calendar that allowed seasonal activities (governed by the sun and the weather) to correspond to the associated festivals (governed by timing with regard to the moon). The timing of religious festivals was a matter of public concern in Athens at the time when these star phases were organised. This meant that calibrating the festival calendar with the cycles of the sun and moon was necessary in order to get the timing of the festivals right. Meton is credited with the construction of a 19-year cycle (everyone seems to agree now that this was on the basis of exposure to Babylonian ideas, where such a cycle had long been in use). Bowen and Goldstein agreed with Neugebauer and Toomer in dismissing the notion that the Metonic cycle was devised in order to reform the Athenian civil calendar "for the good reason that there is no evidence that such cycles were developed to improve the civil calendar."[72] On this score they would have been better keeping their powder dry, as the use of the Metonic cycle to regulate the Athenian calendar is now demonstrable for the Hellenistic period, and may well have been experimented with from 432 BCE, although irregularities in its application would appear to have occurred to start with.[73] But on another score, I think Bowen and Goldstein were right to suggest that "one important purpose of the early Greek calendric cycles was to facilitate the use of almanacs or *parapegmata*. These cycles were used to correlate dates in the *parapegmata* with dates in a lunar calendar."[74]

69 Bowen and Goldstein 1988: 72.

70 Hannah 2009: 85–6.

71 For an example, see Hannah 2005: 62–70.

72 Bowen and Goldstein 1988: 52 n.62.

73 See Hannah 2009: 37 for further references, and Ossendrijver 2018 for the Babylonian evidence.

74 Bowen and Goldstein 52.

Appendix: P. V. Neugebauer's method for calculating the visibility of stellar phenomena (Neugebauer 1925)

	NEUGEBAUER'S TEXT page XXXVIII: Beispiel	MY COMMENTS
24a)	501 v.Chr. = -500	The given year (501 BCE) is transformed into the astronomical year.
24b)	Sirius -500: $a = 73^\circ.7$ $d = -16^\circ.4$	The *equatorial* coordinates for Sirius in 501 BC. a = Right Ascension, the distance in longitude from the vernal equinox (a) to the star d = Declination, the distance in latitude above or below the equator.
24c)	$l = 15^\circ$ östlich Greenwich, $f = 34^\circ$	l = geographical longitude f = geographical latitude of the observer (15°E, +34°).
24d)	Sirius ist ein Stern 1. Grösse. Tafel 28. $b = 11$ für heliakischen Aufgang und Untergang $b = 7$ für scheinbaren akronychischen Aufgang und scheinbaren kosmischen Untergang.	Sirius is a 1st magnitude star $b = -11^\circ$ for arc of visibility below horizon at heliacal rising and setting $b = -7^\circ$ for apparent acronychal rising and apparent cosmical setting. = sun's minimum negative altitude (in *horizon* coordinates) below the horizon for the star still to be visible just above the horizon. Formula:
24e)	Tafel 1. Vertikal Argument $d = -16^\circ.4$ Horizontales Argument $f = 34^\circ.0$ $t = 5^h.29$ oder in Grad = $79^\circ.4$	$t = \cos^{-1}\{-\tan f \tan d\}$ = the semi-diurnal arc, the time or distance between the rising or setting of a star on the horizon and its transit over the observer's meridian. = the hour angle (H) of a star at rising or setting. Hour angle (H) = how far the star has travelled since it crossed the meridian, so here t = time or distance, expressed in *equatorial* terms, between the meridian and the rising/setting point of the star on the horizon. The star's zenith distance = 90° at its rising/setting point, but this is in altazimuth terms, not equatorial.

Following the calculations for heliacal rising and apparent acronychal rising:

$a = 73^\circ.7$
$t = 79^\circ.4$
$y = a - t = 354^\circ.3$

y = the time elapsed/distance covered since the Vernal Equinox (VE) last crossed the meridian, and therefore also = where the meridian is.
Whenever Sirius rises, at any time of the day, VE lies about 24 minutes (0.4h) east of the meridian (i.e. *a* will cross the meridian about 24 minutes after Sirius rises). VE's precise position at Sirius's rising corresponds to the difference between the semi-diurnal arc of Sirius ($t = 79^\circ.4$) and the distance between Sirius and VE (= R.A. of Sirius, $a = 73^\circ.7$)
$= -5^\circ.7$ (= the approx. 24 minutes between VE and the meridian), to which 360° must be added to make it positive, i.e.
$y = 360^\circ - (t - a) = 360^\circ - t + a = 360^\circ + a - t$
$= 360^\circ + 73^\circ.7 - 79^\circ.4 = 360^\circ - 5^\circ.7 = 354^\circ.3$ (= 23h 37m 12s R.A.), which is effectively Neugebauer's formula.
[In the case of calculating heliacal / cosmical settings, $y = t + a$ = the sum of the semi-diurnal arc (t)—which takes us from the meridian westwards to the point of setting for Sirius—plus the R.A. of Sirius—which takes us beyond Sirius round to where a now lies; i.e.
$y = 79^\circ.4 + 73^\circ.7 = 153^\circ1$ = 10h 12m 24s R.A.]
This means that the meridian lies at 354°.3 R.A. in the case of Sirius's rising, and at 153°.1 in the case of its setting.
In its turn, the R.A. of the meridian is also necessarily the R.A. of the zenith, since it lies on the meridian.

24f) Argument $y = 354^\circ.3$:
$p = -0^\circ.6$
$r = +2^\circ.6$
$s = +23^\circ.6$
$S = f + r = +36^\circ.6$

24g) Tafel 25.
Vertik. Arg. $s = +23^\circ.6$
Horiz. Arg. $S = +36^\circ.6$
$P = +16^\circ.6$
Tafel 26.
Dieselben Argumente:
$B = +33^\circ.2$
$y = 354^\circ.3$
$p = -0^\circ.6$
$P = +16^\circ.6$
$L = 10^\circ.3$

24f) - 24g):
y = R.A. of the meridian, and
f = observer's geographical latitude.

Formulae:
$L = \tan^{-1} \{(\sin y \cos e + \tan f \sin e) / \cos y\}$
$B = \sin^{-1} \{\sin f \cos e - \cos f \sin e \sin y\}$ where
e = the obliquity of the ecliptic.

y and f are converted from equatorial to *ecliptic* form (L, B).

f by extension = Declination of the zenith point. Converting f into ecliptic form (B) not only gives the latitude of the zenith point from the ecliptic, but more importantly the angle below the ecliptic, between it and the horizon (= 90˚ – B, since zenith-to-horizon = 90˚). This will be used to calculate L_1: see 24h) below.

y is converted to ecliptic form to provide the sun's position on the observer's meridian (the sun's ecliptic longitude, L), when Sirius is rising or setting.

We need to maintain this angular relationship between Sirius and the sun. Putting the sun on the meridian as Sirius rises or sets gives a time (noon) and date when the sun and Sirius are in this angular relationship. But it is not the date of Sirius's HR, AR, HS or CS, because if we then move the sun of this date to either the eastern or the western horizon, Sirius will also have moved commensurately forwards or backwards from its position on the horizon, to maintain the angle between it and the sun.

We therefore still need to find when the sun is on or just below the horizon as Sirius rises or sets.

24h) Heliakischer Aufgang
$b = 11^\circ$
Tafel 27.
Spalte $b = 11^\circ$
Arg. $B = 33^\circ.2$
$L_1 = 13^\circ.2$

Heliacal Rising
The formula at Tafel 27 is:
$\sin L_1 = (\sin b \,/\, \cos B)$,
so $L_1 = \sin^{-1} (\sin b \,/\, \cos B)$
L_1 = the angular distance on the ecliptic from the horizon to the nearest position of the sun below the horizon at which the star is visible before sunrise / after sunset.

f = observer's geographical latitude, and by extension = Declination of the zenith point.

As noted above, converting f into ecliptic form (B) not only gives the latitude of the zenith point from the ecliptic, but more importantly the angle below the ecliptic, between it and the horizon (= $90^\circ - B$, since zenith-to-horizon = 90°).
This in turn is equal to the angle opposite (angle B), in the triangle bounded by the horizon above (side a) and side L_1 (side c) below, with side b (side b) opposite enclosing a right-angle (angle C) with the horizon. By use of the sine-formula:

$$\frac{\sin B}{\sin b} = \frac{\sin C}{\sin c}$$

$$\frac{\sin (90^\circ - B)}{\sin b} = \frac{\sin 90^\circ}{\sin L_1}$$

$$\frac{\sin L_1}{\sin b} = \frac{\sin 90^\circ}{\sin (90^\circ - B)}$$

$$\sin L_1 = \frac{\sin 90^\circ \cdot \sin b}{\sin (90^\circ - B)}$$

Since $\sin 90^\circ = 1$, and $\sin (90^\circ - B) = \cos B$:

$$\sin L_1 = \frac{\sin b}{\cos B}$$

hence, $L_1 = \sin^{-1} (\sin b \,/\, \cos B)$.

24i) $L = 10^\circ.3$
$L_1 = 13^\circ.2$
$S = L + L_1 + 90^\circ = 113^\circ.5$

We are now in *ecliptic* coordinates, with the sun on the meridian and of course on the ecliptic, while Sirius is rising or setting.

The angular distance *along the ecliptic* from the sun on the meridian to its position on the horizon is 90°. (This is not its zenith distance, but a measure of how the horizon intersects with the great circle of the ecliptic.)

The ecliptic longitude is measured eastwards from *a*. So moving from the meridian to the point of rising increases the longitude of the sun, while moving from the meridian to the point of setting decreases it. Therefore, for HR and AR we must add 90° to move the sun to the rising point, but subtract 90° to move it to the setting point. The same applies to L_1.

$L + L_1 + 90^\circ$ = the ecliptic longitude of the sun at the time of the heliacal rising of the star. From this the date of the phenomenon can be ascertained.

NB: these are ecliptic coordinates, which have to be converted to equatorial.

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Image, text, and pattern: Reconstructing parapegmata

Daryn Lehoux

Introduction

Parapegmata are among the oldest astronomical instruments from the classical world, and are closely related to the earliest astronomical/astrological tradition in Greece, that of stellar astrometeorology.[1] At its most basic, a parapegma is an instrument for keeping track of a temporal cycle or cycles. In an inscriptional parapegma, holes are drilled in a stone or in a wall, and a peg is moved from one hole to the next each day. Astronomical, astrological, calendrical, or other information is inscribed beside each hole and the user simply looks for the peg or pegs to locate themself in the relevant cycle for that day. There are extant parapegmata of different kinds from the fifth century BC through to the Middle Ages. The basic technology of a parapegma was adapted to a range of different uses, often serving as a complement to a calendar, tracking temporal cycles that are not tracked by the local calendar.

In looking at the ways we might reconstruct parapegmata, we are faced with first needing to determine just what kind of information was being tracked by a particular parapegma, and then to look at what kinds of clues are available to us for effecting a reconstruction. These clues fall into three broad classes: internal, comparative, and external. Internal evidence might include fragmentary words or phrases or considerations of physical structure. Comparative evidence is what we can glean from looking at comparable cycles in other parapegmata for clues to what may be missing in a damaged text. What I am calling external evidence includes the use of modern calculations of the stellar phases expected for a given time and place, or the use of modern weather observations for judging the likelihood of a particular meteorological prediction. We shall see that some cycles tracked by parapegmata are easily reconstructed, while others defy our efforts. Paradoxically, I argue that the class of parapegmata that is best attested, astrometeorological parapegmata, turns out to be the least amenable to reconstruction.

Image

Let us begin with a look at an astrological parapegma, the Thermae Traiani Parapegma (see Fig. 2).[2] This parapegma was unearthed in the early 19th century as a graffito in the wall plaster of a Roman house near the baths of Trajan on the Esquiline hill. Two drawings of the parapegma survive. A little-known sketch was made by Piale in 1816, but the usual drawing that is reproduced in the modern literature was made by de Romanis in 1822.[3] The parapegma itself seems to have

1 See Lehoux, 2007, 2006, 2005; Hannah, 2009, 2005; Taub, 2003; Evans and Berggren, 2006; Bitsakis and Jones, 2016; Bevan, Jones, and Lehoux, 2019; Anastasiou *et al.*, 2013; Evans, 1998; Rehm, *Parapegmata, RE.*

2 Figs. 1 and 2 are reproduced from de Romanis, 1822, courtesy of the Thomas Fisher Rare Book Library, University of Toronto. For my classification of parapegmata (astrological, astrometeorological, astronomical, etc.), see Lehoux, 2007.

3 Piale published in Guattani, 1817, pl. XXII, 160-162; de Romanis, 1822.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 109–131. Chapter DOI: 10.5281/zenodo.3975725.

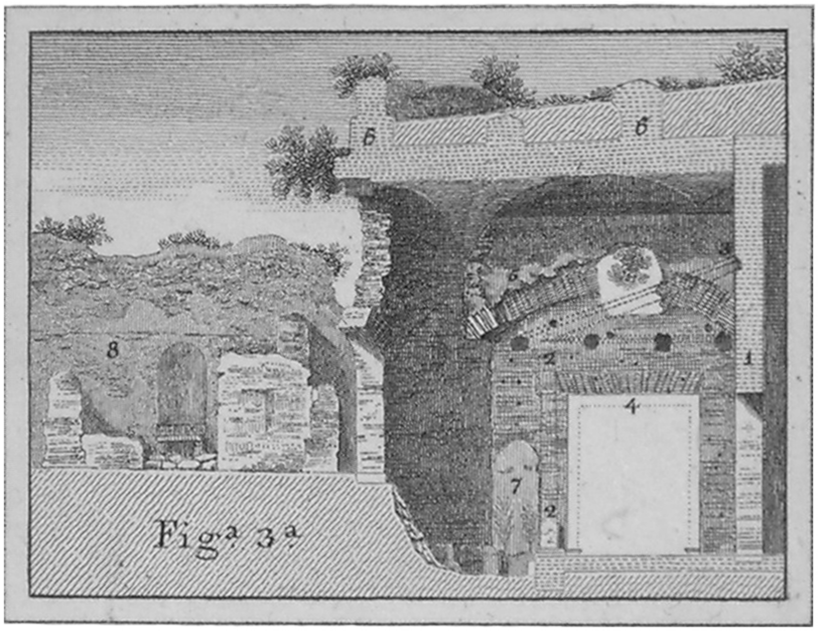


Figure 1. The Thermae Traiani parapegma was inscribed in room 8, on the right-hand wall.

been left exposed to the elements (see Fig. 1) and disappeared shortly thereafter. Further, there is an "improved" terracotta copy, made by a workman named Ruspi in the early 19th century (either from the original or from a drawing), which has turned up at the University of Würzburg. Finally, a plaster cast of the Ruspi copy was found in Rome in the early 1980's,[4] and copies of the Ruspi rendering can now be purchased from the Würzburg museum gift shop.

On the Thermae Traiani parapegma, we find five of the seven deities of the astrological week, reading from left to right. Beginning with a gap (where Saturn should be), we find Sol, Luna, Mars, Mercury, then a blank for Jupiter, and finally Venus, in their traditional order. The numbers from I-XV run vertically down the left side, and from XVI-XXX down the right. In de Romanis' illustration, a hole seems to appear just above and to the right of the hole for XXX.[5] In the middle of the parapegma are the signs of the zodiac, with two holes drilled per sign and running counter-clockwise. A small fragment of a bone peg was found in one of the holes for Gemini.

From simple considerations of functional symmetry and on the basis of the other surviving holes, we can conclude that there must have originally been peg holes beside the numbers I through IV and XXV, and we know the missing deities to be Saturn and Jupiter. Matters of artistic style and iconography defy precise reconstruction, so we can name the missing gods but modern scholars decline to reproduce them pictorially. This point may seem trivial, but Ruspi's Würzburg copy did in fact reconstruct the missing images, and this version is sometimes published in modern accounts without mention of the restorations.[6]

The inscription poses some more difficult problems, specifically in trying to decide what to do with the apparent 31st hole between days XXVIIII and XXX. Is it part of the original parapegma or is it an artifact of the copyist? Is it simply damage incurred by the parapegma at some

4 See Manicoli, 1981.

5 On the significance of this apparent hole see Rehm, 1893-; Erikkson, 1956; Lehoux, 2007.

6 McCluskey, 1998, for example, includes an image of the Ruspi copy. Goessler's, 1928, speculation that Ruspi's copy may have been made before the inscription was damaged is rejected by Stern, 1953, 177 n. 3.

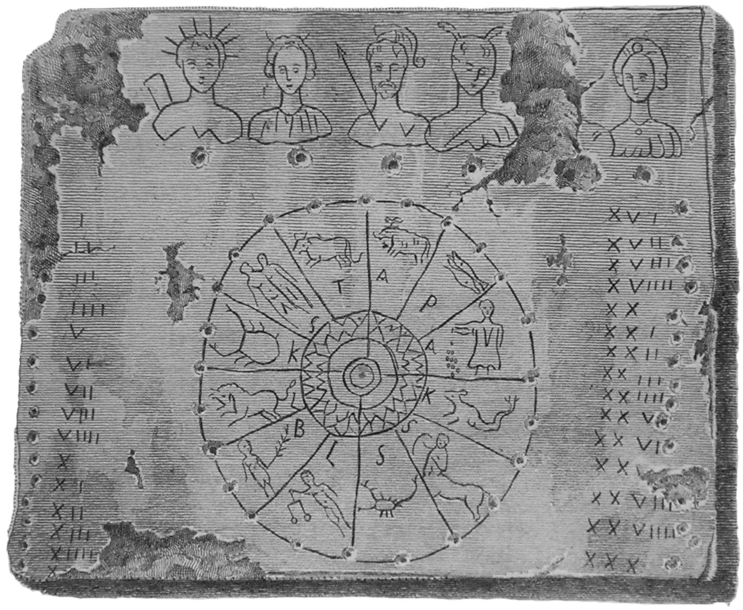


Figure 2. The Thermae Traiani Parapegma: de Romanis' 1822 illustration.

point after excavation? Or perhaps something had been nailed to the wall in antiquity, and so the hole has nothing to do with the parapegma at all.[7] Small as the hole is, the issue is of some consequence in determining the use of the column of numbers, and the use of the parapegma as a whole.

If there are thirty-plus-one holes in the parapegma, then that opens up the possibility that the column was meant for counting calendar dates in the Roman calendar, where any given month could have up to 31 days, but not more.[8] Exactly 31 holes, however, would likely rule out the use of these columns for counting the number of days the sun spends in a given zodiacal sign, as on all the classical schemata known to me there is at least one sign in the year in which the sun spends 32 days.

If, on the other hand, there were only 30 holes in the original parapegma, then the most obvious candidate for what is being tracked would be lunar days. Lunar days count from the first day (probably either full or new moon) to the 30th day and then start again. These lunar days are attested as having an astrological significance in many Roman texts, including Virgil's Georgics.[9] Lunar days are also easily equated with the significant lunar phases which were variously lucky and unlucky for a wide range of activities.[10] Finally, in the description of a parapegma in Petronius' Satyricon, we see what sounds like a combination of lunar days and the seven-day week being tracked for good and bad luck.[11] This hypothesis that the numbers I through XXX are meant to track lunar days is greatly strengthened by comparative evidence, since 30-day sequences are

7 As is the case with the Ostia Hebdomadal Deities (see Lehoux, 2007; Becatti, 1954, 116-7; pl. XXXVIII.3).

8 McCluskey, 1998, 57, for example, thinks the numbers track days of the month. See also Degrassi, 1963, vol. XIII.2, 308-309; Rehm, "Parapegma", *RE*, col. 1364; Erikkson, 1956

9 He says that the seventeenth day of the moon is propitious for planting vines, and the ninth day lucky for fugitives and unlucky for thieves. See *Georg.* I.277-8; Pliny, *NH* XVIII.21.

10 See e.g., Columella, *RR* II.x.10, XI.ii.85; Pliny *NH* XVIII.314.

11 See Lehoux, 2007; Petronius, *Sat.* 53.

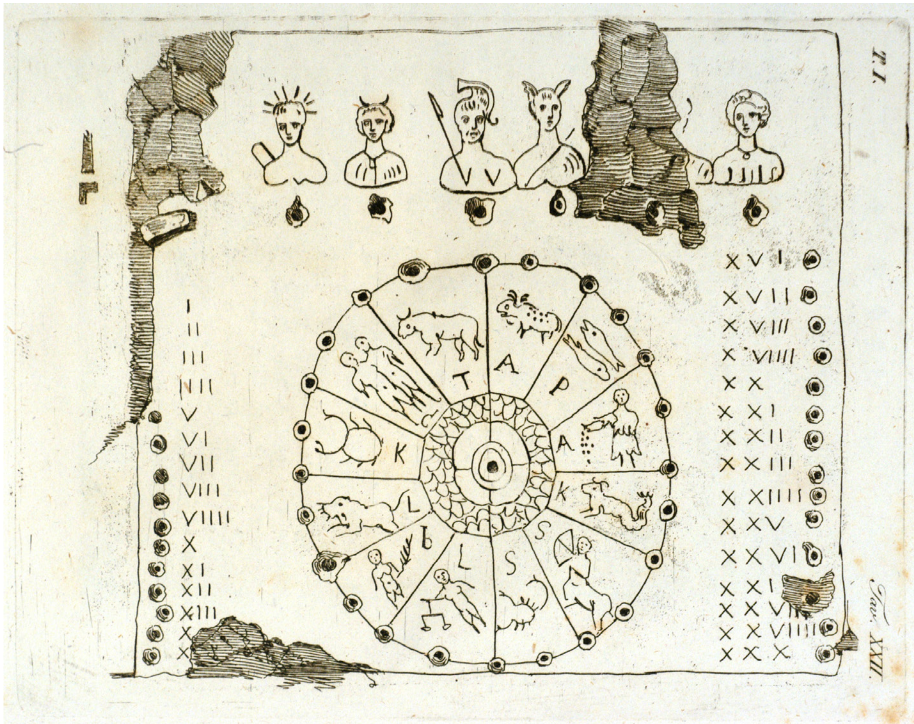


Figure 3. Piale's 1816 illustration of Thermae Traiani. Reproduced from Guattani, 1817. Note the difference in how the "31st hole" is represented.

common in Latin parapegmata, and no parapegma has a sequence that counts upwards to 31. Moreover, in the unique Trier Parapegmatic Mold, meant for casting parapegmata in clay, we see 30 holes inscribed (15 down each side) accompanied by images of changing lunar phases.[12]

A related possibility, but one that still finds a use for a 31st hole, was proposed by Erikkson, following Piale.[13] He agrees that the 30 holes track the lunar cycle, but he speculates that a 31st hole was used to distinguish between full (30-day) and hollow (29-day) months following some schematic rule that would allow the user to determine in advance whether the current lunar cycle was going to be full or hollow. A peg in the 31st hole, situated between day 29 and day 30, would thus act as a stopper, telling the user to skip the 30th hole and go back to day one.

Thus we have three possibilities: (1) there is a 31st hole, either (1a) as part of a calendrical cycle, or (1b) for indicating full or hollow lunar cycles, or (2) there is no 31st hole. As we move on to look at other parapegmata, we will see that only possibility (2), that there is no 31st hole, is supported by comparative evidence. Furthermore neither the Ruspi copy, nor Piale's 1816 illustration[14] (see Fig. 3) of the inscription show a 31st hole. This strongly supports the hypothesis that the hole was damage subsequent to excavation, or else an artifact of de Romanis' copy.

12 The mold is currently in the Rheinisches Landesmuseum Trier.

13 Erikkson, 1956, 24-5; Piale in Guattani, 1817, 161.

14 In Guattani, 1817.

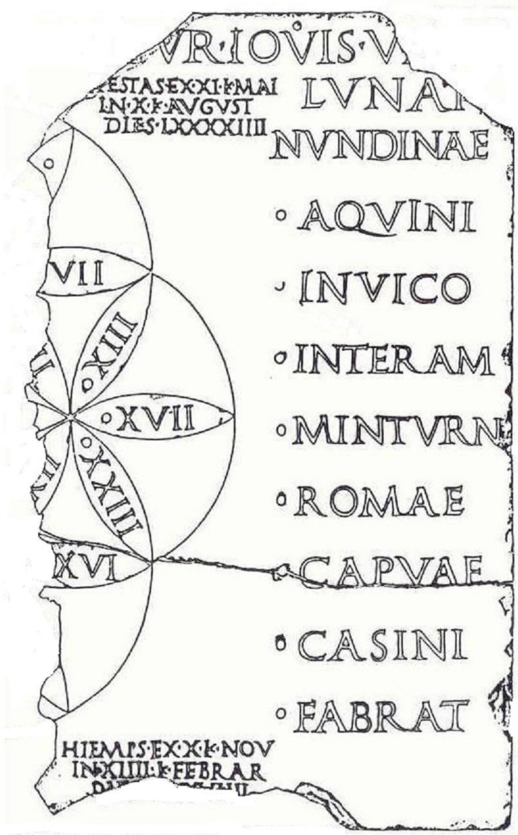


Figure 4. The Latium Parapegma.

Text

Let us turn now to another inscriptional parapegma, the Latium Parapegma (see Fig. 4).[15] At first blush, it is not obvious that this parapegma is at all related to Thermae Traiani. But as we shall see as we work through the reconstruction,[16] there are some familiar details, in spite of the immediately obvious differences.

The first difference to note is that this is a much more carefully executed and aesthetically polished inscription than Thermae Traiani. It is carved in marble (approx. 53.5 cm high, 33 cm wide, and 3 cm thick), and quite possibly meant for public display. The second difference lies in how this parapegma is predominantly textual, where Thermae Traiani was iconographic. Like Thermae Traiani, we see some numbers inscribed, and we see at the very top the fragmentary remains of the names of the days of the seven-day (hebdomadal) week: *...]ur · Iovis · V[...* which we can reconstruct as:

15 First Published by Gruterus, 1707. Later by Henzen in *CIL* VI.4.2, no. 32505. More fully reconstructed by Degrassi, 1963. It is currently in the Museo Archeologico Nazionale di Napoli, inv. 2635. Drawings courtesy of the Unione accademica nazionale and reproduced with permission.

16 My reconstruction follows Henzen and Degrassi quite closely, up to a point, but it will be worthwhile to go through all the steps in more detail than they did, to show how different kinds of pattern internal to the text impel us towards the various aspects of the reconstruction. I do not claim that the reasons I give are the same as theirs although it seems likely enough that they were thinking along similar lines.

∘] ∘ [∘

Merc]ur · Iovis · V[eneris

As we look to the rest of this parapegma we notice elements not included in Thermae Traiani, including a complete column of nundinal days, the older Roman eight-day week named after towns in a nominal market itinerary, and some partly damaged dates for the beginning and end of summer and winter, just to the right of the top and bottom of the numbered floral pattern. At the top of the rosette we can read the following:

> *[A]estas ex XI K. Mai. in X K. August. Dies LXXXXIIII*
>
> Summer is from the 11th day before the Kalends of May until the tenth day before the Kalends of August: 94 days.

And at the bottom of the rosette,

> *Hiemps ex X K. Nov. in XIIII K. Febrar. [Dies LXXXVIIII]*
>
> Winter is from the tenth day before the Kalends of November until the 14th day before the Kalends of February: 89 days.

Considerations of symmetry lead us to presume that the two remaining seasons would have originally been on the left of the rosette. In an attempt to reconstruct the lengths and dates of these two seasons, we suppose that the beginning of spring is the day after the end of winter (rather than, say, at some specific time on the same day), and that the beginning of autumn is the day after the end of summer (in Roman agricultural texts seasons regularly have lengths in full rather than partial numbers of days).[17] We thus get

> *[Ver ex XIII K. Febrar. in XII K. Mai. Dies LXXXXI]*
>
> Spring is from the eleventh day before the Kalends of February until the twelfth day before the Kalends of May: 91 days

and

> *[Autumnus ex IX K. August. in XI K. Nov. Dies LXXXXI]*
>
> Autumn is from the ninth day before the Kalends of August until the eleventh day before the Kalends of November: 91 days.

Pattern

A further difference between Latium and Thermae Traiani is that in the Latium Parapegma, we have the numbers inscribed in the middle, in what appears to be a floral pattern. Following Hen-

17 On Roman seasonal dates, see Hannah, 1989.

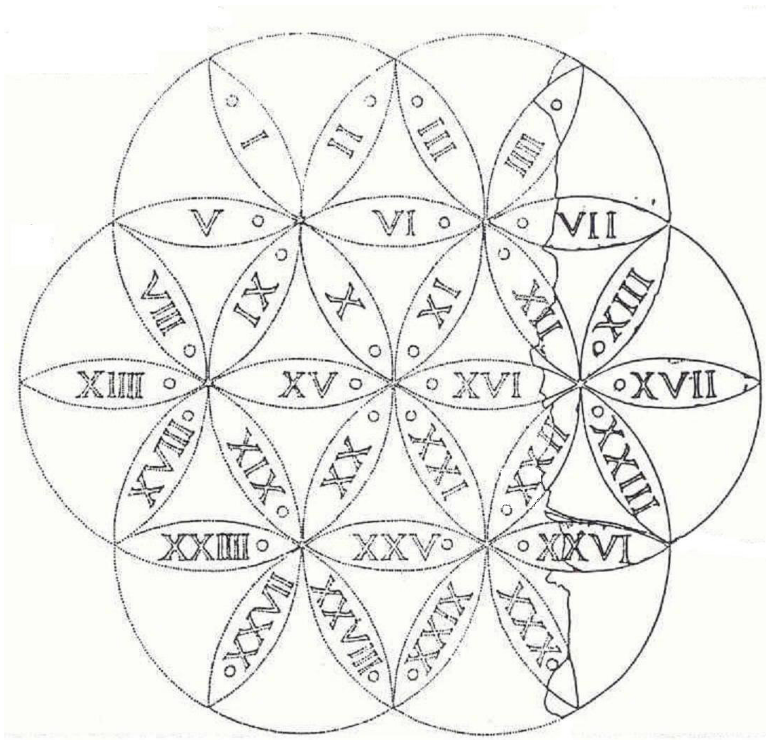


Figure 5. The Reconstructed Latium Rosette.

zen, we can reasonably assume that the rosette is symmetrical, and so it can be reconstructed as in Fig. 5. We should notice here that only the numbers from I-XXX will fit within the rosette itself. This will count against the hypothesis that the numbers are meant to count calendrical dates or zodiacal days. Finally, the very fact that the numbers in the rosette are reckoned upwards from I through XXX, while the calendar in which the seasons are measured is still the traditional Roman one (counting down to the Kalends, Nones, and Ides), is further evidence that these numbers were not used to count calendar dates. This helps to further rule out the possibility that there was an unnumbered peg hole for day 31 in Thermae Traiani.

Some hint of the function of the days numbered in the rosette comes from the word inscribed just above the column of nundinae. Following Degrassi, we suppose that the single word *luna(r)* is grammatically attached to something. Degrassi hypothesizes that the word *dies* originally appeared on the opposite side of the parapegma, to complete the phrase *dies luna(res),* "Lunar Days" as an explanation for what the numbers in the rosette represent. We shall see that comparison with inscriptional dates, and with other parapegmata strongly confirms Degrassi's supposition that the numbers I-XXX are in fact lunar days, and so, I think we have some reason for inserting *dies* on the left side of the parapegma.

Degrassi goes still farther than we have by hypothesizing that down the left side of the parapegma, in what is a conspicuous blank in so symmetrical an inscription as this is turning out to be, there was another set of nundinal days. But here considerations of symmetry may have pushed him too far. There is simply no reason why a single parapegma would need to list more than one set of nundinae, and there is no single inscription, to my knowledge, that has more than one.[18]

18 Although Degrassi thinks the *Allifae Nundinal Lists* were originally part of a single inscription.

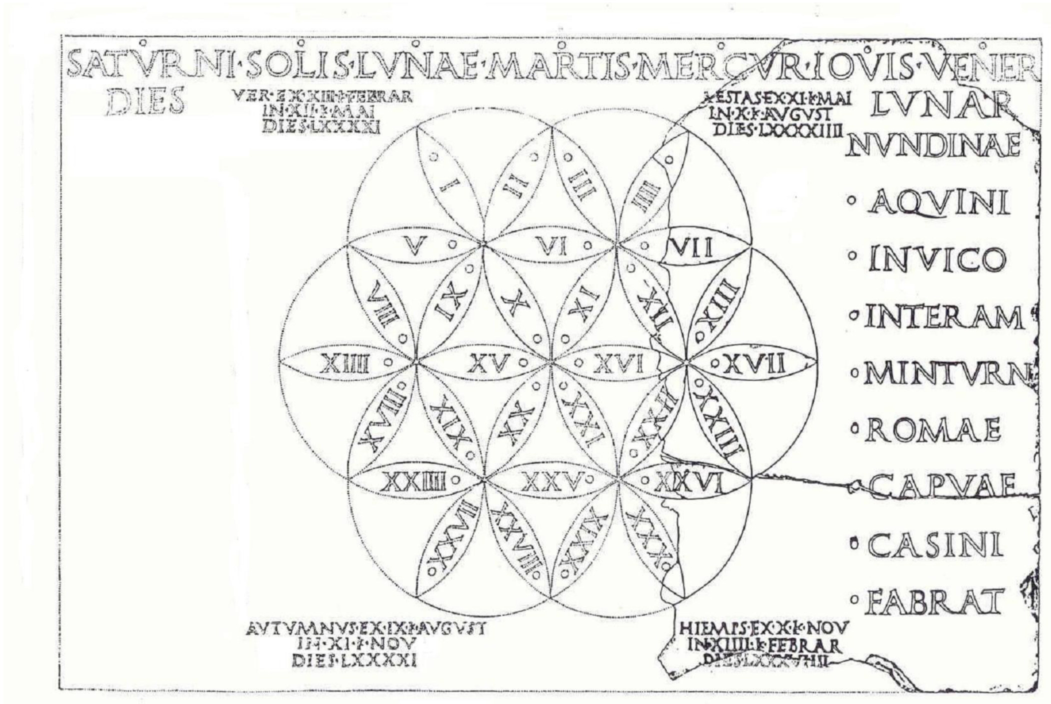


Figure 6. The Reconstructed Latium Parapegma.

Stopping, then, just short of Degrassi, we are left with a fairly complete parapegma, as we see in Fig. 4, with only the left-hand column still a mystery. One reasonable possibility, following Thermae Traiani, would be a list of the twelve zodiacal signs.

The Dura-Europus Parapegma

A remarkable graffito-parapegma (see Fig. 7), found scratched in a wall in a makeshift Roman barracks at Dura-Europus in Syria shows some of the features of each of the two Latin parapegmata we have seen so far.[19] This is a wonderfully crude rendering, with rough versions, additions and deletions still visible. We see the hebdomadal deities, all at least partly preserved across the top, with a few peg holes still intact. Down the left and right sides we see the heading [L]una, followed by the numbers from I through XXX, with an underscore (lucky for us) under the XXX to indicate that the series is complete. Like Thermae Traiani, this parapegma has a history that complicates its reconstruction. The parapegma was badly damaged during excavation and the plaster crumbled, destroying the inscription almost entirely. The drawing was made from the collective memories of the archaeologists, with the partial aid of one photograph. Thus we cannot be certain of many of the details even of the drawing we have before us.

But the big question that emerges from this text has to do with the column that may be for tracking the local nundinal day. The nundinal day is the nominal market-day for a given Italian town. This was reckoned from archaic times onward every "ninth" day on the Roman style of counting (every eighth day counted as we would do), and this eight-day cycle came to define the earliest Roman version of the week, eventually complemented and later supplanted by the

19 See Rostovtzeff, Bellinger, *et al.*, 1936. Image courtesy of Yale University Press.

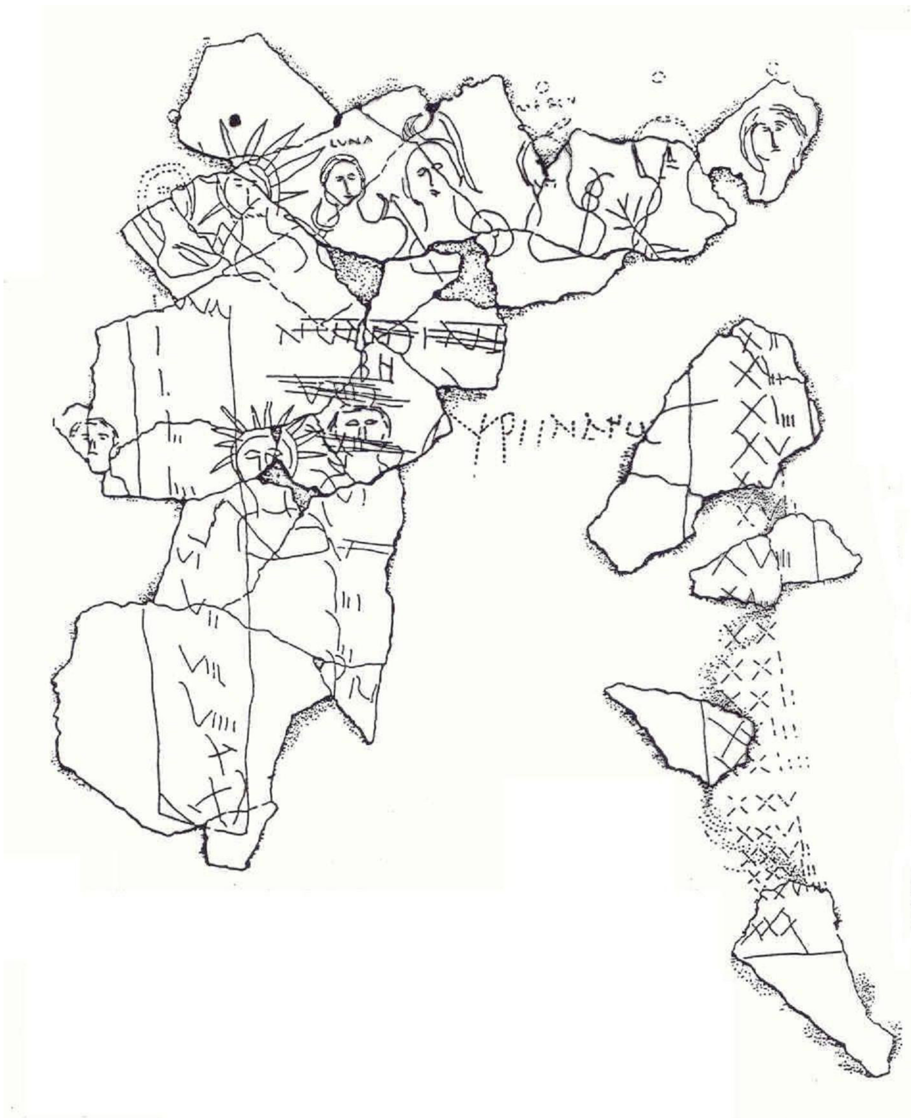


Figure 7. The Dura-Europus Parapegma.

seven-day (astrological) week.[20] The local market day was a holiday from agricultural work, and farmers could come to town to exchange wares and produce, as well as to keep up on local affairs.[21] Various *fasti* have the days of the month labeled consecutively from A through H (called the "nundinal letters"), where one of these days, would be the local market-day.[22] There are also nundinal lists, as we have seen in the Latium Parapegma, which have the names of eight different towns inscribed. This has usually been read as indicating that the market day occurred

20 On the nundinae and nundinal lists generally, see MacMullen, 1970; Deman, 1974; Tibiletti, 1976-7; de Ligt, 1993; Andreau, 2000; Marino, 2000; Rüpke, 1995, 2000; Lo Cascio, 2000; Ker, 2010.

21 Macrobius, *Sat.*, lists the nundinæ as *feriæ* (I.16.5) but points out that there was a divergence of opinion in antiquity on the matter (see I.16.28-31). See also Macrobius, *Sat.* I.16.34

22 On *fasti,* see Degrassi, 1963, vol. XIII.2; Michels, 1967, 23 f.; and especially 187-190; Radke, 1990; Rüpke, 1995. On nundinal letters, see Rüpke, 2000; Michels, 1967.

in eight different towns on eight different days, such that the market day in Rome was at least nominally followed by that in Capua, then Calatia, etc., and then it would be market day in Rome again after eight days.

Looking then at the column headed *nundine* in the Dura-Europus parapegma, if this is indeed a nundinal column, it is reckoned in a new and unique, way. The column seems to read:

~~NUNDINE~~
~~VIII~~
~~VII~~
VI
~~I~~
IIII
III
PRI

Snyder argued that this column should be read as counting down to the local nundinal day in a manner analogous to the way in which the Roman calendar counts down to the Kalends, Nones, and Ides.[23] On this reading, the local nundinal day is indicated simply with the word *nundine*, and the rest of the days are counted down ordinally from VIII, ending with *pri(die)* then going back up to *nundine.*

Snyder's reading does fit a story we can provisionally tell. This column would be the only example of a nundinal list found outside of Italy. The author, stuck in a military outpost at the eastern edge of the Roman empire, may have been in a situation where the local market days were not regulated according to the Italian scheme of nundinae, and the names from home would make little sense to use. This is particularly the case since the names of nundinal days in individual Italian towns seem to have been, as we shall see in the next section, unique to each town, and so for the "artificial" community of soldiers in the barracks, any one list would have been unsatisfactory. The author thus resorted to counting down to the local nundinal day in the best way he knew how: by simple analogy to the calendar.

This is certainly a possibility, though I would not conclude from this that, even if it is correct, counting down to the local nundinal day was common anywhere outside these barracks.[24]

Problems Reconstructing Nundinal Lists

There are a number of parapegmata with complete or fragmentary lists of the hebdomadal days, and there are also some parapegmata with only nundinal days listed. Where it is a simple matter to reconstruct any missing hebdomadal days,[25] we are not so lucky with the nundinal days. It turns out that it is very difficult to get our extant nundinal lists to agree with each other.

A fragmentary parapegma from Pausilipum (transcribed in Fig. 8) has partial lists of both hebdomadal days and nundinae. It is a straightforward matter to complete the hebdomadal row with *[Mercur · Iovis · Veneris],* but if we look at the comparative evidence for the nundinal row, we find that evidence to be terribly conflicted.

23 See Snyder, 1936.

24 Snyder, 1936, for example, argues that this was the normal way of counting nundinae.

25 With the exception of the Mithraic hebdomadal deities, the ordering of the seven-day week is standard in antiquity just as it is today. Espérandieu thinks he has found one case of an alternate ordering from Gaul, but I have argued against this. See Lehoux, 2007, 177-8.

SATVR	SOLI	LUNAE	MARTIS
ROMAE	CAPVAE	CALATIAE	BENEV

Figure 8. The Pausilipum parapegma.

Beginning with two of the best preserved nundinal lists extant, the Pompeii Calendar and the Latium Parapegma, we find the following nundinae:

Pompeii Calendar	*Latium Parapegma*
Pompeis	*Aquini*
Nuceria	*In vico*
Atella	*Interam(na)*
Nola	*Minturn(ae)*
Cumis	*Romae*
Putiolos	*Capuae*
Roma	*Casini*
Capua	*Fabrat(eria)*

The ordering of the last two entries in Pompeii (Roma followed directly by Capua), agrees with two of the entries in the Latium Parapegma. We can compare details of these and other nundinal lists by reordering the lists to emphasize overlap where possible and counting the nundinal days from there,[26] as follows:

Pompeii	*Latium*	*Pausilipum*	*Allifae I*	*Allifae II*	*Suessula*
Rome	Rome	Rome	Interamn[a]	[Ca]les	Cales
Capua	Capua	Capua	Telesia	[Sues]sula	[.........]
Pompeii	Casinum	Calatia	Saepinum	[Sin]uessa	[.........]
Nuceria	Fabrateria	Benev[entum]	Puteoli	[Ta]tinie	Campania
Atella	Aquinum	[.........]	Atella	[...]en[...]	Atella
Nola	*in vico*	[.........]	Cumae	Nucer[ia]	Suessula
Cumae	Interamna	[.........]	Nola	[L]uceria	Nola
Puteoli	Minturnae	[.........]	Altinati	[S]uessa	Cumae

What we find is that no two nundinal lists are identical, each containing a different subset of towns. More significantly, no two lists agree on the relative order of more than just two towns. We find Rome followed by Capua in every list that includes either place, but none of those lists has any of the same days thereafter (although the Pausilipum list is admittedly missing four days). If we look at the three lists that include Atella, Nola, and Cumae, plus one that includes a different overlapping subset, and concentrate only on days that actually overlap, we find:

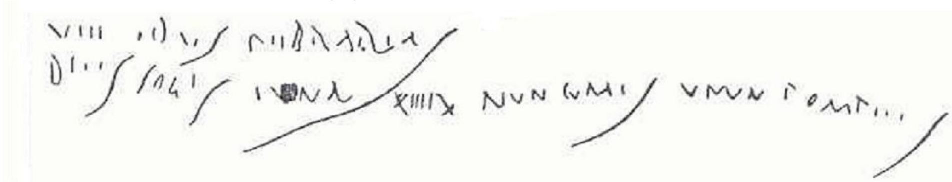
26 This does not change the relative ordering of the days in the list.

Pompeii:	*Nuceria*	Atella	Nola	Cumae	Puteoli		
Allifae I:	Puteoli	Atella	Cumae	Nola			
Suessula:		Atella	*Suessula*	Nola	Cumae	*Cales*	
Allifae II:			*Nuceria*			*Cales*	*Suessula*

Thus we find Atella, Nola, Cumae in the first list, but Cumae and Nola reversed in the second. These two lists also disagree on where Puteoli should be relative to the three, one first putting it after the set, the second before. The Suessula list agrees with neither, putting Suessula in between Atella and Nola, three days before Cales, where Allifae II puts Suessula the day after Cales. There is simply no way to reconcile these differences.[27]

Part of the reason for the discrepancies in the order of the nundinal days as preserved in different inscriptions may just be that for the readers of any one inscription in a particular locality like Pompeii, only the local nundinal day would matter. So long as there are seven other days in between, marked by seven other towns, no discrepancy with the actual nundinal days of these towns (or with other lists from still other towns) will be immediately apparent or even particularly significant. The usefulness for someone in Pompeii of knowing the actual nundinal day in Rome would be marginal. Of course, itinerant traders are attested in numerous inscriptions,[28] but the distances between the nundinal towns on any given list, and the often nonsensical back-and-forth orderings, show that the lists are probably not meant to be read as regular "circuits" even for these merchants.[29]

There is evidence that the nundinal day had some significance in dating formulae. Look at the date formula in the following graffito from Pompeii:[30]



VIII Idus Febrarias
dies Solis, Luna XIIIIX, nun(dinae) Cumis. V (Idus Febrarias?) nun(dinae) Pompeis.

VIII Ides February
Sunday, 16th day of the moon, nundinal day of Cumae. V (Ides February?) nundinal day of Pompeii.

27 Although attempts have been made. Deman, 1974, argues that the *Suessula* and *Pompeii* lists "concordent parfaitement", but I am unconvinced. He claims (based on comparison of the different *Allifae* lists) that Atella and Suessula are interchangeable, as are Puteoli and Cales. He then assumes that *Suessula's* "Atella, Suessula, Nola..." should be read as if it were "Atella *and* Suessula (together on one day), Nola (the next day)..." He does, however, concede that there is a lot a disaccord elsewhere. Tibiletti, 1976-7, tries to reconcile several lists, but his attempt forces him to see a seven-day nundinal cycle in the Latium parapegma, forcing him to ignore one of the eight peg holes.

28 See MacMullen, 1970.

29 See de Ligt, 1993, 115, although he thinks the Suessula list may be an exception; see also MacMullen, 1970, 340.

30 From *CIL* IV, Suppl. 2, no. 4182. Snyder, 1936, reads this text quite differently. Image courtesy of the *Corpus inscriptionum latinarum.*

On the usual reading of this inscription, we see calendar dates for the beginning and end of a three-day period. Corresponding to the VIII Ides of February, the day of the hebdomadal week (Sunday), the lunar day (XIIIIX), and a nundinal day *(nun. Cumis)* are given. Snyder has proposed a different reading such that the second date would be *V nun(dinas) Pompeis,* which is to say as implying that the nundinal day *Cumis* equals the fifth day before the nundinal day of Pompeii. I think it makes more sense here to follow the editors of the CIL in reading the entry as a second date, three days later, which was *nun. Pompeis.*[31] The abbreviation of the second date to just a numeral is possible because of the proximity of the full date formula *(VIII Idus Febrarias)* just before it.[32] In two dates mentioned back to back like this we need not expect the full rendering of the date both times. Compare Petronius: *hoc habebat inscriptum: "III. et pridie Kalendas Ianuarias..."*

Calendrical Columns

One text closely related to these parapegmata has columns for tracking calendrical cycles, in addition to columns for hebdomadal days and nundinae. Fig. 9 reproduces a drawing of the so-called Pompeii calendar.[33] Here we see well preserved, labeled columns for the hebdomadal days, the nundinal days, three columns for a calendrical cycle (notice the PRI(die) and K(alends) in the fourth column, and the Non(es) and Ides in the fifth), and lastly, three columns for the numbers I through XXX, as we saw in both Latium and Thermae Traiani.

The final group of columns shows some damage at line 2 of its middle column, but restoration is simply a matter of completing the numerical series to get *XV, [X]VI, XVII ....* Things get a little more interesting, however, in the calendrical columns. In the reproduction here, we see some damage indicated between the first and second column, from the top down to the level of line 6. The leftmost column can be restored just by counting up from the first complete number, *XV,* such that the column should be read *X[VIIII], X[VIII], XV[II], XV[I], XV, XIV, ...* But something strange happens in the middle calendrical column. Reading down from the top, we have: *VIII, VII,*

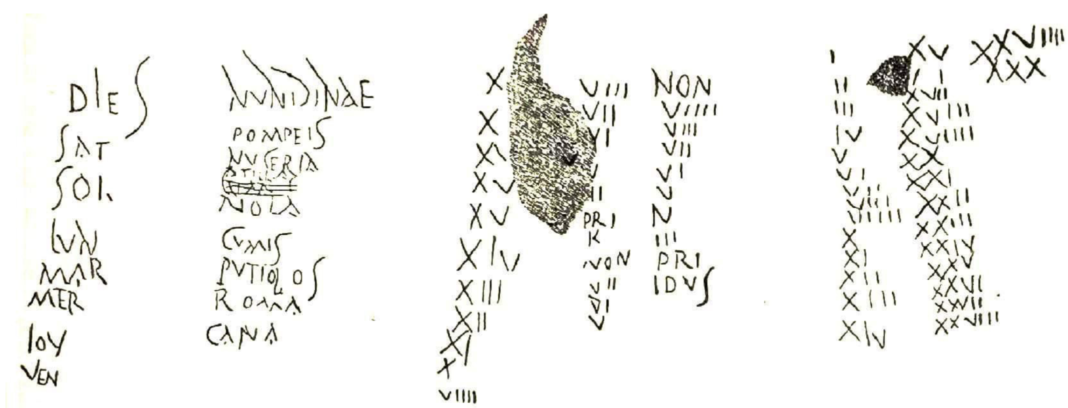


Figure 9. The Pompeii Calendar.

31 I note, for what it is worth, that this ordering disagrees with the Pompeii calendar, where *nun. Pompeis* is four days after *nun. Cumis.*

32 Another possibility (if a remote one) for reading this text is that the author is simply confused. Deman has argued that *dies solis* seems to be a mistake, since, given the consular year higher up in the inscription, the VIII Ides Feb. should be a *dies Mercurii.* Could the *V* be an abbreviation for *vel?*

33 Published in CIL IV, no. 8863. Image courtesy of the *Corpus inscriptionum latinarum.*

VI, [V], [I]V, [I]II, PRI, K, then an unclear entry, followed by *VII, VI, V.* The leftmost column and most of the middle one are clearly for the reckoning of dates in the Roman style, counting down from *XVIIII K.* to *III K., Pridie,* and the Kalends.

If we look to the right-hand calendrical column, we see that it also counts down from the Nones to the Ides, as follows: *Non, VIIII, VIII, VII, VI, V, IV, III, Pri, Idus.* But something funny has happened between the Kalends and the Nones in this list. Although no damage to the wall is indicated in the CIL drawing, we must surmise, for the sake of consistency, that three lines are missing from the bottom of the middle column, in order for it to properly count down to the Nones, thus we restore: *VII, VI, V, [IV, III, Pri]* in the middle column. Degrassi tries to solve this problem differently, supposing instead that the *VII, VI, V* are mistakes for *IV, III, Pri,* but this strikes me as implausible, given that the lack of a *Pridie Non.* should have been immediately obvious to the author, and also because *IV, III, Pri* would only be useable for a short month, not a 31-day month, whose Nones counted down from *VI,* not *IV.*

Perhaps the most troublesome line is the entry immediately after the Kalends. It looks to read something like *IVON,* and has been read as a month name by both Della Corte and Degrassi: *NOV* by Della Corte, and *IAN* by Degrassi. On either interpretation, the list is being interpreted as a calendar for the dates around the Kalends *of a particular month,* beginning with *XVIIII K.* and ending with the Ides. But this does not fit the general nature of the document as a whole, which is, like other parapegmata, a listing of four kinds of cycles (hebdomadal, nundinal, calendrical, and lunar) in their entirety. If it was meant for a particular month, then what would be the point of having four separate cycles of differing periodicities written out just once each, and each starting from their beginnings? Why is each cycle listed from its beginning, and not *in media res,* as we should expect if it were a particular month and its weeks and lunar cycle being tracked? (The odds of all these cycles beginning on the same day are less than one in 50,000.)

Taking it as a perpetual calendar, I propose to read the line after the Kalends as part of the numerical sequence counting down to the Nones, that is, as *VIII* rather than as a month name, and this would make the calendar section agree with the spirit of the rest of the document. I thus reconstruct it as follows:

DIES	NUNDINAE	X[VIIII]	VIII	NON	I	XV	XXVIIII
SAT	POMPEIS	X[VIII]	VII	VIIII	II	[X]VI	XXX
SOL	NUCERIA	XV[II]	VI	VIII	III	XVII	
LUN	~~CUMI~~ ATILLA	XV[I]	V	VII	IV	XVIII	
MAR	NOLA	XV	[I]V	VI	V	XVIIII	
MER	CUMIS	XIV	[I]II	V	VI	XX	
IOV	PUTIOLOS	XIII	PRI	IV	VII	XXI	
VEN	ROMA	XII	K	III	VIII	XXII	
	CAPUA	XI	VIII	PRI	VIIII	XXIII	
		X	VII	IDUS	X	XXIV	
		VIIII	VI		XI	XXV	
			V		XII	XXVI	
			[IV]		XIII	XXVII	
			[III]		XIV	XXVIII	
			[PRI]				

Astrometeorological Parapegmata

The final class of parapegmata is in many ways the most interesting and the best attested. These are the astrometeorological parapegmata, used for correlating annual stellar appearances and disappearances with weather predictions. Such correlations form the core of the earliest attested astronomy in Greece, dating to as early as Hesiod's Works and Days. For example, Hesiod tells us:

> Fifty days after the solstice, at the arrival of the end of the season of weary heat, that is the time for mortals to sail. ... Then are the winds orderly and the sea propitious.[34]

Parapegmata for tracking astrometeorological phenomena were very useful in Greece, where the lunar calendar wandered in and out of the seasons. In this class of parapegmata the vast majority of examples are attested in literary sources rather than inscriptions. In these literary parapegmata, where there was no peg to indicate the current day, some kind of calendar or other temporal tracking device was used to help the reader situate the current day in the cycle. Look at this excerpt from Columella's parapegma:

> V Kal. Febr. Auster aut Africus, hiemat, pluvius dies.
> III Kal. Febr. Delphinus incipit occidere, item Fidicula occidere, significat.
> Pridie Kal. Febr. eorum, quae supra, siderum occasus tempestatem facit, interdum tantummodo significat.
> Kal. Febr. Fidis incipit occidere, ventus Eurinus et interdum Auster cum grandine est.
> III Non. Febr. Fidis tota et Leo medius occidit....

> V K. Feb. south wind or south-west wind; it is wintry; rainy day.
> III K. Feb. Delphinus begins to set, likewise Lyra (begins) to set; there is a change in the weather.[35]
> Pri. K. Feb. The setting of those stars, mentioned above, causes a storm, sometimes there is only a change in the weather.
> K. Feb. Lyra begins to set;[36] there is an east wind and sometimes a south wind with hail.
> III Non. Feb. All of Lyra and the middle of Leo set....

Here the reader is expected to find the current date and read off the astrometeorological situation.[37] But, in spite of the number of examples we have of literary parapegmata, we shall see that they so frequently disagree as to be all but useless as evidence for reconstructing inscriptional or other fragmentary parapegmata.

Some of the main problems that we encounter in trying to reconstruct astrometeorological parapegmata can best be seen by considering one of our most fragmentary examples, coincidentally the only inscriptional astrometeorological parapegma extant in Latin, the Puteoli Parapegma (Fig. 10).[38] Here we see in the main section a partly preserved phrase that we can reconstruct by comparison with Columella: *Delphin[us] occid[it ves]peri, t[empes]tas,* "Delphinus sets in the evening, (there is) a storm." Columella has *Delphinus incipit occidere,* and *siderum occasus tempestatem facit,* "Delphinus begins to set," and "the stars' setting causes a storm" on two con-

34 Hesiod, *Op.,* 663 f.

35 For this translation of *significat,* see Lehoux, 2004a.

36 It is unclear why this entry gets repeated, but this entry is preserved in all MSS. There may be some technical distinction between *Fidicula* and *Fidis* that is unique to Columella, or else one of the two entries is an interpolation.

37 See Lehoux, 2004b.

38 See Lehoux, 2006.

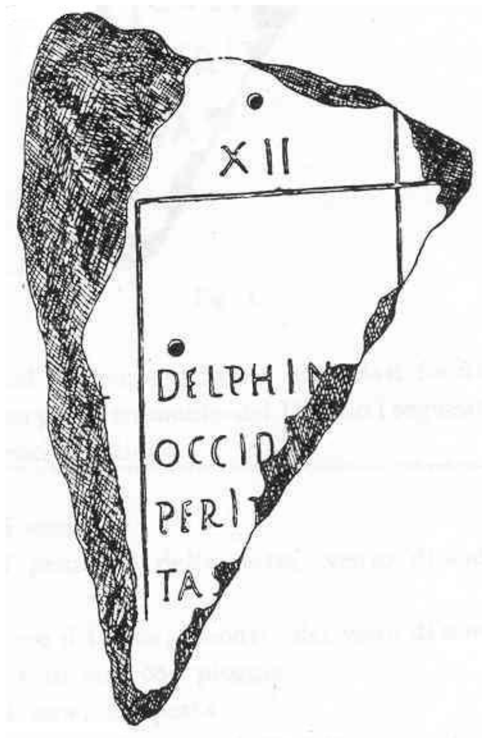


Figure 10. The Puteoli Parapegma.

secutive days. Such discrepancies in day counts, it turns out, are endemic to all but a few small clusters of closely related parapegmata. Given the fragmentary nature of the Puteoli parapegma, it is impossible to tell whether it belongs to such a cluster.

The XII in the top line does not help matters, as there is nothing like it in any other astrometeorological parapegma. Moreover, its hole is considerably smaller than that of the entry for Delphinus below it (2.5 mm versus 4 mm), indicating that the XII is part of a different cycle from Delphinus, probably either calendrical or lunar, tracked with a different-sized peg, as we find in some other Latin inscriptional parapegmata with multiple cycles. We have already seen several parapegmata that track some subset of hebdomadal, nundinal, lunar, and calendric cycles, but this would be the only one to combine an astrometeorological cycle with any other, making it a unique kind of hybrid for which direct comparative evidence is lacking.

If we look to how other parapegmata handle the one astrometeorological entry on the Puteoli fragment, we see just how much variation there generally is in this class of parapegmata, and just how much uncertainty we have in trying to reconstruct any fragmentary parapegma based on such comparative evidence.

All these different parapegmata have different phases listed as significant, and the ordering of entries they do share often differs markedly in ways not explainable simply by differences in latitude (Fig. 11). In only a few cases can we see patterns emerging that show us that some two or three parapegmata are related to each other,[39] and in such cases comparative evidence may be of more use than it is for other examples, but for the most part clear relationships are lacking between astrometeorological parapegmata, just as we have seen with nundinal lists.

39 As with Columella and Polemius Silvius, for example. See Degrassi, 1963, vol. XIII.2, 263.

Puteoli Parapegma	*Columella*	*Ovid*	*Pliny*	*Clodius Tuscus*	*Ptolemy*
	Star in the breast of the Lion sets.				
	S or SW wind, wintry, rainy.				
	Delphinus and Lyra begin to set, change in weather.	Lyra sets, Leo is invisible.		Stormy winds with snow.	Bright star of Lyra sets in the evening. S or NW wind.
Delphinus sets in the evening, Storm.	Storm from evening setting of Delphinus and Lyra.	Delphinus sets in the evening.	Delphinus sets in the evening.	Delphinus tends to set.	
	Lyra begins to set, E wind or S wind with hail.			Part of Lyra begins to set in the first watch of the night, and clouds and a strong N wind with thunder.	
	All of Lyra and the middle of Leo set.	Half of Aquarius is visible.		Rain mixed with snow.	
				S and E wind, and Lyra begins to set.	
	Middle of Aquarius rises, windy.			Stormy air and W wind.	Star on heart of Leo sets in the morning.
				The middle of Leo sets with Lyra.	
			/... a gap of 11 days.../		
			Star in breast of the Lion sets in morning.		
			/... a gap of 8 days.../		
			Lyra sets in the evening.		

Figure 11. Comparison of astrometeorological data.

"Miletus II"

More complete than Puteoli, but not much more reconstructable, is a parapegma from Miletus, known as Miletus II. The main difference here from what we have seen so far is that this parapegma is attributive, that is, the predictions are ascribed to various authorities such as Meton, Euctemon, Eudoxus, and others. There are three fragments preserving parapegmatic data (IMilet inv. 456D, 456A, and 456N, see Figs. 12-14), and additionally a fragment with part of an introductory text and slight remains of the left edge of a column of parapegma (456C).[40] Here we see both weather predictions and astronomical information attributed. Some days, it should be noted, have no astrometeorological information associated with them, and are represented just by place-holding peg holes.

Like the Puteoli Parapegma, Miletus II does not compare well enough with other parapegmata to warrant reconstruction beyond what can be achieved on internal evidence alone. Some attempts have been made to reconstruct the missing stellar phases by a comparison with both astronomical data (for the true sequence of phases at Miletus), but this assumes that the parapegma was observationally derived, which cannot be shown.[41] And comparative evidence in other parapegmata is only useful if a close resemblance between one parapegma and another can be shown, which is not the case here.

But there is a further temptation that raises its head with attributive parapegmata like this one. By comparing this text with other attributive parapegmata, and extracting the predictions ascribed to individual authors, some scholars have tried to reconstruct the lost ur-parapegmata from which the attributions were taken. Thus we get Rehm's and Pritchett and van der Waerden's *Parapegma of Euctemon,* and van der Waerden's *Parapegma of Dionysius.*

On Not Reconstructing Lost Parapegmata

The "Euctemon Parapegma"[42] is a modern reconstruction by Rehm of the presumed fourth century BC calendar from which later parapegmatists are supposed to have excerpted their Euctemon citations. The problem here is that we cannot be sure that the text or texts from which the citations were taken looked anything like the reconstruction (it may have looked more like Hesiod or Aratus), nor—and this is a very important point—that it was written in the same calendar.

Rehm has attributed to Euctemon a list of the date-differences between various stellar phases, specifically, the very one found in *C. Vind. Gr. philos. 108,* fol. 282v-283r.[43] His reasoning relies on the close similarity between some of the timings of phases listed in this text and the dates of phases attributed to Euctemon in Geminus and Ptolemy. But the correspondence between *C. Vind.* and the attributions to Euctemon in Geminus and Ptolemy are not a perfect match and in some instances differ quite markedly. Even a quick glance at Rehm's table where he sets

40 The texts given here are slightly revised from those in Lehoux, 2004b. In particular Rehm's transcription of the currently untraceable 456N has been checked against a photograph in the *Inscriptiones Graecae* archives in Berlin, which is probably the same photograph that Rehm himself used; a scan is available at https://archive.nyu.edu/handle/2451/44434/ . On 456C see now Bevan, Jones, and Lehoux, 2019.

41 See Lehoux, 2004b.

42 Rehm, 1913. See also Pritchett and can der Waerden, 1961; Wenskus, 1990; Hannah, 2002, 2005, 2009; Lehoux, 2007.

43 See Cumont, *CCAG*, VI, 13.

[	• [ ]
[...]Α̣ΤΑ	ΚΑ̣[...]
[...] *vac.*	*vac.* [...]
[...] •	• ὡρίω[ν ...]
[• ... ἀκρώνυ]χ̣ος δύνει	κατὰ̣ [...]
[κατὰ ...] κ̣α̣ὶ Αἰγυπτίους. •	• ὑάδε[ς ...]
[• ...] νότος πνεῖ κατ᾽ Εὔδοξον	κατὰ ̣[... ... καὶ]
[καὶ Αἰγ]υ̣πτίους, κατὰ δὲ Ἰνδῶν Καλ-	λύρα Ε[...]
[λανέ]α σκο̣[ρ]πίος δύνει μετὰ βρον-	κατὰ [...]
[τ]ῆ̣ς καὶ ἀν]έμου	• ὑάδε[ς ...]
[•] •	σφόδ[ρα ...]
[• ...]Ε̣Σ ἀκρώνυχοι ἐπιτ̣έ̣λλουσιν	• χειμ[αίνει ...]
[κατ᾽ Εὔ]δ̣οξον κ̣α̣ὶ̣ Αἰ̣[γυπτί]ους.	• ὑάδ[ες ...]
[... ἑσπ]έ̣ρ̣ιαὶ̣ ἐ̣π̣[ι]τ[έ]λ̣λουσιν	χειμ[αίνει ...]
[...]	• [...]

[	• [ ]
[...]Α	ΚΑ[...]
[...]	[...]
[...]•	• Orio[n ...]
[• ...] sets [acrony]chally	according to [...]
[according to ...] and the Egyptians. •	• The Hyade[s ...]
[• ...] the south wind blows according to	according to [... ... and]
Eudoxus [and the Eg]yptians; and	Lyra E[...]
according to the Indian Cal[laneus,]	according to [...]
Scorpio sets with thunder and wind.	• The Hyade[s ...]
[•] •	very mu[ch ...]
[• ...]ΕΣ rise acronychally	• It is stor[my ...]
[according to Eu]doxus and the E[gypti]ans.	• The Hyade[s ...]
[...] r[is]es in the [eve]ning.	It is stor[my ...]
[...]	• [...]

Figure 12. IMilet inv. 456D.

[...
[...]
[...]
[...] *vac.*
[... ο]υσ̣ιν κατ' ΕΥ-
[... κατὰ Ἰ]νδῶν Καλλανέα
[• ... ἑσ]π̣έριαι δύνουσιν
[... ἐπι]σ̣ημαίνει, χαλάζηι
[κατὰ ...] • • •
[...]Α̣Σ κρύπτεται ἑσπέρας, χάλαζαι
[...]γ̣οντα̣[ι] καὶ ζέφυρος ἐπιπνεῖ
[...] ̣ΜΟ̣[̣] ̣ κατὰ δὲ Ἰνδῶν
[Καλλανέα ...] *vac.*

...]Ε̣Ι̣ ἑσ̣πέρας ΚΑ̣[...]
[κ]α̣τ̣' Εὐ̣κτήμονα. •
• αἴξ ἀκρώνυχος δύνει κα̣[τὰ]
καὶ Φίλιππ̣ον καὶ Αἰγυπτί[ους.]
• αἴξ ἑσπερία δύνει κατὰ Ἰνδῶ̣[ν]
Καλλανέα. •
• αἐτὸς ἐπιτέλλει ἑσπέ̣ρας
κατ' Εὐκτήμονα.
• ἀρκτ̣ο̣ῦρος δύεται ἕωθεν καὶ ἐ[πιση-]
μαίνει κατ' Εὐκτήμονα. τῆ̣ι̣ δ' α[ε-]
τὸς ἐπιτέλλει ἑσπέρας καὶ κα̣[τὰ]
Φίλιππον.

[
[
[
[
[...]s according to Eu-
[... according to the I]ndian Callaneus
[• ...] sets i the [eve]ning
[... there is a ch]ange in the weather
[ ] with hail, [according to ...] • • •
[...]Α̣Σ disappears in the evening. It hails
[...]γ̣οντα[ι] and Zephyrus blows.
[...] ̣ΜΟ̣[̣] ̣ and according to the Indian
[Callaneus ...]

...] evening [...]
[accordi]ng to Euctemon. •
• Capella sets acronychally ac[cording]
to both Philippus and the Egypti[ans.]
• Capella sets in the evening according
to the Ind[ian] Callaneus. •
• Aquila rises in the evening
according to Euctemon.
• Arcturus sets in the morning accord-
ing to Euctemon. On this day [Aqu-
ila rises in the evening also, ac[cor-]
[ding to] Philippus.

Figure 13. IMilet inv. 456A.

Geminus and Ptolemy alongside *C. Vind.*[44] will reveal just how unconvincing the correspondence is between *C. Vind.* and our known sources for Euctemon. Of the thirty-eight day-counts that are listed by *C. Vind.*, eleven match the timings in Geminus' Eudoxus exactly, and four more are within a day or two. Due to textual corruption, nine more entries are impossible to compare. This leaves us with fourteen entries—40% of the text—that are not a good fit between the two texts. In many instances the fit is in fact better between these entries and other parapegmata, and/or material attributed to other authorities in Geminus. Moreover, in Geminus, all the Euctemon entries are wedded to weather predictions, which are lacking in *C. Vind.* All of this leads me to the conclusion that both Geminus and *C. Vind.* were working from multiple sources, not just some single text attributable to a single author. It is therefore not possible to cite the list we find in *C. Vind.* as "the parapegma of Euctemon."

44 Rehm, 1913, 14-26; 30; see also the analysis in Lehoux, 2007, 181-187.

[...]Α̣Ι̣ ἐπιση-
[μαίνει ...]Ι̣ κ̣ατ᾽ Εὐκτήμονα. τῆι δ᾽ ΑΥ̣- •
[... κατὰ Φ]ίλιππον. ἀρκτοῦρος δύε̣-
[ται ...]Ν̣ καὶ ἐπισημαίνει
[...] ἐ̣πιτέλλουσιν ἕωθεν̣ [...]
[...]Ν̣ΕΙ αὐταῖς κατὰ Φίλιππ[ον ...]
[... κ]ατ᾽ Εὔδοξον πλει̣ά̣[δες]
[ἐπιτέ]λ̣λουσιν.
[...]Ι̣Α̣Ι ἐπιτέλ[λουσιν ...]
[κατ᾽ Ἰνδῶ]ν Καλλα̣[νέα ...]

[...]Α̣Ι̣ there is a change
[in the weather ...]Ι̣ according to Euctemon, and at the ΑΥ̣- •
[... according to Ph]ilippus. And Arcturus se-
[ts ...]Ν̣ and there is a change in the weather.
[(pl.) ...] rise i the morni[ng ...]
[...]Ν̣ΕΙ for the same ones, according to Philipp[us ...]
[... acc]ording to Eudoxus. The Pleiades
[ri]se.
[...]Ι̣Α̣Ι ris[e ...]
[According to the Indian] Calla[neus ...]

Figure 14. IMilet inv. 456N.

Conclusion

My general stance has been cautious with respect to reconstructing parapegmata, paying particular attention to the problems posed by comparative evidence in certain classes of these texts. Because of the regularity and simplicity of the lunar and hebdomadal sequences found in some parapegmata, they are easily reconstructed. For nundinal and astrometeorological sequences, however, comparative evidence turns out to be useful for reconstruction in only a few cases, but very few of these texts agree with each other at even a basic level. The degree of variance shown by the different texts is, I think, sufficiently high that we must presume variance to be largely endemic to these cycles. This has the greatest impact on the large body of astrometeorological parapegmata. Reconstructions and editions that try to minimize or correct for their variance are doing these texts an injustice. I would thus caution against the temptation to fix repetitions, mistakes, and omissions in these texts through comparison with other parapegmata, and instead to treat the texts as they have come down to us, flaws and all.[45]

45 I would like to thank Alexander Jones and the anonymous referee for their valuable comments on earlier drafts of this paper.

Abbreviations

CCAG = Cumont, F., et al., ed. *Catalogus codicum astrologorum graecorum*, Brussels, 1898-1953.
CIL = *Corpus inscriptionum latinarum*, Berlin, 1863-.
Georg. = Vergil, *Georgics*.
NH = Pliny, *Naturalis historiae.*
Op. = Hesiod, *Works and Days.*
RE = *Paulys Realencyclopädie der classischen Altertumswissenschaft*, Stuttgart, 1893-.
RR = Columella, *De re rustica*.
Sat. = Petronius Arbiter, *Satyricon*, or Macrobius, *Saturnalia.*

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The equant in India redux

Dennis W. Duke

The *Almagest* equant plus epicycle model of planetary motion is arguably the crowning achievement of ancient Greek astronomy. Our understanding of ancient Greek astronomy in the centuries preceding the *Almagest* is far from complete, but apparently the equant at some point in time replaced an eccentric plus epicycle model because it gives a better account of various observed phenomena.[1] In the centuries following the *Almagest* the equant was tinkered with in technical ways, first by multiple Arabic astronomers, and later by Copernicus, in order to bring it closer to Aristotelian expectations, but it was not significantly improved upon until the discoveries of Kepler in the early 17th century.[2]

This simple linear history is not, however, the whole story of the equant. Since 2005 it has been known that the standard ancient Hindu planetary models, generally thought for many decades to be based on the eccentric plus epicycle model, in fact instead approximate the *Almagest* equant model.[3] This situation presents a dilemma of sorts because there is nothing else in ancient Hindu astronomy that suggests any connection whatsoever with the Greek astronomy that we find in the *Almagest*. In fact, the general feeling, at least among Western scholars, has always been that ancient Hindu astronomy was entirely (or nearly so, since there is also some clearly identifiable Babylonian influence) derived from pre-*Almagest* Greek astronomy, and therefore offers a view into that otherwise inaccessible time period.[4]

The goal in this paper is to analyze more thoroughly the relationship between the *Almagest* equant and the Hindu planetary models. Four models will play a role in our analysis. These are the equant plus epicycle, the eccentric plus epicycle, the concentric equant plus epicycle, and the Hindu model. The first three are geometric in nature. Each one of these has a deferent that carries an epicycle, and each one has some scheme for non-uniform motion on the deferent. Both the epicycle and the non-uniform motion result in so-called anomalies, i.e. departures from uniform, or mean, motion. The Hindu model is not directly geometric but, as we shall see,

1 James Evans, "On the function and probable origin of Ptolemy's equant," *American journal of physics,* 52 (1984), 1080-9; Noel Swerdlow, "The empirical foundations of Ptolemy's planetary theory," *Journal for the history of astronomy*, 35 (2004), 249-71; Alexander Jones, "A Route to the ancient discovery of nonuniform planetary motion," *Journal for the history of astronomy*, 35 (2004); Dennis W. Duke, "Comment on the Origin of the Equant papers by Evans, Swerdlow, and Jones," *Journal for the History of Astronomy* 36, (2005) 1-6.

2 N. M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De revolutionibus* (Springer-Verlag, New York and Berlin, 1984) 41-48.

3 D. W. Duke, "The equant in India: the mathematical basis of Indian planetary models," *Archive for History of Exact Sciences*, 59 (2005) 563-576.

4 O. Neugebauer, "The Transmission of planetary theories in ancient and medieval astronomy," *Scripta mathematica*, 22 (1956) , 165-192; D. Pingree, "The Recovery of Early Greek astronomy from India," *Journal for the history of astronomy*, vii (1976), 109-123; B. L. van der Waerden, "Ausgleichspunkt, 'methode der perser', und indische planetenrechnung," *Archive for history of exact sciences*, 1 (1961), 107-121.

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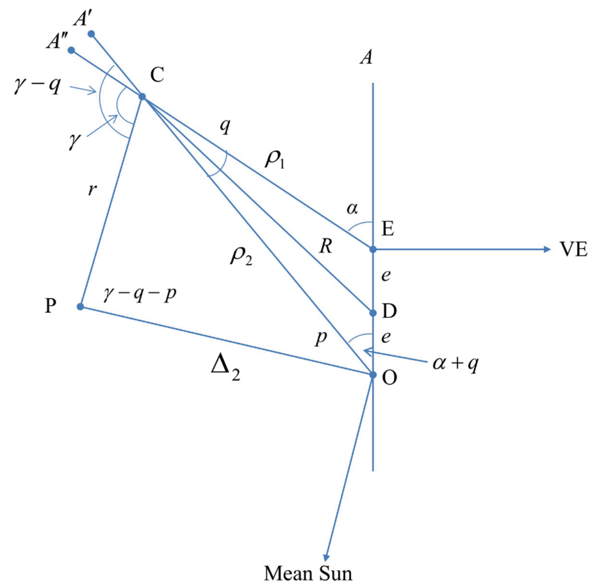

Figure 1. The Almagest equant plus epicycle for an outer planet. The Earth is at O, the equant point is at E, the center of the deferent is at D (the midpoint of OE), the center of the epicycle is on the deferent at C, and the planet is at P. Calculation of the lengths ρ_1, ρ_2, and Δ_2 in terms of e, r, and the angles α and γ, and application of the law of sines to the triangles OEC and OCP, yield the equations q and p, and form the basis of our analysis.

approximates the equant plus epicycle using individual elements found in the concentric equant plus epicycle. For all four models, however, the mathematical relationship between the true longitude λ and the mean longitude $\bar{\lambda}$ is of the form:

(1) $$\lambda = \bar{\lambda} + q + p$$

where the equation of center q is the anomaly associated with the non-uniform motion on the deferent and the equation of the epicycle p is the anomaly associated with the motion on the epicycle. To be more specific, let us consider the outer planets. Given the mean longitude $\bar{\lambda}$ of the planet, the longitude λ_A of the planet's apogee, and the mean longitude $\bar{\lambda}_S$ of the sun, let

(2) $$\alpha = \bar{\lambda} - \lambda_A = \alpha_0 + \omega_P(t - t_0)$$

and

(3) $$\gamma = \bar{\lambda}_S - \bar{\lambda} = \gamma_0 + \omega_a(t - t_0)$$

where ω_P is the mean motion in longitude of the planet and ω_a is the mean motion on the epicycle. If we let ω_S be the mean motion in longitude of the Sun, then the requirement that the radius of the epicycle points in the same direction as the mean Sun implies the relation:

(4) $$\omega_S = \omega_P + \omega_a$$

Then, with the convention $R = 1$ throughout, in terms of the static variables e and r and the dynamic variables α and γ, the calculation of q and p for the equant plus epicycle model is (see Fig. 1):

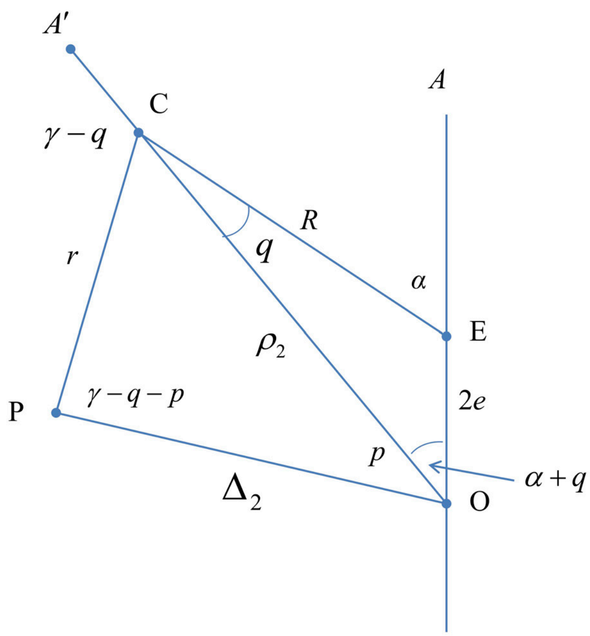

Figure 2. The eccentric plus epicycle model. As in Figure 1 except now the center of the deferent is at E.

(5) $$\rho_1 = \sqrt{1-(e\sin\alpha)^2} - e\cos\alpha$$

(6) $$\rho_2 = \sqrt{(\rho_1 + 2e\cos\alpha)^2 + (2e\sin\alpha)^2}$$

(7) $$\Delta_2 = \sqrt{(\rho_2 + r\cos(\gamma-q))^2 + (r\sin(\gamma-q))^2}$$

(8) $$\sin q(\alpha) = \frac{-2e\sin\alpha}{\rho_2} = \frac{-2e\sin(\alpha+q)}{\rho_1}$$

(9) $$\sin p(\gamma-q) = \frac{r\sin(\gamma-q)}{\Delta_2} = \frac{r\sin(\gamma-q-p)}{\rho_2}$$

Similarly, for the eccentric plus epicycle model we have (see Fig. 2):

(10) $$\rho_2 = \sqrt{(1+2e\cos\alpha)^2 + (2e\sin\alpha)^2}$$

(11) $$\Delta_2 = \sqrt{(\rho_2 + r\cos(\gamma-q))^2 + (r\sin(\gamma-q))^2}$$

(12) $$\sin q(\alpha) = \frac{-2e\sin\alpha}{\rho_2} = -2e\sin(\alpha+q)$$

(13) $$\sin p(\gamma-q) = \frac{r\sin(\gamma-q)}{\Delta_2} = \frac{r\sin(\gamma-q-p)}{\rho_2}$$

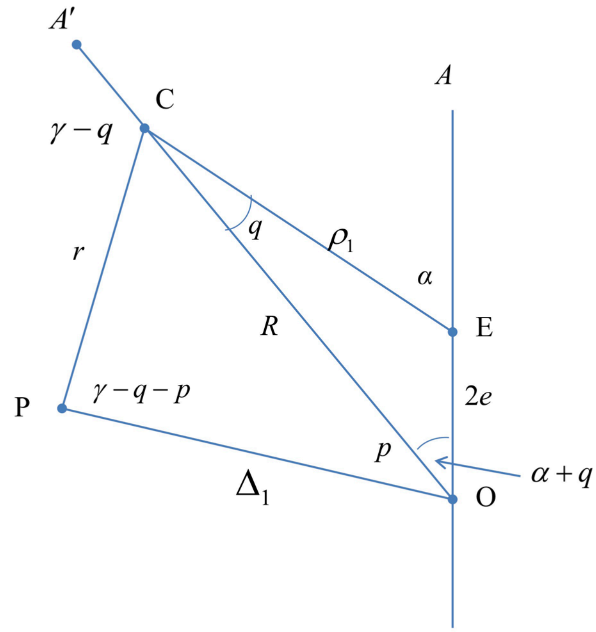

Figure 3. The concentric equant plus epicycle. As in Figure 1 except now the center of the deferent is at O.

Note that the results for these two models are formally quite similar. Both models have the property that while q is a function of just the dynamic variable α, the angle p is, through both its numerator and the denominator, a function of both dynamic variables α and γ. This means that if your goal is to compute q and p using tables and interpolation, a strategy widely used in both Greek and Hindu astronomy, then the table for q can be simple, just a column for α_i values and a column for the corresponding $q(\alpha_i)$ values. The table for the numerator of p is also just a pair of columns. However, the table for the denominator Δ_2 of p would, if built in the most direct way, have as many rows as we have γ values and as many columns as we have α values, and thus be uncomfortably large. For the eccentric plus epicycle model there are no surviving tables that tell us how this problem was solved, if indeed it ever was solved, but the *Almagest* gives a clever interpolation scheme for the equant plus epicycle that reduces the number of columns down to just a few additional auxiliary columns.[5]

Let us now consider the concentric equant plus epicycle model (see Fig. 3). The computation of q and p is:

(14) $$\rho_1 = \sqrt{1-(2e\sin\alpha)^2} - 2e\cos\alpha$$

(15) $$\Delta_1 = \sqrt{(1+r\cos(\gamma-q))^2 + (r\sin(\gamma-q))^2}$$

(16) $$\sin q(\alpha) = -2e\sin\alpha = \frac{-2e\sin(\alpha+q)}{\rho_1}$$

5 G. Van Brummelen, "Lunar and planetary interpolation tables in Ptolemy's *Almagest*," *Journal for the history of astronomy*, 25 (2004) 297-311.

(17) $$\sin p(\gamma - q) = \frac{r\sin(\gamma - q)}{\Delta_1} = r\sin(\gamma - q - p)$$

For this model the tables for the computation of q and p both require just two columns, so no special interpolation scheme is needed.

Finally, let us consider the ancient 4-step Hindu planetary model. For the time being we will consider a specific variant (henceforth 4Ha, the 'a' distinguishing it from other variants, 4Hb and 4Hc, that will appear later) found in the *Aryabhatiya* of Aryabhata, written around 500 CE.[6] As mentioned earlier, the text is not specifically geometrical, but instead refers to two functions, *manda* and *sighra*, the same as the functions q and p of the concentric equant plus epicycle, and in terms of the basic variables $\alpha = \bar{\lambda} - \lambda_A$ and $\gamma = \lambda_S - \bar{\lambda}$ these functions are computed as

(18) $$\sin q(\alpha) = -2e\sin\alpha$$

and

(19) $$\sin p(\gamma) = \frac{r\sin\gamma}{\sqrt{(1 + r\cos\gamma)^2 + (r\sin\gamma)^2}}$$

In terms of these functions the text leads us through the following four steps:

with argument $\alpha = \bar{\lambda} - \lambda_A$ compute $v_1 = \bar{\lambda} + ½q(\alpha)$

with argument $\gamma = \lambda_S - v_1$ compute $v_2 = v_1 + ½p(\gamma)$

with argument $\alpha' = v_2 - \lambda_A$ compute $v_3 = v_2 + q(\alpha')$

with argument $\gamma' - \lambda_S - v_3$ compute $\lambda = v_3 + p(\gamma')$

It is straightforward to verify that the result of the four steps is equivalent to the following sequence of computations:

(20) $$\alpha' = \alpha + \tfrac{1}{2}q(\alpha) + \tfrac{1}{2}p(\gamma - \tfrac{1}{2}q(\alpha))$$

(21) $$\sin q' = -2e\sin(\alpha')$$

(22) $$\sin p' = \frac{r\sin(\gamma - q')}{\sqrt{(1 + r\cos(\gamma - q'))^2 + (r\sin(\gamma - q'))^2}}$$

(23) $$\lambda = \bar{\lambda} + q' + p'$$

Note that at every step of this calculation we are using only the simple single variable functions q and p that characterize the concentric equant plus epicycle model, albeit with arguments of varying complexity, so we can use simple, single variable tables and avoid completely the coupled variable problem that occurs in the equant plus epicycle and eccentric plus epicycle models. What is perhaps less obvious is that the specific series of 4Ha steps leads to values of $q' + p'$ that approximate very well, at least for small to moderate values of e and r, $q + p$ for the equant

6 D. Pingree, "History of Mathematical astronomy in India," *Dictionary of Scientific Biography*, 15 (1978), 533-633.

plus epicycle model, but not $q+p$ for the eccentric plus epicycle model. Our task now is to understand why this happens and how that might have been realized by some ancient mathematician.

Taking into account that the agreement between the equant plus epicycle model and the 4Ha model is best for small to moderate values of e and r suggests expanding $q+p$ for the equant and $q'+p'$ for the 4Ha model in power series in e and r and keeping the first and second order terms. This is something we can do just to see if we are on the right track, but of course this has nothing to do with how some ancient mathematician understood things. The results for the 4Ha model are:

(24) $$q' = -2e\sin\alpha + 2e^2\sin\alpha\cos\alpha - er\cos\alpha\sin\gamma + O(e^3, e^2r, er^2, r^3)$$

(25) $$p' = r\sin\gamma - r^2\sin\gamma\cos\gamma + 2er\sin\alpha\cos\gamma + O(e^3, e^2r, er^2, r^3)$$

and for the equant plus epicycle model:

(26) $$q = -2e\sin\alpha + 2e^2\sin\alpha\cos\alpha + O(e^3)$$

(27) $$p = r\sin\gamma - r^2\sin\gamma\cos\gamma - er\cos\alpha\sin\gamma + 2er\sin\alpha\cos\gamma + O(e^3, e^2r, er^2, r^3)$$

Therefore, $q'+p'$ and $q+p$ agree exactly through second order terms and differ only for third and higher order terms, and these terms are small unless either e or r (or both) get too large. Note that this agreement occurs even though q and q', and likewise p and p', differ already in the second order terms. This explains why we see such good agreement for small to moderate e and r. We now need to investigate how the ancient astronomer understood that such agreement could arise.

To approach this question, consider first a simpler but related problem. Suppose that for some reason we are using a simple concentric equant model, without epicycle, perhaps for the Sun or for the Moon at syzygy (and this is exactly what does happen in the ancient Hindu texts). Then we would likely have tables for the concentric equant equation of center:

(28) $$\sin q_{ce}(\alpha) = -2e\sin(\alpha)$$

so that we could compute, using interpolation, $q_{ce}(\alpha)$ for any value of the argument α, which need not be simply the angle α itself. Now suppose further that for some reason we decide to use instead an eccentric model, for which the equation of center is:

(29) $$\sin q_{ec}(\alpha) = \frac{-2e\sin(\alpha)}{\rho_2} = -2e\sin(\alpha + q_{ec})$$

But rather than compute a new table for q_{ec} we wonder whether we can compute q_{ec} using instead the table we already have for q_{ce}? The answer is yes, and here is how. Using only the first and third terms in the above relation we can compute q_{ec} iteratively as follows:

(30) $$\begin{aligned} q_{ec}^{(0)} &= \arcsin(-2e\sin\alpha) \\ q_{ec}^{(1)} &= \arcsin(-2e\sin(\alpha + q_{ec}^{(0)})) \\ q_{ec}^{(2)} &= \arcsin(-2e\sin(\alpha + q_{ec}^{(1)})) \\ &\text{etc.} \end{aligned}$$

But notice that all the table lookups for the right-hand sides come from the concentric equant table, so there is no need for a new table involving the more complicated middle term with

$$(31) \qquad \rho_2 = \sqrt{(1+2e\cos\alpha)^2 + (2e\sin\alpha)^2}$$

in the denominator. The convergence of the iterations is quite fast, and the computation of q_{ec} from a table of q_{ce} is therefore as exact as you need it to be. In fact, for small the moderate values of *e/R* you can stop after the first iteration step.

The immediate lesson from this exercise is that it is possible to eliminate a denominator in terms of a shifted function argument. The lesson will be applied repeatedly in the following.

Note that this scheme is similar to the first and third steps of the 4Ha model if we ignore the epicycle terms, apart from the fact that here we are shifting α by an amount *q*, while in the 4Ha model we are shifting by an amount ½*q*.

To get a hint about how this factor of ½ arises, let us set aside for the moment the fact that

$$(32) \qquad -2e\sin\alpha = -2e\sin(\alpha + q_{ec})\cdot\rho_2$$

is an exact consequence of the law of sines and instead try to understand it as a consequence of small eccentricity *e/R*. We might notice that

$$(33) \qquad \begin{aligned} \rho_2 &= \sqrt{(1+2e\cos\alpha)^2 + (2e\sin\alpha)^2} \\ &\simeq 1 + 2e\cos\alpha + O(e^2) \end{aligned}$$

So that

$$(34) \qquad \begin{aligned} \frac{-2e\sin\alpha}{\rho_2} &\simeq \frac{-2e\sin\alpha}{1+2e\cos\alpha} \\ &\simeq -2e\sin\alpha(1-2e\cos\alpha) \\ &\simeq -2e\sin\alpha\cos q - 2e\cos\alpha\sin q \\ &= -2e\sin(\alpha+q) \end{aligned}$$

where the second line follows from the approximation $1/(1 + x) \simeq 1 - x + O(x^2)$, the third line follows from the approximation $\cos q \simeq 1 + O(q^2)$, and the final line is the sine addition theorem. All of these are elementary results that can reasonably be presumed to be understood at any time following, say, Archimedes.[7]

Coming now to the factor of ½*q* that appears in the 4Ha model, let us consider the law of sines result for the equant:

$$(35) \qquad \sin q(\alpha) = \frac{-2e\sin\alpha}{\rho_2} = \frac{-2e\sin(\alpha+q)}{\rho_1}$$

Using the first equality and the fact that $\rho_2 \simeq 1 + e\cos\alpha + O(e^2)$, we have:

$$(36) \qquad \begin{aligned} \frac{-2e\sin\alpha}{\rho_2} &= \frac{-2e\sin\alpha}{1+e\cos\alpha} \\ &\simeq -2e\sin\alpha - 2e\cos\alpha\cdot(-e\sin\alpha) \\ &= -2e\sin\alpha\cos\tfrac{1}{2}q - 2e\cos\alpha\sin\tfrac{1}{2}q \\ &= -2e\sin(\alpha+\tfrac{1}{2}q) \end{aligned}$$

7 It is perhaps worth mentioning that Ptolemy uses the law of sines in *Almagest* IX 10 without even mentioning it. See *Ptolemy's Almagest*, transl. by G. J. Toomer (London, 1984), 7 n. 10 and 462 n. 96.

Or alternatively, using the second equality and

$$(37)\qquad \rho_1 = \sqrt{1-(e\sin\alpha)^2} - e\cos\alpha \simeq 1 - e\cos\alpha + O(e^2)$$

we have:

$$
\begin{aligned}
(38)\qquad \frac{-2e\sin(\alpha+q)}{\rho_1} &= \frac{-2e\sin(\alpha+q)}{1-e\cos\alpha} \\
&\simeq -2e\sin\alpha - 2e\cos\alpha\cdot(-2e\sin\alpha) - 2e\cos\alpha\sin\alpha \\
&= -2e\sin\alpha + 2e\cos\alpha\cdot e\sin\alpha \\
&= -2e\sin\left(\alpha + \tfrac{1}{2}q\right)
\end{aligned}
$$

Coming now to the full equant plus epicycle model and its approximation with the 4Ha model, our only remaining task is to explain the origin of the additional term ½p in the shifted argument $\alpha' = \alpha + ½q + ½p$. Let us begin with the expression for $q + p$ in the equant plus epicycle model, and proceed through a series of steps closely related to those we just finished above to show that once again $q + p = q' + p'$. In the process we will need one new result related to the epicycle, namely the approximate factorization

$$
\begin{aligned}
(39)\qquad \Delta_2 &= \sqrt{(\rho_2 + r\cos(\gamma-q))^2 + (r\sin(\gamma-q))^2} \\
&\simeq 1 + e\cos\alpha + r\cos\gamma \\
&\simeq (1+e\cos\alpha)(1+r\cos\gamma) \\
&\simeq \rho_2\Delta_1
\end{aligned}
$$

Proceeding along the same lines as above, we find

$$
\begin{aligned}
(40)\qquad q+p &\simeq \sin q + \sin p \\
&= \frac{-2e\sin\alpha}{\rho_2} + \frac{r\sin(\gamma-q)}{\Delta_2} \\
&= -2e\sin(\alpha+\tfrac{1}{2}q) + \frac{r\sin(\gamma-q)}{(1+e\cos\alpha)\Delta_1} \\
&= -2e\sin(\alpha+\tfrac{1}{2}q)\cos\tfrac{1}{2}p + \frac{r\sin(\gamma-q)}{\Delta_1} - er\sin\gamma\cos\alpha \\
&= -2e\sin(\alpha+\tfrac{1}{2}q)\cos\tfrac{1}{2}p + \frac{r\sin(\gamma-q)}{\Delta_1} - 2e\cos(\alpha+\tfrac{1}{2}q)\sin\tfrac{1}{2}p \\
&\simeq -2e\sin(\alpha+\tfrac{1}{2}q+\tfrac{1}{2}p) + \frac{r\sin(\gamma-q)}{\Delta_1} \\
&= q' + p'
\end{aligned}
$$

Some Hindu texts mention that the agreement between $q + p$ and $q' + p'$ can be improved by iteration to convergence, just as explained above for q_{ec}.

The 4Ha model is not the only model found in the ancient Hindu texts. A similar variant that we might call 4Hb is equivalent to the following sequence of computations:

(41) $$\alpha' = \alpha + \tfrac{1}{2}q(\alpha + \tfrac{1}{2}\mathrm{p}(\gamma)) + \tfrac{1}{2}p(\gamma)$$

(42) $$\sin q' = -2e\sin(\alpha')$$

(43) $$\sin p' = \frac{r\sin(\gamma - q')}{\sqrt{(1 + r\cos(\gamma - q'))^2 + (r\sin(\gamma - q'))^2}}$$

(44) $$\lambda = \bar{\lambda} + q' + p'$$

Once again it is straightforward to verify that just as we found for the 4Ha model, $q' + p'$ for the 4Hb model is the same as $q + p$ for the equant model up to corrections of $O(e^3, r^3, e^2r, er^2)$.

A third variant, that we will call 4Hc, is according to the texts applicable only to the inner planets Mercury and Venus, and is equivalent to the following:

(45) $$\alpha' = \alpha + \tfrac{1}{2}p(\gamma)$$

(46) $$\sin q' = -2e\sin(\alpha')$$

(47) $$\sin p' = \frac{r\sin(\gamma - q')}{\sqrt{(1 + r\cos(\gamma - q'))^2 + (r\sin(\gamma - q'))^2}}$$

(48) $$\lambda = \bar{\lambda} + q' + p'$$

From the analysis above we know that the omission of the argument shift ½q in α' is equivalent to the omission of the denominator $\rho_2 \simeq 1 + e\cos\alpha$ in the expression

(49) $$\sin q = \frac{-2e\sin\alpha}{\rho_2}$$

that follows from the law of sines for the equant model. For Venus, and to a lesser extent Mercury, this omission might be justified by the fact that e/R is fairly small and so ρ_2 never varies from unity by more than a few percent. On the other hand, the inclusion of the shift ½p maintains the full expression for the equation of the epicycle. Even so, the epicycle of Venus is so large ($r/R \simeq 0.7$) that terms of order r^3 that are neglected in $q' + p'$ are not entirely negligible (or, to put it in terms that our ancient mathematician would use, the factorization $\Delta_2 \simeq \rho_2\Delta_1$ is no longer a good approximation). All in all, it seems that the ancient analyst was scrambling to some degree in the effort to account for Mercury and Venus in his model.

Note that the above development of an approximation to the equant in terms of two simple single variable functions uses only two essential results. The first involves elimination of the denominator ρ_2 in favor of a shifted function argument α + ½q, and the second involves the factorization $\Delta_2 = \rho_2\Delta_1$ and the elimination of the denominator ρ_2 in favor of an additional shift of ½p in the argument. Both of these maneuvers are approximations, but it is easy to check, for the values of interest for e/R and r/R, that they are usually good approximations. While the motivation for these developments is reasonably clear—a desire to evaluate the equant plus epicycle using only tables based on the concentric equant plus epicycle—we can never know with certainty what was the inspiration for this particular solution to the problem. However, if the ancient analyst ever considered the eccentric plus epicycle model, he would see immediately from the law of sines for the equation of center that the denominator ρ_2 can be eliminated with an argument shift, and in this case that shift is exact:

(50) $$\sin q(\alpha) = \frac{-2e\sin\alpha}{\rho_2} = -2e\sin(\alpha+q)$$

And this might inspire him to use the factorization $\Delta_2 = \rho_2 \Delta_1$ in the law of sines for the epicycle and follow a similar path to trading the elimination of ρ_2 for an additional argument shift as follows:

(51) $$\begin{aligned} q+p &\simeq -2e\sin(\alpha+q) + \frac{r\sin(\gamma-q)}{\rho_2\Delta_1} \\ &\simeq -2e\sin(\alpha+q) + \frac{r\sin(\gamma-q)}{(1+2e\cos\alpha)\Delta_1} \\ &\simeq -2\sin(\alpha+q) + \frac{r\sin(\gamma-q)}{\Delta_1} - 2e\cos\alpha\cdot r\sin\gamma \\ &\simeq -2\sin(\alpha+q)\cos p + \frac{r\sin(\gamma-q)}{\Delta_1} - 2e\cos\alpha\sin p \\ &= -2\sin(\alpha+q+p) + \frac{r\sin(\gamma-q)}{\Delta_1} \\ &= q' + p' \end{aligned}$$

Alternatively, he might have just stared at the two law of sines results

(52) $$\sin q(\alpha) = \frac{-2e\sin\alpha}{\rho_2} = \frac{-2e\sin(\alpha+q)}{\rho_1}$$

(53) $$\sin p(\gamma-q) = \frac{r\sin(\gamma-q)}{\Delta_2} = \frac{r\sin(\gamma-q-p)}{\rho_2}$$

where the shifted arguments and eliminated denominators are more or less staring us in the face, and somehow found the inspiration to guess the answer, or made a few guesses and checks and finally stumbled upon the right answer.[8] Either way, whether it was analysis or serendipity or some combination, the ancient texts make it fairly clear that a solution was found.

8 This stare and wait for inspiration strategy is more common that you might think. For a recent example, see http://www.preposterousuniverse.com/blog/2013/10/03/guest-post-lance-dixon-on-calculating-amplitudes/.

A curiosity: Did Ptolemy see Uranus?

N. M. Swerdlow

The short answer to this question is, probably not, or it cannot be answered. The long answer is more interesting, indeed, a curiosity. There were a number of sightings with a telescope of Uranus and Neptune, assumed to be faint stars, before their discovery and identification as planets in 1781 and 1846. The earliest and most remarkable were Galileo's observations, in his measurements of elongations of Jupiter's satellites on 28 December 1612 and 28 January 1613, of a star that turned out to be Neptune. In the second observation he noted that, compared to an unrecorded observation the preceding night in relation to another star, "they (the stars) appeared more distant from each other" (*videbantur remotiores inter se*), so he actually saw Neptune move (about 0;1° retrograde).[1] Neptune is always about magnitude 8, so can be seen only with a telescope, but Uranus varies from about 5.4 to 6, so in principle can be seen with the unaided eye under favorable conditions of a clear, dark sky. I have been able to learn little of unaided sightings of Uranus since its discovery, although if its location is known and conditions are favorable, it should be possible to see along with other stars of magnitude 5.5 to 6. John Tebbutt of the Windsor Observatory of New South Wales reported that several weeks after its opposition of 15 February 1878, Uranus can be "distinctly seen without a telescope," and with a telescope its brightness is comparable to ν Leonis, about 0;45° below the planet, of magnitude 5½. Then, on 18 March 1880, twenty-one days after opposition, "by means of the naked eye and also a small telescope," he compared its brightness with BAC 3621 of magnitude 5½ and BAC 3622 of magnitude 6, finding Uranus about equal to 3621 and superior to 3622.[2]

Following William Herschel's discovery of Uranus, there were examinations of earlier star catalogues to find possible observations with coordinates that could be used to determine and refine the elements of its orbit. The first to do this, even before it had been definitely confirmed as a planet, was Johann Bode, and one of the stars he investigated was from pre-telescopic observation. This was the 27th star in Capricorn in Tycho's catalogue, for epoch 1 January 1601, described as "preceding (east of) this (26, the southern star in the upper part of the tail) to the north," of longitude Aquarius 20;16°, latitude –0;10°, magnitude 6, which Hevelius had found did not exist. Using an identification of a star observed by Tobias Mayer in 1756 as the planet, which turned out to be correct, he provisionally assumed the same for the star in Tycho's catalogue and, using an observation from 1589, calculated back 166 years 10 months, finding a period for the planet of 80 years 8 months. But when he received Laplace's elements in 1784, with a period of over 83 years, Bode realized there was a difference of 24° of longitude and withdrew his identification.[3] Recently, K. P. Hertzog has proposed that the 17th star in Virgo in Ptolemy's

1 This discovery was made by Stillman Drake and Charles Kowal, and published in Galileo's Sighting of Neptune, *Scientific American* 243 no. 6 (1980), 74-81. It is reprinted in Stillman Drake, *Essays on Galileo and the History and Philosophy of Science* 1, Toronto, 1999, 430-41.

2 *Journal and Proceedings of the Royal Society of New South Wales* 12 (1878), 220-21; *idem* 14 (1880), 23, in which he also notes, "Some idea may be formed of its conspicuous character when it is stated that I determined pretty accurately its distance from Regulus and γ Leonis by means of an ordinary sextant."

3 A. F. O'D. Alexander, *The Planet Uranus*, *A History of Observation*, *Theory and Discovery*, New York, 1965, 81-82, with a

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 143–146. Chapter DOI: 10.5281/zenodo.3975731.

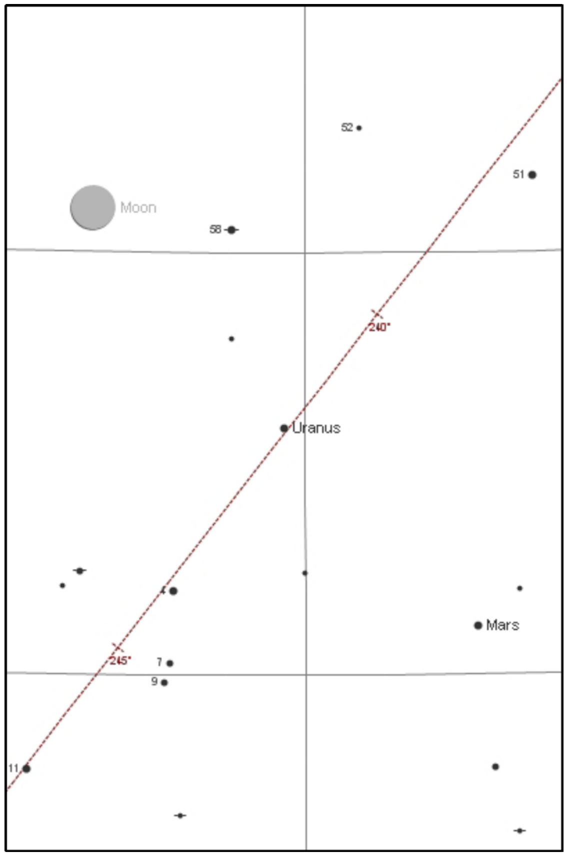

Figure 1. The Moon, Uranus, and Mars, 139 May 30, 8:35 PM in Alexandria.

star catalogue, longitude Virgo 27¼°, latitude +0⅙°, magnitude 6, doubtfully identified as 76(h) Virgo, then at longitude 179;21°, latitude –0;18°, differing by +2;6° and –0;28°, is actually Uranus, observed, not by Ptolemy, but by Hipparchus, possibly on or near 25 March –127, when Uranus was at longitude 175;12°, latitude +0;45°, an elongation from the sun of 173°, close to opposition and thus brightest.[4]

list of twenty-two pre-discovery telescopic observations, 90. E. G. Forbes, The Pre-Discovery Observations of Uranus, *Uranus and the Outer Planets*, ed. G. Hunt, Cambridge, 1982, 67-80. The story of the "missing" 27 Cap in Tycho's catalogue, identified as *μ* Cap by Beyer and ever since, is complicated and related to problems in 26 Cap. In addition to the catalogue, *Tychonis Brahe Dani Opera Omnia*, ed. J. L. E. Dreyer, Copenhagen, 1913-29, 2.264.24, there are, in a lengthy series of observations of zodiacal stars in 1589, 11.363, on 20 November distances of 27 and 28 from 2 Pisces (southern of the two stars in the back of the head), and in a catalogue of observations in 1589 of zodiacal stars, in equatorial coordinates and converted to ecliptic coordinates, 11.405, coordinates of 27, of which Dreyer notes, "This location is false since there is no star in this place." There is an analysis of the problems of 26 and 27 by D. Rawlins, Tycho's 1004-Star Catalog, *Dio* 3, 1993, 32-33.

4 K. P. Hertzog, Ancient Uranus?, *Quarterly Journal of the Royal Astronomical Society* 29 (1988), 277-79. Hertzog prefers Baily's consecutive numbering for the entire catalogue, used by Peters and Knobel, B513, for 17 Virgo, and for the longitude Virgo 27°. It is possible that the text is erroneous, by –1° (ζ for η) in longitude, north for south (βο for νο) in latitude. At the end of *Almagest* 7.4 Ptolemy refers to (some number of) descriptions of constellations and positions of stars, meaning coordinates close enough to his own to identify the stars, by his predecessors. The assumption that he adapted exclusively Hipparchus's star catalogue requires a difference from his coordinates of –2;40° in longitude and the same latitude, or something close, and here for Uranus the differences from the text are –2;3° in longitude, or –1;48° for Virgo 27°, and +0;35° in latitude, although the text could be erroneous. Still, errors of over

But there is an observation of another kind, an observation actually dated and reported, for which the question can be asked of whether Uranus was seen. In *Almagest* 10.8 Ptolemy reports an observation of Mars, three days after opposition to the mean sun, to determine the radius of its epicycle. The observation, of Mars and the moon, was in the second year of Antoninus, Epiphi 15/16 at an apparent time of three equal hours before midnight in Alexandria (139 May 30/31, 9 PM). Using the astrolabe (armillary with graduated rings) for direct measurement of longitude, set on Spica with the 20th degree of Libra culminating, he found Mars at Sagittarius 1⅗° and the same distance, 1⅗°, to the east of the moon, meaning difference in longitude. The computed longitude of the moon, corrected for parallax, was Sagittarius 0°, so the two measurements confirmed the same longitude of Mars, Sagittarius 1;36°. Toomer has found that in computing the position of the moon, Ptolemy applied an equation of time of −0;23^{h}, which correctly from his tables should be about −0;25½^{h}.[5] The configuration of the observation for a mean time of 8:35 PM in Alexandria is shown in Figure 1, in horizon coordinates, 7° of azimuth, 10° of altitude, with nearby stars of Sagittarius and Ophiuchus of fourth to sixth magnitude. The bodies are located in the south-east 20½° to 25½° above the horizon, and the diagonal line is the ecliptic.

It can be seen that Uranus, almost exactly in the ecliptic, is about midway between Mars and the moon, and that is what is interesting about the observation. The computed coordinates of the three bodies and the sun, azimuth measured from south to west and the horizon coordinates corrected for refraction, along with Ptolemy's longitudes, the moon corrected for parallax in both, and the computed magnitudes, are as follows:[6]

	Azimuth	*Altitude*	*Longitude*	*Latitude*	*Ptolemy's Longitude*	*Magnitude*
Moon	312;15°	+25;31°	240;22°	+3;55°	240; 0°	−12.5
Uranus	314;44°	+22;45°	241;45°	+0; 2°	—	5.5
Mars	317;10°	+20;34°	242;12°	−3;12°	241;36°	−2.5
Sun	131;34°	−18;53°	66;41°	—	65;27°	—

The angular separation of Mars and the moon is about 7;21°, of Mars and Uranus 3;16°, and of Uranus and the moon 4;8°. Since the altitude of the sun is about −19°, astronomical twilight has just occurred, which is favorable for seeing faint stars. Uranus, 175° from the sun, near opposition, is at its brightest, but the moon, 174° from the sun, is nearly full, which is unfavorable.

Now, could Ptolemy have seen Uranus as a faint star between Mars and the moon in making this observation? Again, as in the short answer, probably not, in fact, almost certainly not as seeing a star of magnitude 5.5 about 4° from the full moon seems very unlikely if not impossible. But the matter is not hopeless. The observation followed by three days the third of Ptolemy's oppositions of Mars, Epiphi 12/13, two equal hours before midnight (139 May 27/28, 10 PM, it appears mean time), used to find its eccentricity and direction of its apsidal line. In order to find

½° in the observations are entirely possible, so any decision rests upon first determining whether the coordinates of this star were adapted from Hipparchus. I do not know how to do this.

5 G. J. Toomer, *Ptolemy's Almagest*, New York, 1984, 499, n. 57. There is much of interest about this observation, but our concern here is the time, for which we use a mean time of 8:35 PM in Alexandria.

6 The coordinates and magnitudes, here and throughout this paper, are computed and the figures are drawn using Alcyone Ephemeris 3 with Δ*T*, the secular acceleration, from JPL Horizons Ephemeris; with other values of Δ*T* the longitude of the moon is about 0;6° less. Ptolemy gives the mean longitude of the sun, Gemini 5;27°, virtually at apogee; the true longitude differs by less than +0;1°.

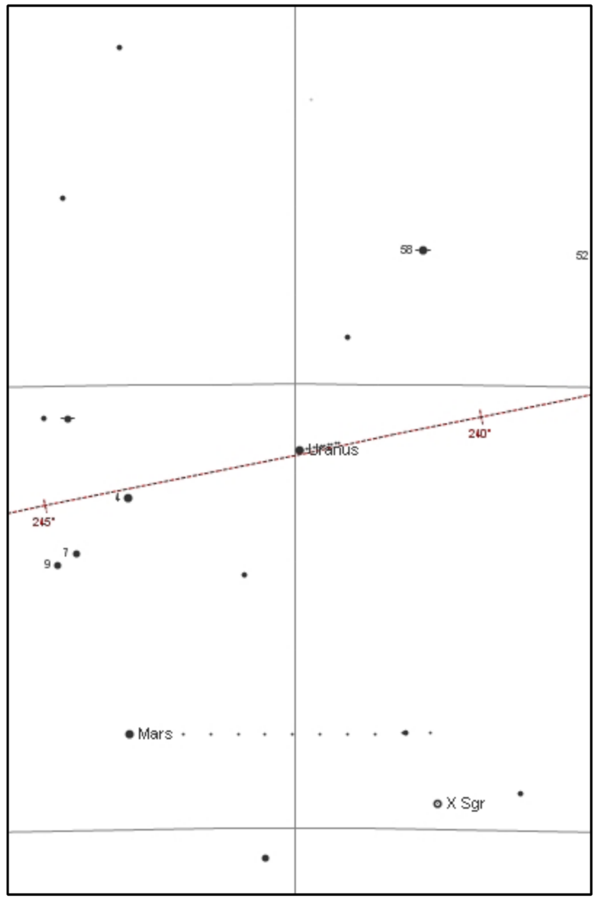

Figure 2. Mars and Uranus, 139 May 22 to June 2, 10 PM in Alexandria.

the time of opposition to the mean sun by interpolation, Ptolemy measured the longitude of Mars using the astrolabe on successive nights before and after the anticipated time, including the observation on May 30 three days after opposition. On earlier nights, Mars and Uranus were less than 4° apart, with the moon 40° to the west at opposition and 75° to the west three nights before opposition, setting at about 1:30 AM, so less likely to overwhelm the light of a faint star at those distances. Figure 2, in equatorial coordinates, 7° of right ascension, 10° of declination, shows the paths of Mars and Uranus, both retrograde from left to right, from May 22 to June 2 for 10 PM in Alexandria, during which time the distance of Uranus from Mars is between 3;43° and 3;16°.

So if in measuring the longitude of Mars from night to night with the astrolabe, or simply in looking in the direction of Mars if the ecliptic ring blocked seeing close to the ecliptic, Ptolemy noticed a faint star about in the ecliptic above Mars, he would have seen Uranus. (I realize that this will not be welcomed with delight by Ptolemy's critics.) But perhaps this is too optimistic, for even if the star could be *seen*, it may not have been *noticed*, and concerning this I have no opinion. Still, the configuration, very nearly in a line, of the moon, Uranus, and Mars at the very time of Ptolemy's measurement of the distance in longitude of Mars from the moon, and Mars and Uranus remaining close together during the several nights Ptolemy measured the position of Mars to find its opposition, are remarkable, the earliest, and only, (possible) sighting of Uranus for which there is evidence that someone was actually looking, and looking in the right place, and for that reason is worthy of notice. And it certainly is a curiosity.

Limits of observation and pseudoempirical arguments in Ptolemy's *Harmonics* and *Almagest*

Alexander Jones

Introduction

Ptolemy's surviving writings can be divided into two categories: treatises covering a branch of scientific knowledge in a systematic and comprehensive manner, and works that address narrower problems and questions. The general treatises are the *Almagest* (astronomy), the *Tetrabiblos* (astrology), the *Harmonics* (musical pitch relations and systems), and the *Optics* (visual perception). Of these four works, the *Almagest* and *Harmonics*, and also to a large extent the *Optics*, are concerned with fields that Ptolemy regards as mathematics, that is, the study of "shape, number, size, and moreover place, time, and the like" (*Almagest* 1.1), whereas the *Tetrabiblos* concerns physics, the study of qualitative properties of matter and change.[1] This ontologically-based distinction with respect to the brances of theoretical philosophy is paralleled by a distinction in the epistemological approach of the treatises: while the *Tetrabiblos* operates primarily with an interplay between the received tradition of astrological doctrine and aprioristic reasoning leading to inexact knowledge, the "mathematical" works aim at exact knowledge by an interplay between sense perception and analytical reasoning.

In the opening chapter of the *Harmonics* Ptolemy tells us that reason and the sensory faculty of hearing are the two "criteria" (i.e. faculties for determining truth) in harmonic science, and outlines how the interplay between them is supposed to work. If we substitute vision for hearing as the sensory criterion appropriate for astronomy and optics, the demonstrative structures of all three "mathematical" treatises largely follow the strategy of *Harmonics* 1.1. The process begins with sense perception, and specifically with certain rather crude but indisputable perceptions that provide reason with the starting points for developing a theory and designing more sophisticated observational procedures and instruments for refining that theory. The back-and-forth between progressively narrower observations and progressively deeper analysis may be repeated until a theory or model is reached whose agreement with observations is within the limits of accuracy of the relevant senses. As Ptolemy asserts in an astronomical context (*Almagest* 9.2), the standard of theory verification is that one should be able to "fit [ἐφαρμόσαι] pretty well all the phenomena" to the model, that is, not only observed data that went into the model's deduction but also any other observed data that one may possess.

Hence in principle every theoretical outcome in Ptolemy's mathematical sciences ought to rest on empirical evidence, and to a great extent the "narrative" of his treatises—the linear flow of evidence and argument that one encounters by reading the works from start to finish—conforms to this expectation. As Swerdlow has pointed out, in the *Almagest* Ptolemy provides his

1 In accordance with the Aristotelian theory of "proper" and "common" sensibles, the *Optics* also deals with the perception of color, the qualitative "physical" property of bodies that is the proper sensible by means of which vision perceives the common sensibles—which more or less coincide with the properties that Ptolemy treats as mathematical.

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empirical evidence in two distinct manners.[2] Considerations that lead to conclusions about the general, as yet unquantified structures of his models tend to be presented as bald assertions of a general phenomenon relating to the apparent behaviour of the relevant heavenly bodies, without explicit reference to specific observations or observational procedures; these correspond to the crude sensory perceptions of the *Harmonics*'s epistemological strategy. But finer details and in particular quantifications of the models are justified on the basis of observations that are described in greater detail, often with specific dates and, where pertinent, indications of the instruments that were employed; these correspond in the *Harmonics*'s account to the more refined sensory perceptions that have been guided by reason. Both types of presentation of empirical evidence are present in the *Harmonics* and *Optics* too, though where in the *Almagest* the second type consists of reports of actual observations (whether expressly dated or not) that are supposed to have been made in the past by Ptolemy or his predecessors, in the *Harmonics* and *Optics* they are descriptions of demonstrations that the reader is invited to recreate, though presumably Ptolemy means us to suppose that he has also tried them out. One can also add a third type of empirical appeal: implied invitations to the reader to compare predictions derived from the quantitative models with the established phenomena. In the *Almagest*, parts of the eclipse theory in Book 6 and the sections concerning planetary stations and visibility conditions in Books 12-13 have this function of implicit model verification—implicit because Ptolemy usually does not say outright that his predictions agree with the phenomena.[3]

Data derived from the senses play a predominant role in controlling the evolution of Ptolemy's mathematical models, but do not suffice to determine them. An obvious, if usually unspoken, consideration in every decision Ptolemy makes about his models is simplicity. In discussions of astronomy in particular it was a commonplace that a multiplicity of models could be devised that were equally in agreement with the phenomena.[4] Ptolemy's response is that "generally we consider that it is appropriate to demonstrate the phenomena through simpler models, so far as this is possible, insofar as nothing significant [ἀξιόλογον] in opposition to such a proposal is apparent from the observations" (*Almagest* 3.1).

The qualification "significant" shows that in principle Ptolemy conceded that a simpler model should sometimes be preferred even if it did not fit the empirical data as closely as a more complicated model, especially if the discrepancies could be plausibly explained as arising from observational or computational errors. Yet he was generally reluctant to allow a simplicity argument to override observation, as we can see in a well known passage in *Almagest* 13.2, where he defends the staggering complexity of his models for the latitudinal motion of the planets on the grounds that the standards of simplicity for heavenly bodies cannot be learned from comparisons with the mundane constructions to which we have more direct access. His explicit invocations of simplicity in choosing between model options are rare, and typically in situations such as the question whether to explain an anomaly by means of an epicycle or an eccenter, where two model structures are kinematically identical, not just indistinguishable to the senses.

2 Swerdlow 2004, 249-250.

3 The confrontation of prediction and phenomena is explicit, however, in *Almagest* 13.8, where Ptolemy takes up the "strange" (ξενίζοντα) visibility phenomena of Venus (extreme variation of intervals of invisibility at inferior conjunction) and Mercury ("missed" appearances in the alternation of morning and evening visibility). See also 3.1 where Ptolemy adduces the accuracy of predictions of eclipse times as evidence that his solar model is correct in assuming a constant tropical year.

4 E.g. Theon of Smyrna, ed. Hiller 166-189 and the passage quoted from Geminus' digest of Posidonius's *Meteorology* quoted by Simplicius (*Commentary on Aristotle's Physics* ed. Diels, *Commentaria in Aristotelem* Graeca 9.291-292).

It is worth asking whether for Ptolemy simplicity is a "criterion" in the technical sense of that term in Hellenistic philosophy, that is, a standard guiding us towards knowledge of the reality underlying the phenomena, or whether it is merely a basis for selecting from among several hypotheses, any of which could equally be true, the one that is easiest to comprehend or most convenient. He does regard computational simplicity as a merit justifying the omission or approximation of certain theoretically necessary elements in methods of predicting phenomena, when the effects of these shortcuts are below the threshold of observational precision (*Almagest* 6.7). In the introductory section of the *Planetary Hypotheses* (1.2) he writes, rather obscurely, that in describing the models for the motions of the heavenly bodies he will use "the simpler ones among the approaches [ταῖς ἁπλουστέραις τῶν ἀγωγῶν] for the sake of convenience in the construction of mechanical models [πρὸς τὸ εὐμεθόδευτον τῆς ὀργανοποιίας], even if some discrepancy [παραλλαγή] ensues," probably referring to the models for planetary latitude which are much simpler than those of the *Almagest*.[5] However, later in the same work (2.6) he writes that one should not suppose that anything pointless and useless exists in nature, while the passage in *Almagest* 13.2 already referred to, while asserting that simplicity is not a trivial thing for human beings to appraise in heavenly bodies, nevertheless implies that it *is* an attribute of their motions. It is thus clear that simplicity is one manifestation of Ptolemy's Platonizing belief (*Harmonics* 1.2) that "the works of nature are crafted [δημιουργούμενα] with a certain reason and ordered cause, and nothing is brought about without plan or at random."

Another important class of argument that rests on the presumed orderliness of the cosmos is appeal to analogy; as Ptolemy succinctly expresses the principle in *Planetary Hypotheses* 1.2, "the most wondrous nature portions out very like things [τὰ παραπλήσια] to similar things," that is, entities that resemble each other in certain essential respects are naturally endowed with other similar characteristics. One might describe this principle as a simplicity argument applied to the cosmos as a whole. Explicit analogical argument is rare in the *Almagest*, but conspicuous in the *Harmonics* (as well as in the more speculative cosmology of the *Planetary Hypotheses*). One form it can take is the extension of attributes that have been deduced for one entity to other entities that are regarded as of the same kind; for example, the empirically deduced spherical shape of the Sun and Moon can be presumed to apply also to the invisible etherial bodies composing the heavens (*Almagest* 1.3). Another form is to infer from a numerical correspondence between two sets of entities that they can be paired off one-to-one in a meaningful way; this kind of argument comes into its own extravagantly in *Harmonics* Book 3 where the theoretical structures based on ratios of whole numbers that Ptolemy has deduced for musical pitch systems earlier in the treatise are assigned putative analogues in various aspects of the human soul and the heavens. Analogical argument can also be applied inversely to infer a kinship between entities on the basis of their having similar attributes, as when Ptolemy adduces the structural similarities among the models for certain subsets among the heavenly bodies as evidence that the models are spatially contiguous (*Almagest* 9.1 and *Planetary Hypotheses* 1B.3[6]). Like simplicity arguments, arguments

5 Aside from the planetary latitude models, the only structural difference between the *Almagest* models and those of the *Planetary Hypotheses* is Ptolemy's abandonment of the special definition of the apogee of the Moon's epicycle from *Almagest* 5.5. If Ptolemy has the latitude models in mind in *Planetary Hypotheses* 1.2, his remarks would appear to cast doubt on whether he believed that the simpler models of the *Planetary Hypotheses* were true representations of the way that the planets move, though they are in fact more accurate as geocentric transformations of the inclined heliocentric orbits of the planets than Ptolemy's earlier models; see Swerdlow 2005.

6 Goldstein 1967, 7. The two arguments are distinct: in *Almagest* 9.1 Ptolemy uses the criterion of restricted/unrestricted possible elongation from the Sun to divide the planets into two groups spatially separated by the Sun's sphere, while in *Planetary Hypotheses* 1B.3 he invokes the structural similarity between the Moon's and Mercury's

based on analogy seem to have the status of weak or probabilistic criteria in Ptolemy's epistemology. They are invoked only when empirical arguments are not available, and often with a qualification that the argument has the force of likelihood, not certainty.

Ptolemy seldom appeals to the authority of his predecessors, if we except the observation reports from the past that he necessarily accepts as givens in the *Almagest*; and when it happens, this kind of appeal is normally offered as a supplement to an empirical argument. Thus in *Almagest* 3.1 he adduces several passages from Hipparchus's works as supporting his value for the length of the tropical year, though his primary justification for that parameter comes from the comparisons of observed solstice and equinox dates in that chapter; 7.1-3 similarly draw on Hipparchus's authority as a secondary support for the elements of his precession theory. Previously, in 1.12, he has pointed out that his value for the obliquity of the ecliptic, again ostensibly derived from his own observations, is in agreement with values accepted by Eratosthenes and Hipparchus. On the other hand, in 9.1, without providing any additional rationale, he accepts the consensus of "pretty well all the leading mathematicians [σχεδὸν παρὰ πᾶσι τοῖς πρώτοις μαθηματικοῖς]" that the outermost spheres of the heavenly bodies in the cosmos are, in order of progressive proximity to the Earth, those of the fixed stars, Saturn, Jupiter, and Mars, while the Moon's sphere is closest to the Earth; but this seems to be a unique instance of Ptolemy's acquiescing in insupported authority, and the matters at issue do not come into play in the subsequent logical argument of the *Almagest*.

Thus the narratives of Ptolemy's mathematical works present the reader with a series of rational decisions determining, refining, and quantifying the models under investigation, where each decision is based on empirical evidence, or if pertinent empirical evidence is not available, on less reliable principles reflecting the presumed orderliness of nature. This appearance, however, is partly deceptive, because Ptolemy sometimes presents ostensibly empirical evidence that, on closer examination, could not be observed. These claims, which I will call pseudoempirical, are statements concerning things that are observable, such as musical pitches and apparent positions and speeds of heavenly bodies, but what is said about them could not have been confirmed or refuted under ancient observing conditions because the claimed behaviors are smaller than the limits of observational accuracy, and one can be fairly sure that Ptolemy knew this. Pseudoempirical claims really are untestable predictions of phenomena derived from the very models for which they are supposed to provide evidence, and as such they mask gaps in the deductive completeness of Ptolemy's treatises.

My primary purpose in this paper is to show that pseudoempirical claims are present in the *Harmonics* and, with some frequency, in the *Almagest*. (I am not aware of instances in the *Optics*.) Secondarily, I wish to raise the question of why Ptolemy sometimes chooses to adduce pseudoempirical claims instead of either genuine empirical evidence or metaphysical considerations such as simplicity or analogy. I begin with a case that arises in the *Harmonics*, because in that work Ptolemy discusses in some depth the limits of observational precision and their consequences for the appropriate use of the evidence of the senses. My other examples are from the *Almagest*, and are presented in the order that they appear in that treatise; but they also represent a progression of increasing complexity, leading to cases whose status as pseudoempirical is difficult to assess with certainty.

rapidly revolving eccenters as grounds for believing that Mercury's model is contiguous with the Moon's.

The Harmonics and the limits of auditory perception

The premise of the *Harmonics* is that the systems of intervals between the tuned pitches employed by the musicians of Ptolemy's culture were mathematical entities whose structures and properties could be described and explained by ὑποθέσεις ("hypotheses" or "models") built up out of ratios of whole numbers, just as astronomical phenomena are explained by ὑποθέσεις built up out of uniform circular revolutions. The quantitative and relational character of pitch intervals, Ptolemy argues, is inferred from the primitive observation that the pitches produced by a sounding object are dependent on quantitative elements such as the object's size and density and the distance between the point of origin of movement and the place where the air is struck and incited into motion. The effects of these quantitative elements must unite into a single, more abstract quantitative attribute called τάσις ("tenseness") that originates in the sounding body and spreads outward through the air, where it is manifested as sound. Only discrete intervals between sounds that maintain a fixed pitch are mathematically tractable, and they obviously have the character of relations, i.e. ratios, between magnitudes.

Our sense of hearing, in Ptolemy's view, is inherently inexact, but it is more reliable in certain kinds of responses and assessments than in others. At the high end of reliability are its recognition of certain pitch intervals as ὁμόφωνοι ("same-sounding") or σύμφωνοι ("together-sounding"), meaning that the pitches, though distinct, sound somehow the same or very nearly. The octave is an example of a "same-sounding" interval, and the fifth and fourth are examples of "together-sounding" intervals. Our auditory recognition of such special intervals is an elementary response to a pair of heard pitches that are approximately in a particular relation to each other, something more like a resonance than a measurement; Ptolemy gives an analogy of our visual recognition of circularity in an approximate circle, even if it is drawn freehand, which does not depend on a measurement of radii. Secondly, our hearing can reliably judge which of two heard pitches is higher so long as the interval between them is not smaller than some threshold. On the other hand, we cannot trust our hearing to correctly measure intervals or to compare the sizes of two heard intervals that are close to the same size and that do not have either their higher or their lower pitches in common.

Ptolemy's methodology for acquiring scientific knowledge involves iterative, alternating appeal to sense perception and reason to refine each other's contribution. Having arrived at a basic theoretical framework for harmonics, namely the modelling of pitch intervals as whole num-

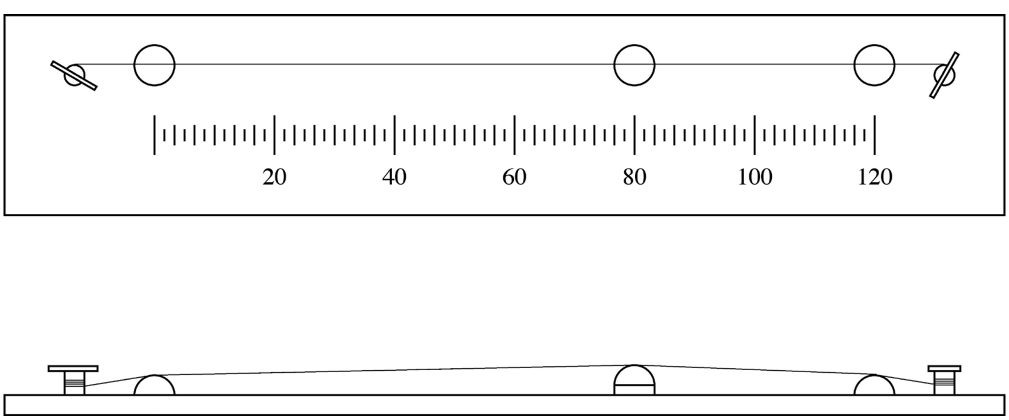

Figure 1. Ptolemy's monochord, top and side views. The bridges have spherical surfaces of equal radius. The middle bridge is movable and slightly raised relative to the fixed bridges; here it is set to produce a 2 : 1 ratio between the two parts of the string, which will be heard as an octave. The scale is divided linearly into 120 parts.

ber ratios, reason can direct the next stage of sensory investigation by devising observational techniques and instruments that exploit the model to enable one's hearing to make more exact perceptions, on the basis of which reason will take the modelling to the next stage. The instrument that Ptolemy advocates as particularly suited to harmonic research is the κανών, actually a class of instruments based on tensed strings divided by bridges into measured lengths, which produce pitches when struck or plucked. The κανών effectively reduces the sounding body to a single, easily controlled variable magnitude that has a simple and direct relation to the τάσις and hence to the perceived pitch. The simplest form of κανών is a monochord, which comprises a single string between two fixed bridges, and a movable bridge that divides the string into equally tensed parts whose lengths and ratio can be measured by a ruler running along the string's length (Fig.1).

Ptolemy employs the monochord only for the most basic demonstrations, while for more advanced work he prefers a κανών consisting of eight independent monochords. With the single monochord, we are merely to divide the string into simple whole number ratios for which we have a theoretical expectation that they correspond to "same-sounding" and "together-sounding" intervals, and verify that the pitches made by each part of the divided string produce the expected sensory response. Thus dividing the string into a 2 : 1 ratio results in a "same-sounding" octave, while dividing it into a 3 : 2 or 4 : 3 ratio results respectively in a "together-sounding" fifth or fourth.

Ptolemy also describes another demonstration that he ascribes to the followers of the fourth century B.C. Peripatetic philosopher Aristoxenos, which would most easily be carried out on a multi-string κανών. This demonstration concerns the relationship between the fourth and a smaller interval called the τόνος, which is the interval obtained by tuning up a fifth and then down a fourth from a given pitch, in other words the "difference" between a fourth and a fifth. One begins with two strings *a* and *b*, tuned by ear to sound at the interval of a fourth. String *c* is tuned (always by ear) two τόνοι above *a*, and string *d* is tuned two τόνοι below *b*. Lastly, string *e* is tuned a fourth below *c*, and string *f* is tuned a fourth above *d*. Strings *e* and *f* will ostensibly sound at the interval of a fifth, and from this it can be inferred that a fourth is equal to two and a half τόνοι.[7] A corollary of this result is that an octave consists of exactly six τόνοι.

The conclusion of the Aristoxenian demonstration is inconsistent with the equations in Ptolemy's model of the fourth with the 4 : 3 ratio and the fifth with the 3 : 2 ratio. Assuming Ptolemy's ratios, one obtains the ratio for the interval between *e* and *f* as $2^{18} : 3^{11}$ (i.e. 262144 : 177147), which is obviously not equivalent to a 3 : 2 ratio. Ptolemy has no doubt about which demonstration to trust: our hearing "all but screams out" that the monochord divided into 3 : 2 and 4 : 3 ratios produces the fifth and fourth, whereas the multiplicity of by-ear tunings in the other demonstration gives ample room for small perceptual errors that could cumulatively stretch the final interval into something heard as a fifth. Ptolemy calculates from his model that the interval between pitches a true fourth and two and a half τόνοι above a given pitch would correspond to a ratio of 129 : 128, and he remarks that even the followers of Aristoxenos would not assert that such a tiny interval could be judged by ear.

Though its appearance in a polemical context might lead one to suspect Ptolemy's statement that one cannot discriminate accurately between pitches in a 129 : 128 ratio (i.e. about 0.8%), it is in fact quite reasonable. While under ideal conditions hearing has been shown to capable of dis-

7 Half a τόνος is the interval such that if strings *i* and *j* are a half τόνος apart and strings *j* and *k* are a half τόνος apart, then *i* and *k* are a τόνος apart.

tinguishing pitches differing by as little as 0.4%,[8] 0.8% is about the average threshold of pitch discrimination for non-tone-deaf individuals comparing electronically generated sinusoidal tones of half a second's duration around 500Hz.[9] The threshold for comparisons of rapidly fading tones made by plucked strings would surely be still higher.

To make his point still more explicit, he invites the Aristoxenians to fetch the most skilled musician that they can find, and have him tune a series of seven strings at successive intervals of a τόνος, and he guarantees that the final pitch will not sound as an octave above the first. Hence either six τόνοι do not make an octave or no musician can be relied on to perform perfect tunings, and either way the Aristoxenians will be baffled. By contrast, if the strings are tuned according to calculated 9 : 8 ratios using the ruler (i.e. following the methodology of Ptolemy's simple monochord demonstrations), it will become apparent both visually and aurally that six τόνοι make an interval larger than an octave.

A pseudo-empirical claim about pitches

This statement that the ear cannot be expected to detect discrepancies as small as $^1/_{128}$ between intervals is worth remembering when one comes to *Harmonics* 2.1, which describes an exceptionally elaborate and ingenious demonstration, the capstone of the first major part of the treatise. Ptolemy has up to now been in pursuit of the mathematical rules determining the possible structures of *tetrachords*, sets of four pitches spanning an interval of a fourth, which were the building blocks out of which the various tuning systems (loosely, "scales") of Greek music were built. For our purposes it will not be necessary to review Ptolemy's investigations in detail. It is enough to know that, in accordance with the rules that he devises for the division of the tetrachord, he arrives at a very limited number of possible divisions, just six plus one that he presents as his own invention and one that he regards as a theoretically improper approximation to one of the others.

The purpose of *Harmonics* 2.1 is to show that four of the tetrachord divisions that the musicians of Ptolemy's time employed can be rigorously identified among the set that his theory has generated. The required apparatus consists of an eight-string κανών, treated as two sets of four strings, and a musician capable of making accurate tunings by ear. We are repeatedly asked to have one or the other of the two sets of four strings tuned to a particular tetrachord division from among those familiar to musicians; after the first time this is done, the additional condition is imposed that one of the pitches in the new tetrachord is tuned to match one of the pitches in the tetrachord that is already tuned on the other half of the κανών. Following each tuning operation, the observer compares specific pairs of pitches from among the eight strings, in most instances merely judging which pitch is higher, and inferences are made that ultimately lead to the identification of the four musician's tetrachords among the theoretically prescribed repertoire.

The procedure has more in common procedurally with the Aristoxenian demonstration than with those that Ptolemy has advocated hitherto, since the tunings are to be made by ear, not by calculation and the ruler. We are not asked to carry out long series of cumulative tunings between observations, however, so the effects of sensory imprecision can be expected to be less deleterious. Still, there is a problematic comparison where Ptolemy tells us that the second-lowest

8 Harris 1952.

9 Loui, Alsop, & Schlaug 2009, 10216; for details of the tests see Loui, Guenther, Mathys, & Schlaug 2008, supplementary data. A similar study (with similar results) is reported in Tervaniemi *et al.* 2005, 2-3.

pitch of one tetrachord "will be found to be a little sharper" than the corresponding pitch of the second tetrachord, an observation that provides a necessary step in his deduction of the identity of the latter tetrachord. According to Ptolemy's theoretical model for the tetrachord divisions that he has identified with the two sets tuned on his κανών at this stage, the interval between the two strings under comparison is 5120 : 5103, which is approximately 301 : 300. So if Ptolemy's model is correct, the difference between the pitches, if exactly tuned, would be much smaller than the 129 : 128 difference that, he previously claimed, could not be accurately judged by ear, and in fact it would be below the normal threshold of pitch discrimination. In other words, even if a good musician was doing the tuning, it would be a matter of chance which of the strings would turn out sharper than the other, and in any case they would likely be so close that the observer would have difficulty telling the pitches apart.

Ptolemy was certainly aware of the numbers that his theory predicted for this pair of pitches, though he does not state them in the chapter in question, and he can hardly have failed to realize that they were so close to equality as to make them indistinguishable under experimental conditions.[10] This realization need not have shaken his confidence in his model, but it would have shown him that the elaborate demonstration he devised in *Harmonics* 2.1 was faulty. If he actually performed the demonstration before including it in his treatise, and his musician consistently tuned the two strings in such a way that the first one always sounded slightly but unambiguously sharper than the second, he should have concluded that either the musician was systematically mistuning, rendering this stage of the demonstration worthless, or the model was false. If, on the other hand, in repeated trials the relation of the two pitches was inconsistent or indeterminate, he should have concluded that the model was confirmed up to this point but the demonstration could not proceed making use of this relation as an observed inequality.

The proper way to regard Ptolemy's statement about the perceived sharpness of one pitch relative to the other, I believe, is as a pseudo-empirical fact, a kind of ideal observation that Ptolemy is *sure* would be made if it were not prevented by the limitations of human sense perception and the physical conditions we work within. It plugs a rather small hole in the didactic structure of the *Harmonics*, which is all about the interplay of observation and reasoning, and Ptolemy likely felt that such stopgaps were unavoidable and pardonable if one was proposing to construct large scale mathematical deductions about the perceptible world.

The sphericity of the visible heavenly bodies

Ptolemy establishes the broad cosmological framework of the *Almagest* in a series of chapters, 1.3-8; the theses of these chapters, none of which would have surprised Plato or Aristotle, are founded on arguments that are predominently empirical though some aprioristic considerations also come into play. 1.3 addresses a twofold thesis, that "the heavens are spherical and move spherically [σφαιροειδῶς]."[11] Most of the empirical arguments adduce phenomena from which it can be inferred that, broadly speaking, the visible heavenly bodies all revolve daily on circular paths that are centered on a single axis and lie in planes perpendicular to it, which is effectively what Ptolemy means by "moving spherically." This conclusion is consistent with but does not prove the hypothesis that the heavens are, taken as a whole, spherical. To establish that, Ptole-

10 Proportional string lengths derived from the model are written, as sexagesimal approximations, in the accompanying diagram, though it is not certain whether this is a feature that goes back to Ptolemy or a medieval supplement.

11 The phrase appears at the end of 1.2.

my turns to a mathematical-metaphysical argument (that the heavens, being the largest of all bodies, should have the three-dimensional form that has the greatest volume in proportion to its surface) and two arguments that he calls physical, since they depend on assumptions about the properties of the etherial matter that he assumes the heavens to be composed of. The second physical argument is as follows:

> Nature has formed all mundane and perishable bodies generally from curved but nonhomeomeric shapes, and all those that are in the ether and divine again from (shapes that are) homeomeric and *spherical*—since if they were planar or disk-shaped, a circular shape would not be apparent to all who see (them) from various places of the Earth at the same time—and because of this, it is plausible that the ether surrounding them, being of similar nature, is spherical and travels circularly and uniformly on account of its being homeomeric.

("Homeomeric" here means that all parts of the surface or circumference are geometrically congruent, which is a property of circles, cylinders, and spheres.)

There are two empirical claims here, capped by the argument from analogy that we have already cited in passing. First, we see around us that naturally formed bodies are more or less all rounded, but those in our terrestrial environment are geometrically irregular while the ones in the heavens, by which Ptolemy certainly means the Sun and Moon and perhaps also the planets and stars, are geometrically regular.[12] Specifically, they are seen as having circular outlines, which would be compatible with their being circles or disks seen head-on or spheres seen from any direction. Secondly, their outlines appear circular no matter where we observe them from on the Earth's surface, which rules out their being actual circles or disks since, by a well known optical theorem, a circle is normally seen as oval when seen obliquely.[13]

Just three chapters later, however, Ptolemy demonstrates that the Earth has, to the senses, the relation of a point to the heavens (1.6). In other words, so far as observations are concerned, all points on the Earth's surface can be treated as the same point; hence the empirical argument that the visible heavenly bodies are not circular or disk-shaped has no force. Granted, Ptolemy's arguments pertain only to the distances of the fixed stars and the Sun, and in Book 5 he shows that the Moon is near enough to the Earth to exhibit a significant parallax. But even at its minimum distance according to Ptolemy's lunar model, 33 Earth-radii, the Moon would not be near enough for its outline to be seen from any point on the Earth as noticeably oval, supposing it had a planar circular face—especially since the minimum distance coincides with half-Moon phase.[14] Thus the statement that the heavenly bodies are always seen as circular from every terrestrial vantage point, while a legitimate empirical claim in its own right, becomes pseudoempirical in the context of the argument, since the reader is led to suppose that the theoretically predicted appearance of noncircularity of a flat-faced heavenly body would be perceived.

12 Ptolemy believed that the planets and stars have small but discernible apparent disks, and in *Planetary Hypotheses* 1B.5 (Goldstein 1967, 8) he offers estimates of their diameters.

13 Euclid, *Optics* 34-35 (in the first recension in Heiberg's edition) = 34-36 (in the so-called "Theonine" recension). According to these propositions, the circle's diameters will also be seen as equal if the eye lies at a distance from the circle's center equal to the circle's radius, but this obviously does not apply to the heavenly bodies.

14 Ptolemy *could have* argued for the Moon's sphericity on the basis of the appearance of its phases, just as in 1.4 he could have argued for the Earth's sphericity on the basis of the outline of its shadow on the Moon during lunar eclipses. Why Ptolemy does not mention these well-known arguments is a mystery.

The choice of solar model

Ptolemy introduces the eccenter and epicycle models as general approaches to modelling anomaly in *Almagest* 3.3. The next chapter turns to the specific solar model. Ptolemy writes:

> Now that these things have been set out in advance, there should be a preliminary assumption also about the anomaly apparent with respect to the Sun for the sake of its being single and causing the time from the least motion to the mean to be always greater than that from the mean to the greatest; for we find that this too is concordant with the phenomena. It can be accomplished by means of either of the models under consideration, making the proviso that (it is accomplished) by means of the epicyclic (model) such that the Sun's shifting towards the leading (signs) takes place on the arc of (the epicycle) that is farther from the Earth. But it would be more reasonable [εὐλογώτερον] to apply the eccentric model since it is simpler and accomplished not by two motions but by one.

Unpacking this paragraph, we see that Ptolemy starts out not with two candidate models but with three: the eccenter model, and two epicyclic models differentiated by the direction that the Sun revolves around its epicycle (Figs. 2-3). The process of deciding which one to adopt involves two stages. First, the version of the epicyclic model in which the Sun revolves around its epicycle in the opposite sense to that of the epicycle's revolution around the Earth is chosen over the other version in which the two motions have the same sense. This choice is made on the basis of an empirical claim, that the "phenomena" are concordant with having the time from slowest to mean apparent speed greater than that from mean to greatest, which was shown in 3.3 to be characteristic of the opposite-sense epicyclic model but not of the same-sense model. Secondly, the eccenter model is chosen over the opposite-sense epicycle model, not on empirical grounds, but according to a simplicity argument. An empirical discriminant would not in fact be possible at this stage, because the eccenter model and the opposite-sense epicycle model are (with appropriate choice of parameters) kinematically equivalent, that is, each would place the Sun at exactly the same point in space at any chosen time.

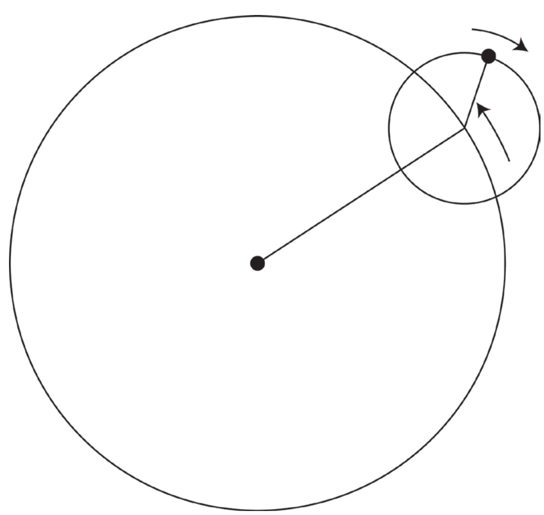

Figure 2. Opposite-sense epicycle model. The revolution of the body on the epicycle is considered in a geocentric frame of reference, i.e. relative to the revolving radius from the Earth to the center of the epicycle. If the two periods of revolution are equal, the radius from the epicycle's center to the body will maintain a fixed direction in a Cartesian frame of reference and the body will trace out an eccentric circular path at uniform actual speed.

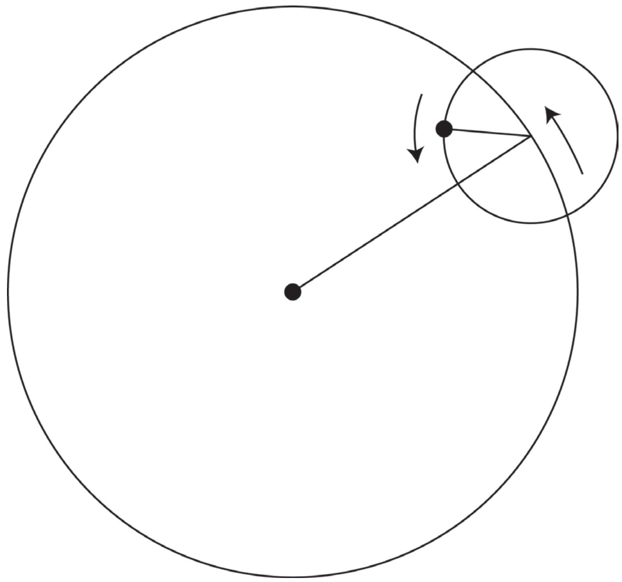

Figure 3. Same-sense epicycle model. If the periods of revolution in a geocentric frame of reference are equal, the body will trace out a closed epitrochoid that, for a small epicycle, closely approximates an eccentric circle with slight flattening around the perigee. The body's actual speed, as well as its apparent speed as seen from the Earth, are slowest at perigee and fastest at apogee.

Before examining Ptolemy's claim of establishing on empirical grounds that, considerations of simplicity aside, the opposite-sense epicycle model can be shown to be viable—and by implication, the same-sense model can be shown not to be viable—it is worthwhile to look at the treatment of this question by Ptolemy's older contemporary, Theon of Smyrna in his *Mathematics Useful for Reading Plato*. Theon motivates the application of eccenter and epicycle models to explaining the Sun's apparent motion in a way similar to *Almagest* 3.4, by asserting that the time intervals between the solstices and equinoxes are not equal whereas the angles separating the Sun's longitudes at these events are all 90°. He gives exactly the same figures as Ptolemy for the time intervals between vernal equinox and summer solstice (94½ days) and between summer solstice and autumnal equinox (92½ days), but he also gives the remaining two intervals between autumnal equinox and winter solstice (88⅛ days) and between winter solstice and vernal equinox (90⅛ days), which Ptolemy does not give. This is a significant difference. Ptolemy sets out to derive the parameters of his eccenter model from the given time intervals, a calculation that calls for observed dates of just three of the four events. Theon, however, simply posits—without even proving that this is geometrically possible—a position of the eccenter relative to the Earth and ecliptic such that the eccenter is divided by the solstitial and equinoctial lines in unequal quadrants proportional to the four given intervals, and then he asserts as bald facts the same parameters that Ptolemy derives trigonometrically, namely that the center of the eccenter is displaced from the center of the Earth and cosmos by $^1/_{24}$ of its radius in the direction of Gemini 5½°.

Theon now turns to the same-sense epicycle model, using the diagram shown in Fig. 4. This diagram shows the Earth as point Θ, the ecliptic as circle ΑΒΓΔ (with the direction of increasing longitude being counterclockwise), the deferent as circle ΜΟΝΞ, and the epicycle in four successive positions as circles ΕΖΗΚ, ΛΠ, ΦΥ, and ΧΨ. From a geocentric radial perspective, the Sun travels counterclockwise on the epicycle, from Ε to Ζ to Η to Κ and back to Ε, in the same time that the epicycle takes to revolve around Θ, so that its actual locations in relation to the whole

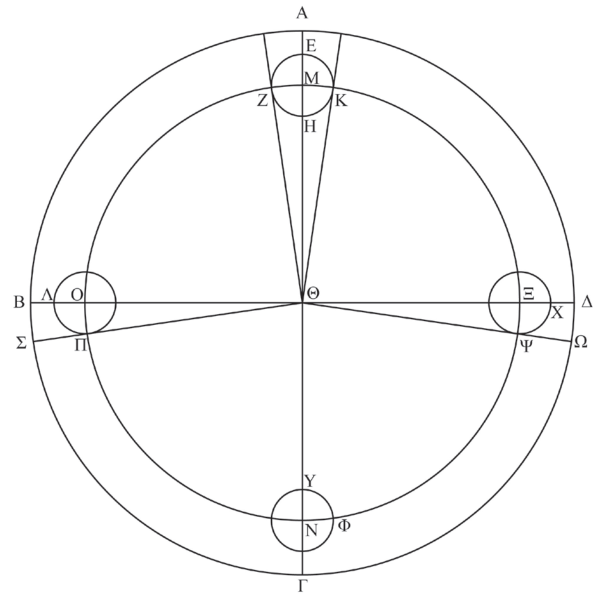

Figure 4. Theon's diagram for the same-sense epicycle model.

system are successively Ε, Π, Υ, and Ψ. For the observer at Θ these points are projected on the ecliptic as Α, Σ, Γ, and Ω. Noting that Ε represents the Sun's furthest distance from the Earth, Theon therefore equates this position with the apogee of Gemini 5½° that he previously gave for the eccenter model. From the diagram he has no difficulty in showing that the Sun sweeps out the larger arc ΩΑΣ in the same time interval (half a year) as it sweeps out the smaller arc ΣΓΩ, which means that the Sun appears to be moving faster around Gemini 5½° than around the diametrically opposite point, which contradicts the apparent speeds implied by the given time intervals between the solstices and equinoxes.

The diagram for the opposite-sense model (Fig. 5) is similar, but now the Sun revolves clockwise around the epicycle going from Ε to Κ to Η to Ζ and back to Ε in the same time as the epi-

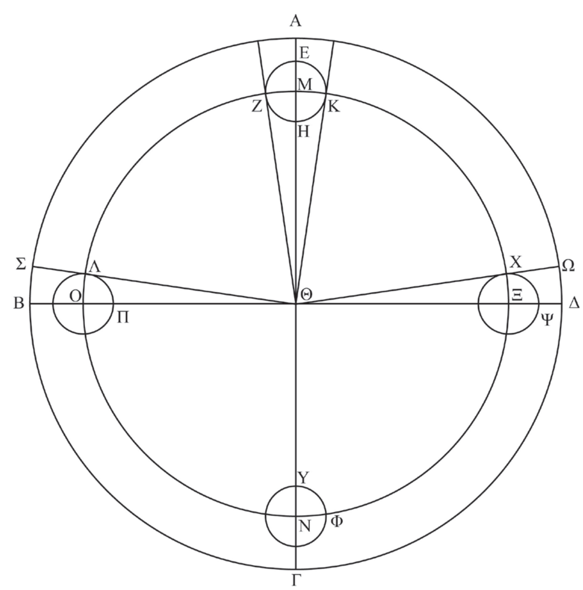

Figure 5. Theon's diagram for the opposite-sense epicycle model.

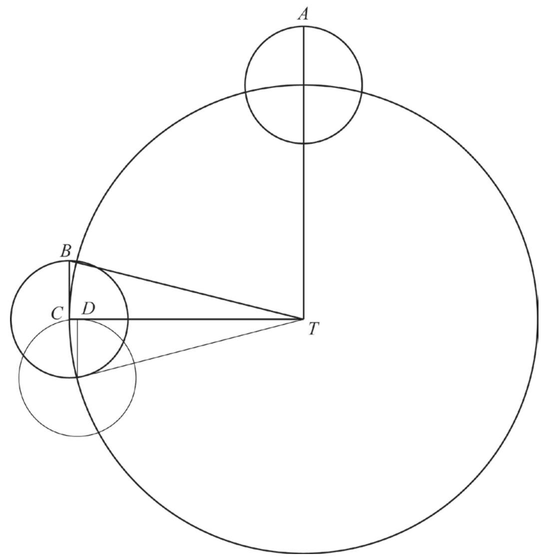

Figure 6. Behavior of the opposite-sense epicycle model. When the Sun is at its apogee, A, the apparent speed is slowest. One quarter of a year later, it is at B, but the moment when the apparent speed equals the mean is slightly later, when the Sun, at D, is at the point where TD is tangent to the epicycle. At this point the true anomaly, angle ATD, is 90°.

cycle revolves counterclockwise around Θ. The four positions of the Sun and their projections on the ecliptic are lettered as before, but now arc ΩΑΣ is smaller than arc ΣΓΩ, so that the Sun's apparent motion around Gemini 5½° is slower than around the diametrically opposite point, in agreement with the given time intervals. For Theon's didactic purposes, this is a sufficient demonstration that the opposite-sense epicycle model is viable for the Sun.

Theon's treatment of the same-sense model is obviously fallacious, because he has no right to assume that the apogee of the path traced by the Sun in this model must coincide with the apogee found for the eccenter model. In fact the opposite is the case: if one were to hypothesize a same sense model and then use trigonometric methods as in *Almagest* 3.4 to derive the direction of the apogee, it would turn out to be almost exactly 180° from Gemini 5½°. Hence in Fig. 4 the region around Γ would correspond to the part of the ecliptic around Gemini 5½°, so that the model predicts slower motion just where it ought to.

Ptolemy does not make this mistake. His criterion is whether the moments when the Sun appears to move at mean speed are closer to the moment of slowest apparent motion or that of fastest apparent motion, which is a valid discriminant between the two varieties of epicyclic model. In the opposite-sense model (Fig. 6), one quarter of a year's motion brings the Sun from its point of least apparent speed at apogee, *A*, to a position *B* such that its longitude less than the mean longitude, but its equation, angle *BTC*, has not yet reached its (subtractive) maximum, at which point the apparent speed equals the mean speed. This occurs a little later, when the Sun is at *D*. In the same-sense model (Fig. 7), when the Sun has travelled for a quarter of a year starting from its point of least apparent speed at perigee, *A*, it will be at exactly the same position *B* as in the opposite-sense model, but it will *already* have passed the point of maximum equation, *D*, when the apparent speed equalled the mean speed.

But how could one test this discriminant empirically? Ptolemy just writes vaguely that it is "concordant with the phenomena." He would have had absolutely no way of directly measuring the apparent speed of the Sun to a precision of even a minute per day, and around the dates of maximum equation, when the rate of change of the speed is greatest, it is accelerating or decel-

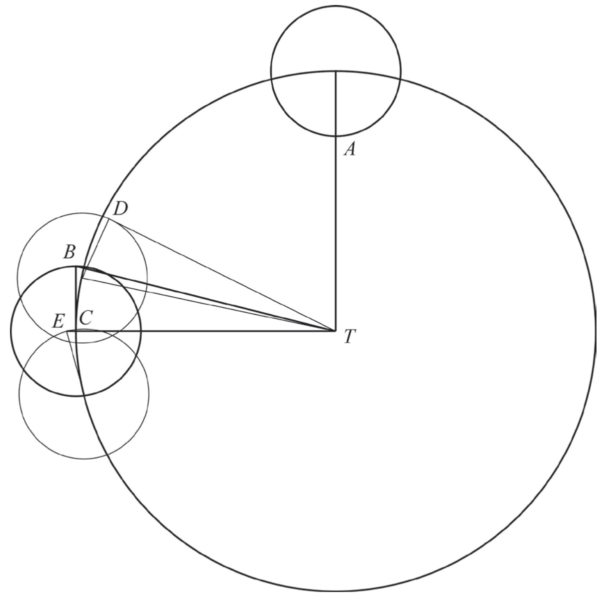

Figure 7. Behavior of the same-sense epicycle model. The apparent speed is lowest when the Sun is at its perigee, A. One quarter of a year later, it is at B, but the moment when the apparent speed equals the mean is slightly earlier, when the Sun is at D such that TD is tangent to the epicycle. The position of the epicycle when the true anomaly of the Sun, at E, is 90° is very close to the corresponding position in the opposite-sense model.

erating by less than three *seconds* per day. The observations on which the *Almagest*'s solar theory is based are solstices and equinoxes, which yield dated longitudes only at four points 90° apart. In principle Ptolemy could have tried to determine solar longitudes at other dates from the observed solar declinations, using a meridian instrument or armillary, but these would certainly not have been accurate enough to determine the date when the Sun's apparent speed was equal to the mean speed.

Even considering the apparent speeds cumulatively, that is, comparing observed longitudes over longer intervals with the predictions of the models, one would not be able to demonstrate empirically that one model is more successful than the other. If one computes solar longitudes for every day over an entire year according to Ptolemy's eccenter model (kinematically equivalent to the opposite-sense epicycle model) and according to a same-sense epicycle model having equivalent parameters and aligned with its perigee matching the eccenter model's apogee, the difference between the models has a maximum of just 6 minutes when the mean Sun is around 45° and 135° on either side of the apsidal line, well below the precision of longitudes derived from observed declinations. The effects on phenomena involving other heavenly bodies of discrepancies of 6 minutes in solar longitudes would also be too small to isolate; for example the times of true syzygies would be affected by about ten minutes at most.[15]

The most pronounced difference between the predictions of the two models from a geocentric point of view is in the distances of the Sun from the Earth. The opposite-sense model makes the Sun trace, with uniform true speed, an eccentric circular path whose apogee is at Gemini

15 Swerdlow 2004, 250 suggests that one could confirm Ptolemy's claim by observing that the time from when the Sun is at apogee (meaning Gemini 5½° or thereabouts) to when it is observed at 90° elongation from apogee is about five days greater than the time from 90° elongation to perigee. This is, however, practically the same as Theon's attempted demonstration; the phenomenon would be consistent with a same-sense epicycle model having its perigee near Gemini 5½°, as one can see by comparing the situations of the Sun and its epicycle at the moment of 90° elongation according to the two models in Figs. 6 (Sun at *D*) and 7 (Sun at *E*).

5½° according to Ptolemy's parameters. The same-sense model fitted to the same initial equinox and solstice observations makes the Sun trace, not quite uniformly, an epitrochoid that closely approximates the eccentric circle of the other model in shape and size, but with its *perigee* at Gemini 5½°. According to either model, the apparent diameter of the Sun's disk should be about $^1/_{12}$ greater at perigee than at apogee, so one might hope to use measurements of the diameter to determine which end of the apsidal line is the apogee. Whether an ancient observer could have detected a variation of this order of magnitude in the apparent solar diameter is an open question, but we know what Ptolemy thought: he writes in *Almagest* 5.14 that "we find that the diameter of the Sun is always subtended by approximately the same angle in every situation, with no significant variation arising from its distances."

Ptolemy's claim that the phenomena determine the required sense of the Sun's revolution in an epicycle model is thus pseudoempirical, in a stronger sense than the claim about the apparent disks of the heavenly bodies in 1.3, since here he is directly asserting that one can observe an unobservable effect.

Planetary epicycles and eccenters

In *Almagest* 9.5 Ptolemy presents a rationale for the structure of his models for the five planets. The key points are as follows:

> (1) The planet revolves uniformly around an epicycle, with the sense of revolution such that the planet travels in the direction of increasing longitude when it is on the part of the epicycle furthest from the Earth.
>
> (2) The center of the epicycle revolves around an eccentric deferent circle.
>
> (3) The apsidal line of the model shifts uniformly in the direction of decreasing longitude at the rate of precession.
>
> (4) The angular motion of the epicycle's center around the eccenter is such that its angular motion is uniform with respect to an equant point distinct from the center of the deferent.

Only the first two points are justified in this chapter on the basis of empirical claims similar to the one we have just examined concerning the Sun's motion.

Ptolemy begins by pointing out that there are two fundamental model types available, the eccenter model and the epicycle model, and that there are "similarly" (ὁμοίως) two anomalies in each planet's motion, the synodic anomaly correlated with the planet's elongation from the Sun, and the zodiacal anomaly correlated with the planet's longitude. Although he does not yet explicitly draw an inference from this conjunction of two pairs, the linkage by ὁμοίως clearly hints at the plausibility of a one-to-one correspondence: if one anomaly is caused by an epicycle, the other would be caused by an eccenter. This sets up a weak, aprioristic bias against a double epicycle model even before we have looked at the details of the two anomalies.

Ptolemy then offers a phenomenon that ostensibly proves that the synodic anomaly is produced by a same-sense epicycle:

> We find in the case of the five planets from various configurations observed successively and around the same parts of the zodiac that the time from the greatest speed to the mean is always greater than that from the mean to the least.

This appears to mean a procedure along the following lines.[16] For some selected region of the ecliptic, one determines from observation two dates when the planet was at its maximum elongation on either side of its mean longitude. If these belonged to different synodic cycles, one subtracts whole mean synodic cycles from the interval separating them, and what remains will be an estimate of either the interval from mean apparent speed through maximum and back to mean or that from mean through minimum and back to mean, on the assumption that the zodiacal anomaly has not significantly changed throughout. What Ptolemy asserts, then, is that the intervals from mean to greatest to mean speed are consistently more than half a mean synodic period, and those from mean to least to mean are consistently less. This is a test that could have been performed for all five planets; Saturn would be the most difficult case because of its small synodic anomaly, but careful observations and interpolation ought to have made it possible to detect the inequality of the intervals if not their precise length.[17] Ptolemy's inference that the synodic anomaly must be produced by a same-sense epicycle is true to the extent that an opposite-sense model can be ruled out, though he fails to mention that there exists an eccenter model with advancing apsidal line that is kinematically equivalent to the same-sense epicycle model.[18] The empirical claim itself, however, is sound.

Conversely, Ptolemy has an empirical argument to prove that the zodiacal anomaly is produced by an eccenter or an opposite-sense epicycle:

> In the case of the anomaly that is observed [θεωρουμένης] in relation to the parts of the zodiac, we find contrarily from the arcs of the zodiac taken up at the same phases or the same configurations that the time from the least speed to the mean is always greater than that from the mean to the greatest.

Ptolemy's highly compressed statement can be expanded as follows. We have deduced that the synodic anomaly results from the planet's revolution around an epicycle. We now wish to investigate the motion of the epicycle's center around the Earth, to see whether the time from this center's fastest apparent motion to the moment when its apparent motion equals its mean motion is greater or less than the time from that moment to the slowest apparent motion. Since the center cannot be observed directly, one uses multiple observations of the planet at a particular stage of its synodic cycle ("phases" or "configurations"), when it is approximately at the same point on the epicycle so that the center's longitude can be approximated from the observed longitude of the planet. On the basis of such observations, Ptolemy asserts that the time from least to mean speed is consistently greater than the time from mean to greatest speed.

16 This is a slight simplification and generalization of the second method outlined by Swerdlow 2004, 252. For an inferior planet, the mean longitude is the same as the Sun's mean longitude; for a superior planet, it is obtained from the relevant period relation. In either case the exact alignment of the mean longitude is not required, only a reasonably accurate rate of mean motion.

17 Swerdlow 2004, 251 asserts that a demonstration of this kind is only practicable for Venus. In the case of Mercury, it is true that the planet's day-to-day longitudes could only be observed adequately in certain portions of the ecliptic. However, his statement that the point of tangency on the epicycle cannot be observed for the superior planets is mistaken; one merely has to find the date when the difference between observed longitudes of the planet on successive days and the planet's calculated mean longitudes is at a maximum.

18 Ptolemy mentions the possibility of eccenter alternatives at *Almagest* 12.1, though only for the superior planets. See *POxy astron.* 4173 (Jones 1999a, 1.166-167 and 2.152-155) for a fourth century AD fragment of a set of mean motion tables, based on Ptolemy's but apparently pertaining to a system in which the inferior planets had epicycle models and the superior planets eccenter models.

Swerdlow remarks that the most direct way to carry out such a demonstration would be using oppositions of a superior planet, since this phase should coincide with the moment when the planet's observed longitude coincides with the longitude of the epicycle's center.[19] In fact oppositions (or accurately interpolated conjunctions) are the *only* synodic phenomena that have a hope of yielding meaningful results, since stations and first and last visibilities involve factors that make the elongation of the planet from its mean longitude significantly variable. A sufficiently dense collection of observed oppositions (which requires a sustained program of observations over several years or even decades, depending on the planet) would allow one to show by interpolation that the time from the point of slowest apparent speed to a longitude 90° greater is longer than the time from this point to the point of greatest apparent speed.[20] But this is a situation closely paralleling our examination of the Sun's anomaly. If the epicycle's center travels on an eccenter or an opposite-sense epicycle, the point when it has a longitude 90° away from the point of slowest apparent speed will be the point of mean apparent speed; but the nature of the model is precisely what we are trying to determine, so it would be a *petitio principii* to claim that we have demonstrated Ptolemy's empirical claim. A same-sense epicycle model with the perigee at the point of least apparent speed would be equally compatible with the observations, though it would result in the points of mean speed being closer to the point of slowest speed. As was the case with the Sun, there is no way that Ptolemy could have obtained a set of observations dense enough or precise enough to discriminate between an optimally-fitted same-sense epicycle model and the eccenter model that he adopted. It is noteworthy that, after ruling out the same-sense model, he chooses the eccenter over the kinematically equivalent opposite-sense epicycle by invoking not just the simplicity argument used in *Almagest* 3.4 but also the correspondence principle, that two distinct anomalies call for two distinct model types.

The precessional motion of the planetary apsidal lines

Ptolemy's justifications of the two remaining points about the planets' models that he asserted in *Almagest* 9.5 are more complex. In each case he provides a detailed demonstration based on dated observation reports for one or two planets, but for the remaining planets he gives only a brief general empirical claim. Thus he shows in 9.7 by an analysis of observations made in his own time and four centuries earlier that Mercury's apsidal line has shifted eastward by 4°, the amount corresponding to his rate of precession, but there is no corresponding demonstration for the other four planets, only a terse remark at the end of 9.7 that we find the hypothesis that the apsidal line has precessional motion to be "concordant" (σύμφωνον) "from the part-by-part fitting [ἐκ τῆς... κατὰ μέρος ἐφαρμογῆς] of the phenomena relating to the other planets."[21]

At this point it will be useful to have a sense of how well Ptolemy's models and their parameters are fitted to the motions of the planets. For this purpose we have used Ptolemy's *Almagest* models and the JPL Horizons ephemeris[22] to compute long runs of longitudes (comprising a

19 Swerdlow 2004, 253.

20 Swerdlow 2004, 253-254, illustrated for Jupiter.

21 The extension to the other planets is thus not simply an instance of analogical argument as stated in Jones 2005, 30 (cf. Swerdlow 2004, 254), though analogy is a latent, secondary consideration, since the reader is likely to infer that Ptolemy would not have postulated moving apsidal lines for the other planets purely on the basis of the "fitting of the phenomena" without the ostensibly secure example of Mercury.

22 http://ssd.jpl.nasa.gov .

Planet	*Date range*	*Mean Δλ*	*Standard Deviation Δλ*	*Maxima*
Mercury	AD 110-155 (46y)	−1.26°	2.86°	−9.30°/+7.07°
Venus	AD 100-163 (64y)	−1.24°	1.07°	−2.65°/+4.68°
Mars	AD 110-156 (47y)	−1.17°	0.38°	−2.11°/+0.03°
Jupiter	AD 140-151 (12y)	−0.95°	0.12°	−1.23°/+0.60°
Saturn	AD 120-149 (30y)	−1.15°	0.24°	−1.58°/+0.56°

Table 1. Fit of Ptolemy's models to runs of longitudes computed from the JPL ephemeris.

rough return to the initial Sun-planet configuration) for each planet at 5-day intervals,[23] and in Table 1 we display (1) the mean value of the difference $\Delta\lambda = \lambda_{\text{Ptolemy}} - \lambda_{\text{JPL}}$, which is the systematic longitudinal offset, incorporating the approximately −1° error in Ptolemy's tropical frame of reference for his own time, (2) the standard deviation of Δλ, and (3) the maximum positive and negative values of Δλ.

Next, using the same runs of JPL longitudes, we determined the optimum values for the three dimensional parameters of Ptolemy's models, namely the longitude of apogee λ_A, the eccentricity *e* of the deferent (scaled such that the deferent's radius is 60), and the epicycle radius *r* (according to the same scale), so that the standard deviation of Δλ is minimized.[24] In Table 2 we give Ptolemy's parameters, the optimized parameters, and the measures of fit for the models with the optimized parameters.

One is struck by how close Ptolemy's parameters for the superior planets are to their optimum values. Among them, the parameter that makes the largest contribution to error in predicted longitudes is Saturn's epicycle radius.[25] Venus's epicycle radius is also very accurate,[26] its apogee at least decent, but the eccentricity is significantly too large. Mercury has a good epicycle radius, too small an eccentricity, and a disastrously inaccurate apogee. In addition, the

Planet	*Ptolemy* λ_A	*e*	*r*	*Optimized* λ_A	*e*	*r*	*Mean Δλ*	*Standard Dev. Δλ*	*Maxima*
Mercury	190°	3	22.5	218.6°	4.57	22.14	−1.26°	2.05°	−6.69°/ +4.03°
Venus	55°	1.25	43 1/6	59.2°	0.83	43.35	−1.23°	0.73°	−1.94°/ +1.79°
Mars	115°	6	39.5	116.3°	5.89	39.48	−1.17°	0.31°	−2.40°/ +0.24°
Jupiter	161°	2.75	11.5	160.7°	2.69	11.54	−0.95°	0.09°	−1.12°/ 0.74°
Saturn	233°	3 25/60	6.5	234.1°	3.53	6.30	−1.15°	0.07°	−1.37°/ +1.02°

Table 2. Ptolemy's planetary parameters compared with optimally fitted parameters.

23 The *Almagest* longitudes were computed for mean noon, Alexandria, and the JPL longitudes for 14:00 UT.

24 For these optimizations we have retained Ptolemy's mean motions. The error contributed by Ptolemy's inaccurate tropical frame of reference is small over the time intervals used here.

25 Interestingly, in the earlier *Canobic Inscription* Ptolemy gave 6 1/4 for the epicycle radius, much closer to the optimum. One wonders what led him change it.

26 The precise value adopted by Ptolemy almost exactly the radius required in a simple epicycle model such that the greatest elongation is 46°, which is one of the traditional estimates reported in ancient sources (Pliny, *Naturalis Historia* 2.6.38 with attribution to Timaeus, see Duke 2002, 57).

Planet	*Ptolemy* 2nd cent. AD	3rd cent. BC	*Optimized* 2nd cent. AD	3rd cent. BC	*Shift*	*Adjusted Shift*
Mercury	190°	186°	218.6°	213.2°	5.4°	3.8°
Venus	55°	51°	59.2°	52.3°	6.7°	5.1°
Mars	115°	111°	116.3°	110.5°	5.8°	4.2°
Jupiter	161°	157°	160.7°	155.4°	5.1°	3.5°
Saturn	233°	229°	234.1°	230.0°	4.1°	2.5°

Table 3. Parameters for Ptolemy's planetary models optimized for his own time and 400 years earlier.

peculiar model Ptolemy assumes for Mercury, with its rapidly revolving eccenter and resulting double perigee, makes a large contribution to the error in predicted longitude; an optimally fitted model having the same structure as for the other planets would bring the standard deviation of $\Delta\lambda$ down to 1.67°.[27]

Thirdly, we determine the optimized parameters to fit runs of longitudes from the JPL ephemeris of the same length and density as those used above, but exactly 400 Julian years earlier. The apogees for Ptolemy's time and for four centuries earlier are compared in Table 3. The shift in longitude according to Ptolemy's theory is of course 4° for all planets. For the optimized apogees, we give both the shift in true tropical longitude and the shift reduced by 1.6° to correct for the accumulated error in Ptolemy's tropical frame of reference over four centuries.[28]

Now if Ptolemy's resources for locating Mercury's apogee in his own time were so defective that the result was nearly 30° off, it should be obvious that he could not have detected a shift on the order of 4°, as he claims, by comparing his result with observations from the third century BC, the oldest period from which he appears to have had planetary observation reports. The reasons behind the large errors in Ptolemy's parameters for Mercury as well as those behind the unnecessary special structure of his model cannot be recovered in detail, but the chief cause was undoubtedly the highly restricted conditions under which Mercury could be observed in proximity to fixed stars. An approximate idea of these conditions can be obtained from the numerous preserved Babylonian records from the last four centuries BC of observed positions of Mercury's position relative to the so-called Normal Stars. Figure 8 shows the planet's actual positions in longitude and latitude (according to modern theory) at the dates of these observations. The parts of the zodiac within which Babylonian observers were able to see Mercury together with a nearby Normal Star turn out to have been limited to two intervals of roughly half the zodiac, one of them applying to evening observations, the other to morning observations.[29] An observer in Alexandria or generally in Egypt would have had slightly better observing conditions for Mercury because of the lower terrestrial latitude, but there would still have been large "blackout" areas within which it would have been difficult or impossible to obtain accurate observed positions of the planet. This would apply both to Ptolemy's own observations and to any that were available to him from past centuries (e.g. by Timocharis or the unknown observers who recorded planetary observations with dates "according to Dionysius").

27 The parameters of the optimized conventional model would be λ_A = 218.6°, e = 2.75, and r = 22.74.

28 In any planetary observations used by Ptolemy, he has determined the planet's longitude relative to one or more fixed stars, whose positions for the date are found from his star catalogue adjusted according to his 1° per century precession rate.

29 The absence of reported observations of Mercury around 15°-30°, 240°-270°, and 300°-350° results from large gaps between stars in the regularly used Normal Stars; see Jones 2004, 481-491 with Figures 1-2 on pp. 493-494.

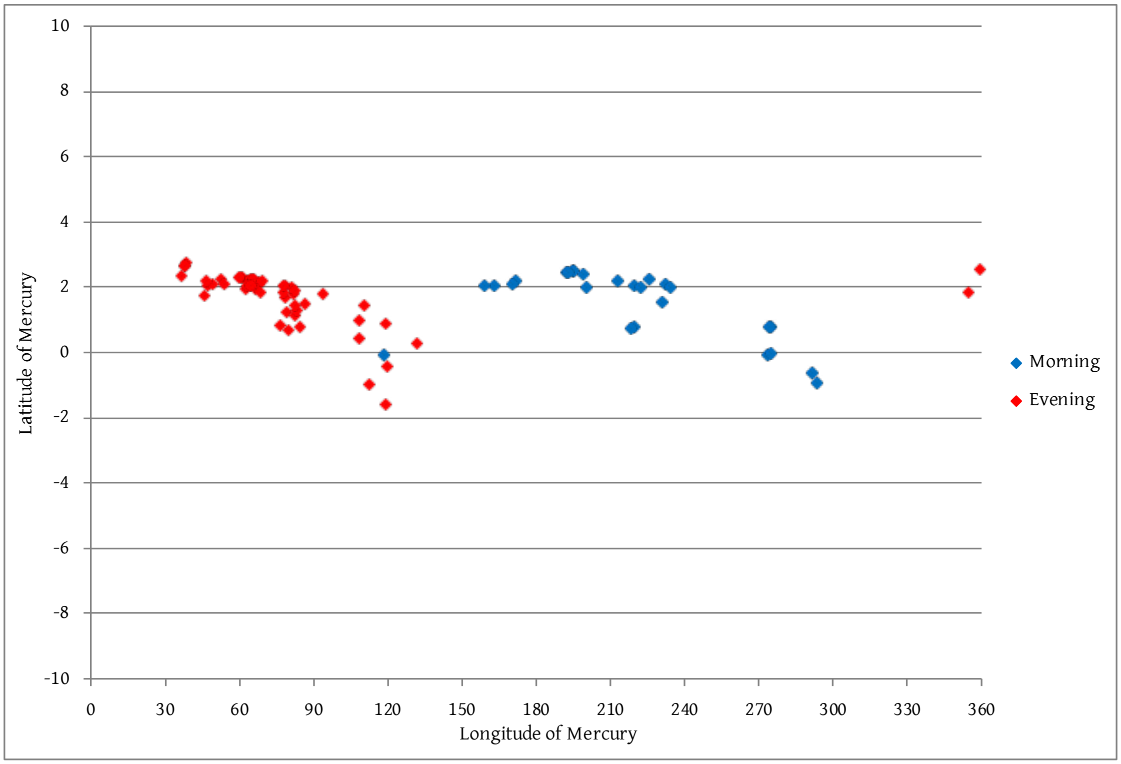

Figure 8. Locations of Mercury at the dates of preserved Babylonian observations of the planet near Normal Stars.

That Ptolemy nevertheless gives the appearance of demonstrating empirically that the longitude of Mercury's apogee and perigee were almost exactly 4° lower in the first half of the third century BC than in his own time is a tribute to his skill in manipulating an analysis of observation reports to yield a preordained result. The method, which involves finding a line of symmetry between one selected observation and an interpolation between two others, is very sensitive to small variations in the data, as Ptolemy has to have known by experience, and the agreement of the apogee that he obtains with his theoretical expectation depends on several small imprecisions in the calculations.[30] It is hard to avoid the conclusion that his analysis of the third century BC observations constitutes a pseudoempirical claim of a more elaborate kind than the general statements about solar and planetary speeds that we have considered up to now.

This places a greater burden on Ptolemy's brief and frustratingly imprecise statement that the phenomena for the remaining planets "fit" a precessional motion of their apogees. Read strictly, Ptolemy's wording could signify only that the phenomena are *consistent* with shifting apogees, but the context implies a stronger connotation, that the phenomena *require* them; the word translated above as "fitting," ἐφαρμογή, is used, for example, to signify coincidence of geometrical objects. The planets' apogees really do shift in a tropical frame of reference at rates close to that of precession as shown in Table 3 (in other words, in a sidereal frame of reference they move more slowly than the solstitial and equinoctial points). Could Ptolemy have detected and measured these motions for the planets other than Mercury by locating their apogees at a period several centuries before his time from old observation reports?

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30 See Jones 2005, 27-30 for details. Ptolemy's inclusion of two Babylonian reports of Mercury's passage by Normal Stars suggests that the selection of observations as well as their analysis was motivated by the desire to obtain a particular result, since such reports are very imprecise indicators of the planet's longitude.

In the case of Venus, the answer is almost certainly not. Ptolemy's value for the longitude of Venus's apogee in his own time, 55°, is ostensibly obtained as one of the midpoints of two pairs of observed equal but opposite greatest elongations of the planet from the mean Sun. But these observations do not stand up to closer scrutiny. In the first place, their reported dates all differ by from 12 to 20 days from those of the actual greatest elongations; these discrepancies far exceed the plausible range of error in determining these phenomena, and they are large enough so that the planet's elongation on the reported dates would have been significantly less than a greatest elongation occurring on that date would have been.[31] Two of the observations (AD 127 October 12, morning, and 132 March 8, evening) are ascribed to Theon the mathematician, and as reports of the location of Venus relative to nearby stars they appear to be reasonably accurate.[32] The other two (136 December 25, evening, and 140 July 30, morning), which Ptolemy says that he observed himself, both state that the planet was a small fraction of a degree from a nearby star when in fact its distance was well over a degree away; these are obviously fabricated positions chosen so that the elongations are exactly equal and opposite to the elongations of the observations by Theon with which they are paired off.[33]

As Swerdlow has convincingly argued, genuine observations of greatest elongations would only have allowed Ptolemy to determine the location of the apogee very roughly, at best to within a broad region of a zodiacal sign, and he likely chose the specific 55° longitude because it is approximately 90° from the mean solar longitudes for a pair of greatest elongations that he uses to locate the center of uniform revolution of Venus's epicycle.[34] Supposing that he had observations from, say, four centuries earlier of sufficient quality and density to allow him to determine a satisfactory collection of greatest elongations, he would have been in no better position to locate a precise apogee for the earlier date than for his own time.[35]

The situation with respect to the superior planets is different. Ptolemy's apogees, adjusted for the error in the tropical frame of reference, are correct to within half a degree for Mars and Saturn, and within a degree and a half for Jupiter. There is little reason to doubt that his apogees were determined by essentially the method of analysis of three observed oppositions to the mean Sun that he uses to demonstrate them in *Almagest* 10.7, and 11.1, and 11.5, although the ostensibly empirical data in these chapters have been adjusted so that the calculations yield more or less exactly the round number eccentricities that he adopts in his models.[36] If he was

31 See Swerdlow 1987, 36-43 and especially Table 1, p. 37.

32 Theon's distances are expressed in terms of the "length of the Pleiades" in these reports, adding a subjective element to the reduction of Venus's location to a precise longitude. The AD 127 report states that Venus appeared to be passing β Vir "one Moon" to the north, which must be a mistake since their latitudes were almost equal.

33 In the AD 136 report Ptolemy states that Venus was ⅔ of a "Moon" west of φ Aqr and that a near-occultation seemed to be about to occur, when in fact the planet was approximately 1¼° west of the star in longitude and almost a degree north of it in latitude. The AD 140 report has Venus half a "Moon" northeast of ζ Gem, but Venus was actually almost 1½° east of the star and at nearly the same latitude.

34 Swerdlow 1989, 41-43. Rawlins 2002 and Thurston 2002 show that the parameters for a model for Venus could be derived from analysis of an arbitrary set of three observed greatest elongations; there is no evidence, however that this method was used in antiquity, and Ptolemy clearly was not aware of it.

35 In any case, because of the small eccentricity of Venus's orbit and the imperfect fit of Ptolemy's model with any parameters to the planet's actual geocentric longitudes, the optimal apogee for the model fluctuates over a range of several degrees depending on the selection of date-longitude pairs to which the model is fitted.

36 Thurston 1994. The calculations in the *Almagest* have also been manipulated so that the long iterative procedure appears to converge faster than it really should (Duke 2005). I would guess that the *Almagest* parameters were

able to determine the apogees with comparable success from third century BC observations, a shift of their longitudes of the same order of magnitude as precessional motion would have been obvious.

For this, Ptolemy would have needed a sufficient number of reliable dated observations of each planet near each of several oppositions so that he could accurately estimate the moments when the planet was diametrically opposite the mean Sun according to his theory. There were certainly a few reports of near-oppositions among the early planetary observations available to Ptolemy. In *Almagest* 11.7 he cites a Babylonian report from 229 BC of Saturn near a Normal Star that, by his calculations, was four days before mean opposition. A report from 241 BC of Jupiter's location relative to fixed stars was cited in an early second century AD astronomical treatise that was probably known to Ptolemy; he would have determined that this was about two days before mean opposition.[37] But we have Ptolemy's own testimony in *Almagest* 9.2 that most of the old planetary observations preserved in his time were unsuitable for theoretical analysis, being mostly first visibilities, last visibilities, stations, and observations of positions such that the reported distances from fixed stars were too large to be considered reliable. His contention in this chapter is that satisfactory observational resources for working out proper planetary models did not exist much before his own time.

In any case, if Ptolemy had meant to indicate in 9.7 that the precessional shift of all the planets' apogees could be demonstrated by comparing apogees independently determined at widely separated periods, he would have written this more explicitly. His expression, invoking the ἐφαρμογή ("fitting") of phenomena to the hypothesis, actually harks back to a passage in 9.2 in which he warns the reader of certain methodological compromises that he will have to make in his planetary theory, including "hypothesizing certain primary matters not on the basis of an observed starting point [μὴ ἀπὸ φαινομένης ἀρχῆς], but having grasped them in accordance with continued trial and fitting [ἐφαρμογή]."[38] Such a process would mean, in the present instance, that Ptolemy tried out various possibilities for the behavior of the planets' apogees, and found that sidereally fixed apogees yielded the best agreement with observations at dates remote from his time.

The problem with this "better fit" account is that small changes in the assumed longitude of apogee do not have a very pronounced effect on the predicted longitudes of the planets, and in individual observations the effect would be obscured by noise. For example, if we compare our 12 years' worth of longitudes of Jupiter computed according to Ptolemy's parameters with longitudes computed according to the same model but with a 4° change in the apogee, we find that the differences never exceed ±0.5°, with a standard deviation of 0.26°. But the differences between Ptolemy's model and modern theory have a roughly ±0.3° range with standard deviation 0.12°, while even carefully selected observation reports would have been subject to errors on the order of, say, a sixth of a degree. The effect of apogee shift on longitude is still smaller in relation to the other errors for Venus and Saturn. Mars offers the best prospect for discerning it; for this planet, a 4° shift can lead to differences in predicted longitude as large as 2.5°, with standard deviation 0.70°, though 89% of the absolute differences are less than 1, and the larger

derived by selection or averaging of results from several sets of oppositions.

37 *P. Oxy. astron.* 4133 in Jones 1999a, 1.69-80 and 2.2-5; for the probable relation to Ptolemy see Jones 1999b. The observation was probably made by the same person whose report of Jupiter's position earlier in the same year is in *Almagest* 11.3.

38 "ὑποτίθεσθαί τινα πρῶτα μὴ ἀπὸ φαινομένης ἀρχῆς, ἀλλὰ κατὰ τὴν συνεχῆ διάπειραν καὶ ἐφαρμογὴν εἰληφότα τὴν κατάληψιν."

discrepancies occur around the planet's intervals of invisibility. Ptolemy would have been lucky to find suitable observations among his sources of early planetary observation records.

It seems likely, therefore, that Ptolemy's entire treatment of the motion of the apsidal lines, not just the demonstration for Mercury, is pseudoempirical. He presents this feature of his models in 9.5 as an additional complication to the basic epicycle-on-eccenter model, because his longitudinal frame of reference is tropical, but he is probably taking over the assumption that the planets' apsidal lines are tropically fixed from earlier planetary models that had this assumption by default since their frame of reference was sidereal.[39] Fortuitously, his shifting apsidal lines turn out to be a much better approximation to reality than tropically fixed lines would have been.

The eccentricities of Venus

Ptolemy's fourth point concerning the models for the planets is that the centers of uniform angular motion (i.e. equants) for their epicycles are distinct from the centers of the eccentric deferents. The special model for Mercury, described in 9.6, has the deferent's center revolving rapidly on a small circle whose center lies twice as far from the Earth in the direction of the apogee as the equant. We will not discuss Mercury's model further here. For the remaining four planets, the deferent's center lies at the exact midpoint of the Earth and the equant, a condition often called the "bisection of the eccentricity."

Ptolemy justifies the bisection of the eccentricity in a similar manner to his treatment of the motion of the apsidal lines, that is, he provides a detailed observation-based deduction of the two eccentricities for one planet, Venus (*Almagest* 10.2-3), but the extension of the hypothesis to the remaining three planets rests only on a general empirical claim (10.6). This claim is, however, more specific than the one provided in 9.7 for the motion of the apsidal lines of the planets other than Mercury:

> In the case of each of these (planets), following a general approach [κατὰ τὸ ὁλοσχερέστερον τῆς ἐπιβολῆς], the (eccentricity) found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac is found to be approximately double the eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle [ἐκ τῆς πηλικότητος τῶν περὶ τὰς μεγίστας καὶ ἐλαχίστας ἀποστάσεις τοῦ ἐπικύκλου προηγήσεων].

This passage has been much discussed in recent scholarship, and we can afford to be brief with it.[40] Ptolemy clearly means this argument to be a rough empirical indication of the bisection, not a summary of a rigorous deduction, in contrast to what he has previously shown for Venus. Whether or not this is what Ptolemy intended, the reader would likely interpret it as saying that there are two *independent* ways of nonrigorously estimating the eccentricity causing a planet's zodiacal anomaly, assuming an epicycle and eccenter model without yet differentiating between the center of the deferent and the center of the epicycle's uniform motion. One way, based on empirical information relating to the planet's retrogradations, indicates the variation in distance of the epicycle's center from the Earth and thus the deferent's eccentricity; the other,

39 Jones 2005, 29-30.

40 Evans 1984, 1088; Swerdlow 2004, 262-263; Jones 2004, 376-380.

based on equations of center derived from observations, indicates the location of the equant.[41] Read thus, the statement is simply false with respect to Jupiter and Saturn, while for Mars it is correct so far as it goes, but omits the crucial fact that the apogee derivable from Mars's retrogradations according to an equantless model is diametrically opposite that derivable from the equations of center. If Ptolemy had written this passage so as to give a more or less valid statement of the phenomena, it would have been something like this:[42]

> The eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle is, in the case of Mars, about half that found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac, with the apogee in the opposite part of the zodiac, while in the case of Saturn (the eccentricity derived from retrogradations) is about a third (of the eccentricity derived from equations of center), with the apogee in the same part of the zodiac, and in the case of Jupiter it is too small to determine.

Needless to say, a statement along these lines, while showing that the simple model is inadequate, would not make the necessity of bisecting the eccentricity appear obvious.

On the other hand, Ptolemy's statement could be understood as a radical compression of the following:

> The eccentricity derived from equations of center, if applied to a simple epicycle and eccenter model, does not yield retrogradations that agree with the phenomena. If, however, we assume that this is the eccentricity of the center of uniform motion, but that the eccentricity of the eccenter is half that, the predicted retrogradations agree with the phenomena.

This would be a valid description of an effect of the bisection, which is accurate within the range of precision of ancient observations and detectable for all three superior planets. It is likely what Ptolemy meant,[43] but one may reasonably suspect that he deliberately expressed the idea so as to convey a deceptive impression that a 1 : 2 ratio of eccentricities is apparent already from a "naïve" consideration of each planet's observed behavior. Despite the elliptical and misleading wording, I would *not* characterize this as a pseudoempirical claim.

The demonstration for Venus is a different matter. Here Ptolemy has a procedure that appears to truly separate the measurement of the epicycle's distance at apogee and perigee from the determination of the center of uniform motion. Let us first consider the latter.

Ptolemy has begun his treatment of Venus in 10.1 by locating its apsidal line as passing through 55° and 235° using the symmetry of pairs of equal and opposite greatest elongations; we have already noted that the reported observations are not genuine greatest elongations and are partly doctored, but here we can accept the result as a given. Comparing greatest elongations where the mean Sun was near either apsidal longitude, he establishes that 55° is the apogee and that the epicycle's radius is 43⅙ such that the deferent's radius is 60 (10.2); we will return pres-

41 The order of phrases in Ptolemy's Greek reinforces the impression that the method of deriving an eccentricity from retrogradations is to be thought of as independent of, and not subordinate to, the derivation from equations of center. The phrasing cannot be reproduced in literal translation, but this slight rewording conveys the effect: "… the eccentricity arising from the quantity of the retrogradations around the greatest and least distances of the epicycle is found to be approximately half that found by means of the greatest difference in the anomaly dependent on (the position in) the zodiac."

42 Jones 2004, 377-379.

43 This is how Swerdlow 2004 interprets the passage.

Ptolemy date	*mean Sun*	$\lambda_{♀}$	*elongation*	*Modern date*	*mean Sun*	$\lambda_{♀}$	*elongation*
134 Feb 18, AM	325½°	$281^{11}/_{12}$°	$43^{7}/_{12}$°	134 Feb 15, 8 UT	323.55°	279.24°	44.31°
140 Feb 18, PM	325½°	13⅚°	48⅓°	140 Feb 19, 20 UT	327.52°	15.77°	48.25°

Table 4. Two greatest elongations of Venus according to Ptolemy's data and modern theory.

ently to this part. Now he selects a pair of observed greatest elongations, one in either direction from the mean Sun, with the mean Sun being approximately 90° from the apogee at both dates; by hypothesis, this means that a perpendicular dropped from the epicycle's center to the apsidal line will intersect it at the center of uniform motion. Hence by a simple trigonometrical calculation involving the sum and difference of the elongations and the epicycle radius, he finds that the center of uniform motion is almost exactly 2½ units from the Earth such that the deferent's radius is 60.

The calculation is legitimate in principle, and moreover Ptolemy was able to find a pair of greatest elongations that met the criterion of having the mean Sun close to 90° from the apsidal line, which is not something that can be taken for granted.[44] In Table 4 we give the data for these events according to Ptolemy's report as well as according to the JPL ephemeris.[45]

The first thing that is apparent from this table is that Ptolemy's dates are very close indeed to the true dates of greatest elongation, close enough that the longitudes of Venus on Ptolemy's dates can be treated as longitudes at greatest elongation without introducing any significant error in the ensuing calculations. (Incidentally we can also see that Ptolemy was capable of determining dates of Venus's greatest elongation within a margin of very few days.) The second thing we notice is that Ptolemy's observed longitudes of Venus cannot both be accurate, since the sum of his elongations (which should be independent of the mean Sun) is approximately 91.92° whereas according to modern theory it is 92.56, a discrepancy of close to two-thirds of a degree. In fact, taking into account the systematic error of approximately −1° in Ptolemy's tropical longitudes for this range of years, it turns out that the longitude reported by Ptolemy for the earlier greatest elongation is about ⅔ ° too low, while the reported longitude for the later one is approximately correct.

Of course a measurement error of ⅔ ° is implausibly large, especially for an event that by its nature had to be determined by repeated observations on successive nights. It is also enough to make a significant difference in the calculated eccentricity of the center of uniform motion. If we repeat Ptolemy's computations using 44.31° and 48.25° as the given elongations, the eccentricity becomes 2.1 units instead of 2.5.[46] We may recall that our optimized eccentricity of deferent for an equant model of Venus was 0.83 (Table 2), so that the optimized eccentricity of the equant would be about 1.6. It is reasonable to suspect that 2.5 was a predetermined quantity (likely to match Ptolemy's solar eccentricity), and that Ptolemy has adjusted the longitude of one of his greatest elongations to get this result.

44 Since Venus performs five synodic cycles in almost exactly eight years, any particular synodic phenomenon (such as a greatest morning elongation) will only occur within a span of several decades with the mean Sun in five narrow and more or less equally spaced intervals of the ecliptic.

45 To estimate the mean Sun for the modern theory calculations, we use the Sun's mean longitude according to the Almagest solar theory plus 1° to compensate for the error in tropical frame of reference.

46 Shifting the assumed longitudes of the mean Sun by 0.1°, which affects only the difference between the elongations, changes the resulting eccentricity by about 0.1 units.

Ptolemy's method of finding the eccentricity of the deferent requires two observed greatest elongations, one with the mean Sun at Venus's apogee and the other with the mean Sun at the perigee; a trigonometrical calculation yields both the eccentricity and the epicycle's radius. Again the procedure is theoretically sound, but the observations that it requires could not have been made in the interval between AD 120 and the mid 140s that encompasses the observations that Ptolemy uses for his planetary theories. The observations that Ptolemy claims to be greatest elongations with the mean Sun at 55⅖° and 235½°, i.e. within a degree of the two ends of his apsidal line, are respectively 14 and 25 days distant from the true greatest elongations, and his reported longitudes have probably both been doctored, in the case of the latter one (AD 136 November 18) by more than a degree, to enlarge the elongations.[47] The elongations that he assigns can hardly be better than guesses, and while the elongation for his claimed observation with the mean Sun at perigee, 47⅓°, would be about right for an actual greatest elongation in this situation, the elongation for the claimed observation at apogee, 44⅘°, is about ¾° too small. If Ptolemy somehow had access to well-observed greatest elongations with the mean Sun truly close to the apogee and perigee—the most recent candidates would have been in the mid first century AD and the late first century BC respectively—he would have had about 45.6° and 47.3° for the elongations, leading to an eccentricity of deferent of approximately 0.85 units, in pretty good agreement with the eccentricity in our optimized model. The elongations that he gives in 10.2 must, however, have been chosen to yield predetermined values for both the eccentricity and epicycle radius.[48]

To sum up the situation, Ptolemy's approach to demonstrating the bisection of Venus's eccentricity depends on the availability of observations of Venus at its greatest elongations from the mean Sun when the mean Sun is in highly particular locations: at apogee, perigee, and 90° from the apsidal line. Only for the last of these conditions did suitable greatest elongations take place in Ptolemy's own time, whereas for the others he would have needed reports from as long as a century and a half earlier. But it is clear that he did not have these; otherwise why did he not use them explicitly in the *Almagest*, or at least make the simulated observations that he does cite agree with them? More generally, if Ptolemy's belief that Venus had an equant model with a bisected eccentricity was based on empirical evidence, how could it have come about that both his eccentricities, ostensibly found by independent analyses, are about 50% too large? I conclude that, although a viable procedure in the abstract, Ptolemy's deduction of the eccentricities in 10.2-3 is *for his circumstances* pseudoempirical. His observations may have sufficed to indicate that the eccentricity of the deferent is smaller than that of the center of uniform motion, but the choice of specific ratio must really have depended on analogy with the superior planets.

47 The report for AD 129 May 20, which is attributed to Theon, is a rather accurate description of the location of Venus relative to two stars, but Ptolemy's reduction of the information to a longitude involves inaccuracies that deducted nearly half a degree.

48 Swerdlow 1989, 43 suggests that Ptolemy could have estimated the deferent's eccentricity by using the actual greatest elongations that occurred nearest to the false dates of greatest elongations used in 10.2, treating them as if they were really in the apsidal line; such a calculation does result in an eccentricity of about 1.3 units, close to Ptolemy's 1.25. But when the mean Sun is away from the apsidal line, one must use the sum of a pair of oppositely oriented greatest elongations, not just a single greatest elongation, to determine the distance and radius of the epicycle because the epicycle's center is assumed to revolve uniformly around the equant, not the Earth. Doing the calculation with the two actual greatest elongations nearest perigee and the two nearest apogee in Ptolemy's time, one would again get an elongation of about 0.8 units.

General remarks

Before turning to the general question of why Ptolemy incorporated pseudoempirical claims in the two treatises considered in this article, we may note in passing that all the examples we have discussed from the *Almagest* lead Ptolemy to conclusions that are, in a qualified sense, correct. The Sun, Moon, and planets really are spherical; the Sun really is furthest from the Earth when its apparent motion is slowest; when a planet's heliocentric revolution is optimally approximated in a geocentric frame of reference by an epicycle and eccenter model, the center of the epicycle really is farthest from the Earth too when its apparent motion is slowest; the apparent apsidal lines of these geocentric approximations really do precess in a tropical frame of reference at rates on the order of magnitude of the precession of the fixed stars; and bisecting the eccentricity yields a good fit for an equant model of Venus. On the other hand, Ptolemy's broad theoretical hypothesis for the *Harmonics*, that all pitches in the tuning systems of Greek music should correspond exactly to whole number ratios subject to certain constraints, would not be considered viable today, so we would not accept the conclusions that he draws from any of the comparisons of heard pitches described in *Harmonics* 2.1, including the problematic one we have discussed.

If we ask what were the hidden nonempirical reasons behind the choices that Ptolemy ostensibly justifies by pseudoempirical claims as well as why Ptolemy leaves these reasons hidden, the most plausible answers differ from case to case. His avoidance of same-sense epicycles to model the anomaly of the Sun and the zodiacal anomaly of the planets seems to be a case of preferring a simpler model. In comparison with the eccenter or even the opposite-sense epicycle model, the same-sense model produces the effect of anomaly in a rather perverse way, by slowing down the actual speed precisely where its proximity to the Earth should give an appearance of swiftest motion and *vice versa*. I suggest that Ptolemy did not offer such a simplicity argument in the *Almagest* because the models, though to the senses indistinguishable, are nevertheless kinematically distinct. It is an extreme case of the priority that Ptolemy attributes to empirical evidence over aprioristic reasoning, which he invokes elsewhere (*Almagest* 13.2) to justify adopting *complex* models. Here, where a choice of model suggested by considerations of simplicity could notionally be confirmed by observation if our means of observing were only sensitive enough, and is definitely not refuted by the observations we can make in reality, Ptolemy offers the reader the kind of positive evidence that *ought* to be available.

In other cases, analogy seems to have been the primary form of reasoning. Thus Ptolemy had a sound empirical basis for the bisection of the eccentricity of Mars, Jupiter, and Saturn, even if he was unable to present it as a fact deduced straightforwardly from observation reports so that he had to justify it as a case of "fitting" the phenomena to an otherwise incompletely motivated model; it would have appeared plausible to assume that the bisection applied also to Venus, for which the empirical evidence pertaining to the eccentricities was murky. However, it would have seemed desirable to offer the reader at least one clear demonstration of the bisection from observations, and Venus was a good candidate for this not just because it came before the superior planets in the order of presentation in *Almagest* Books 10-11, but because it was comparatively easy to design a procedure for notionally isolating the two kinds of eccentricity based on greatest elongations. As for the shapes of the visible heavenly bodies, the Moon's sphericity was obvious from the appearance of its phases, so one would expect the Sun, planets, and stars to be spherical too; but it might have seemed too bold to apply analogical reasoning twice in a row, to go from the Moon alone to all the visible bodies, and then from the visible bodies to the invisible etherial bodies composing the bulk of the heavens. Finally, the decision to have the planets' apsidal lines sidereally fixed looks like an essentially cautious move; from Ptolemy's point of view (with his commitment to a tropical frame of reference as the best description of reality), precessing apsidal lines were not the choice recommended by considerations of simplicity, but he may have been reluctant to posit a long-term behavior for them that was different from the consensus of earlier planetary models and tables in the absence of decisive empirical proof one way or the other.

Overlaying the particular considerations that may have led Ptolemy to introduce pseudoempirical arguments is the privileged status of mathematical science as he delineates it in *Almagest* 1.1. The two defining characteristics of mathematics, according to Ptolemy's account, are that its objects are intrinsically knowable as exact things (unlike the objects of physics) and, crucially, that through our senses we are able to grasp these objects (unlike those of theology). Ptolemy knows that there are limitations to our ability to know mathematical realities through our senses, and sometimes he admits it. But one senses that the severe standard set by his conception of what we would call "mathematical sciences" as "mathematics" *tout court*, together with the principle that all knowledge comes ultimately from the senses, drove him from time to time to push the boundary between secure reasoning based on the senses and plausible reasoning based on metaphysical considerations or consensus further in the direction of the senses than his circumstances allowed.

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On the distances of the sun and moon according to Hipparchus

Christián C. Carman

Introduction

Among the Ancient astronomers that tried to measure the distance of the sun and moon, three stand out: Aristarchus of Samos, Hipparchus and Ptolemy. But, while we know with certainty what was the procedure followed by Aristarchus and Ptolemy, because the works in which they made the calculations survived, we cannot but conjecture what was Hipparchus's method, because the book in which he probably made the calculations, titled *On Sizes and Distances* or *On Sizes and Distances on the Sun and Moon*[1], did not come down to us. Nevertheless, we have two important references to the content of the book: the first from Ptolemy and the second from Pappus, who, commenting on Ptolemy's passage, added important information, including some values for the distances.

After discussing the difficulties for obtaining the lunar distance, Ptolemy (Almagest V, 11; Toomer 1998:243-244) says:

> Now Hipparchus used the sun as the main basis of his examination of this problem. For, since it follows from certain other characteristics of the sun and moon (which we shall discuss subsequently) that, given the distance to one of the luminaries, the distance to the other is also given, Hipparchus tries to demonstrate the moon's distance by guessing at the sun's. First he supposes that the sun has the least perceptible parallax, in order to find its distance, and then he uses the solar eclipse which he adduces; at one time he assumes that the sun has no perceptible parallax, at another that it has a parallax big enough [to be observed]. As a result the ratio of the moon's distance came out different for him for each of the hypotheses he put forward; for it is altogether uncertain in the case of the sun, not only how great its parallax is, but even whether it has any parallax at all.

Pappus, commenting on this passage asserts:[2]

> Now, Hipparchus made such an examination principally from the sun, [and] not accurately. For since the moon in the syzygies and near the greatest distance appears equal to the sun, and since the size of the diameters of the sun and moon is given (of which a study will be made bellow), it follows that if the distance of one of the two luminaries is given, the distance of the other is also given, as in Theorem 12, if the distance of the moon is given and the diameters of the sun and moon, the distance of the sun is given. Hipparchus tries by conjecturing the parallax and the distance of the sun to demonstrate the distance of the moon, [but] with respect to the sun, not only the amount of this parallax, but also whether it shows any parallax at all is altogether doubtful. For in this way Hipparchus was in doubt about the

1 See Toomer 1974: 127, note 1 for a discussion of the title.

2 The translation is taken from Swerdlow 1969: 18-19, except for a short paragraph around the end of the text that Swerdlow didn't translate. The translation of this paragraph was taken from Toomer 1974: 127.

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> sun, not only about the amount of its parallax but also about whether it shows any parallax at all. In the first book "On the Sizes and Distances" it is assumed that the earth has the ratio of a point and center of the [sphere of the] sun. And by means of the eclipse adduced by him first it is assumed that the sun shows the smallest parallax, then a greater parallax. And thus there arose the different ratios of the distances of the moon. For, in book 1 of "On Sizes and Distances" he takes the following observation: an eclipse of the sun, which in the regions round the Hellespont was an exact eclipse of the whole solar disc, such that no part of it was visible, but at Alexandria by Egypt approximately four-fifths of it was eclipsed. By means of this he shows in Book 1 that, in units of which the radius of the earth is one, the least distance of the moon is 71, and the greatest 83. Hence the mean is 77. Having shown the foregoing, at the end of the book he says: "In this work we have carried our demonstrations up to this point. But do not suppose that the question of the moon's distance has been thoroughly examined yet. For there remains some matter of investigation in this subject too, by means of which the moon's distance will be shown to be less than what we have just computed." Thus Hipparchus himself also admits that he cannot be altogether sure concerning the parallaxes. Then, again, he himself in Book 2 of "On Sizes and Distances" shows from many considerations that, in units of which the radius of the earth is one, the least distance of the moon is 62, the mean 67⅓, and the sun's distance 490. It is clear that the greatest distance of the moon will be 72⅔.

These texts are not too clear, they are not totally consistent with each other, and one surely would like Ptolemy and Pappus to be more explicit. Nevertheless, at least a couple of things seem clear: Hipparchus conjectured the solar distance for obtaining lunar distances. In the first book he obtained the set 71—77—83 for the lunar distance and the solar distance is not mentioned. In this calculation, Ptolemy used the solar eclipse described in the text. In book two, however, he made a new calculation and obtained a new set of values: 62—67⅓—72⅔. In this case, a solar distance is mentioned: 490, but there are no details about the method used to obtain these distances. Pappus says that Hipparchus obtained the values "from many considerations."

The fact that in each case Pappus mentioned three distances, the minimum, the mean and the maximum is easily explained. As Toomer 1967 has shown, the three distances of each set are consistent with assuming one of the two Hipparchian attested proportions between the radii of the epicycle and deferent: $r/R = 247.5/3122.5$. So, probably Hipparchus obtained just one value for the lunar distance conjecturing the solar distances, and calculated the other two using the proportion r/R.

Three important steps have been taken up to now in recovering the methods used by Hipparchus. The first, a step now understood to have been in the wrong direction, is due to Friedrich Hultsch in 1900, the second helped to understand the method used in book two and was made by Swerdlow in 1969 and the third one was made by Toomer in 1974, helping to understand the method used in book one. No more significant steps have been taken since then. In this paper we will show that both Swerldow's and Toomer's steps are in the right direction, but that one more step could be taken that would render Hipparchus procedure even more consistent and smarter.

Swerdlow and Toomer

In his *Mathematics useful for Reading Plato*, when he is explaining the nature of eclipses, Theon of Smyrna mentions that Hipparchus found the sun to be 1880 times the size of the earth and the earth 27 times the size of the moon (Martin 1849: 320-321). Hultsch (1900) conjectured that The-

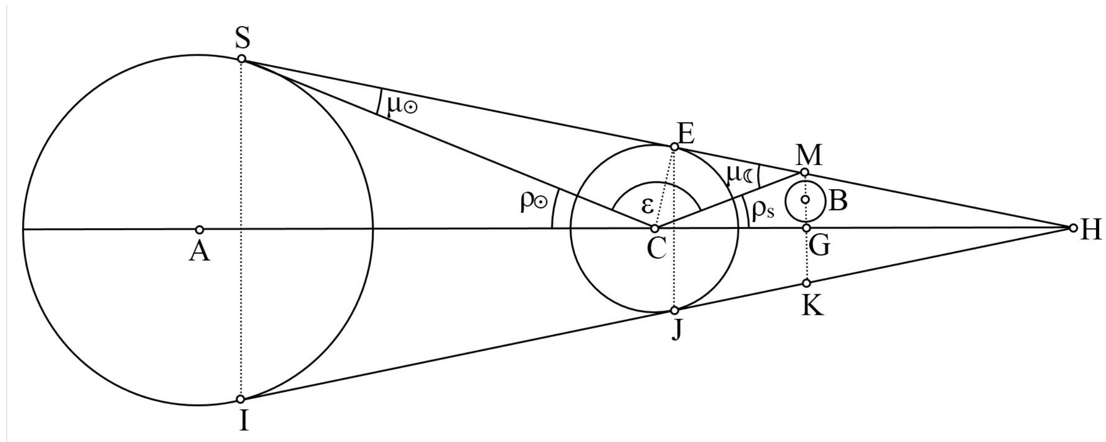


Figure 1. Hipparchus's method for calculating the lunar distance using a lunar eclipse. The center of the sun is at *A*,

on is talking about volumes and, therefore, the diameters are proportional to the cube root of these values. Accordingly, the sun is around 12⅓ times the earth and the earth 3 times the moon. Consequently, the sun is 12⅓ · 3 = 37 times bigger than the moon. But, because both luminaries have the same apparent size, this proportion expresses also the proportion between the distances. Now 37 times the mean distance (67⅓) is 2,491⅓ which could be rounded to 2,490. Hence, Hultsch (1900: 190-191) concluded that the correct number in Pappus' text must be 2490 (͵βυϙ) but the initial β disappeared leaving υϙ = 490 in the manuscripts. This modification was generally accepted and even incorporated in the text in A. Rome's edition of Pappus commentaries of books 5 and 6 of Ptolemy's *Almagest* (Rome 1931).

Swerdlow's reconstruction: the values of the second book

In 1969, however, Swerdlow proved Hultsch to be wrong. In the *Almagest*, V, 15 (Toomer 1998: 255-257), Ptolemy obtains the solar distance assuming a certain lunar distance. The procedure is perfectly reversible; you can obtain a lunar distance assuming a solar distance. The method used has its origin in Aristarchus and it is known as the eclipse diagram method. Because there is also a method that follows from the solar eclipse used by Hipparchus, I will call Aristarchus's method the *lunar eclipse method* and the method based on the solar eclipse, the *solar eclipse method*. Swerdlow shows that, if the data that Ptolemy says that Hipparchus used is used as input data, the solar distance that follows from the lunar mean distance 67⅓ is very close to 490. Thus, at the same time, Swerdlow showed that the value as it is in the manuscripts is correct (490), and identified the method used by Hipparchus.

Let me explain briefly the lunar eclipse method and how Hipparchus used it. Figure 1 represents a lunar eclipse: the center of the sun is at *A*, the center of the earth at *C* and the center of the moon at *B*. The triangle *SHI* represents the light cone of the sun, and therefore, the triangle *EHJ*, the part of the cone that represents the earth's shadow produced by the lunar eclipse. Angles $\mu_\odot$ and $\mu_☾$ represent the horizontal parallax of the sun and moon, respectively; angle $\rho_\odot$ represents the apparent radius of the sun (and consequently, also of the moon), while ρ_s represents the apparent radius of the earth's shadow at the lunar distance. If you look at triangle *DBF*, it is easy to see that $\mu_\odot + \mu_☾ = 180 - \varepsilon$ but, also $\rho_\odot + \rho_s = 180 - \varepsilon$. Therefore,

1 $$\rho_☾ + \rho_s = \mu_\odot + \mu_☾$$

i.e., the addition of the horizontal parallaxes is equal to the addition of the apparent radii of the moon and shadow. But we also know that:

2 $$CS = \frac{1}{\sin \mu_\odot}$$

and

3 $$CM = \frac{1}{\sin \mu_☾}$$

CS is approximately the sun-earth distance and CM the earth-lunar distance. Therefore, combining the three equations, we have that:

4 $$CM = \frac{1}{\sin\left(\rho_☾ + \rho_s - \sin^{-1}\frac{1}{CS}\right)}$$

So, knowing $\rho_\odot$, ρ_s and one of the distances, one can know the other. Fortunately, Ptolemy in *Almagest* IV,9 (Toomer 1998: 205) mentions that Hipparchus assumed that the diameter of the moon "goes approximately 650 times into its own orbit, and 2.5 times into [the diameter] of the earth's shadow, when it is at mean distance in the syzygies". Therefore, for Hipparchus, $\rho_\odot$ = 360/(650·2) and ρ_s = (360·2.5)/(650·2). If we assume these values and a lunar distance of 67⅓, the corresponding solar distance is 484.44 which could be rounded to 490. This is more than enough for showing that Hipparchus applied the lunar eclipse method. Swerdlow, however, adds some textual evidence: on the one hand, Pappus's text mentions "theorem 12," which is Pappus's way of referring to the lunar eclipse method; on the other, Ptolemy himself said that the method he proposes was used previously by Hipparchus (Almagest, V,14; Toomer 1998: 254).

According to Swerdlow's reconstruction, Hipparchus started from 490 as the solar distance and, applying the lunar eclipse method, obtained a lunar mean distance of 67⅓. Why did Hipparchus assume that the solar distance is 490 earth radii (*e.r.*)? Swerdlow noted that it is almost exactly the distance that corresponds to a parallax of 7 minutes. So, probably, Hipparchus considered this parallax the "least perceptible parallax," calculated the solar distance and, applying the eclipse method, found the lunar distance. Swerdlow's contribution was a huge step in the direction of understanding Hipparchus's procedure.

Among his concluding remarks, Swerdlow asserts that "there remains to be explained the first set of lunar distances, the set presumably derived from the solar eclipse. The second set, as we have seen, has nothing to do with a solar eclipse, but was derived, as Pappus states, 'from many considerations.'" As I will show later, it is not totally true that the second set has nothing to do with the solar eclipse, but Swerdlow's request will be fulfilled by Toomer, who five years later suggested a plausible explanation for obtaining the first set of values using the solar eclipse method.

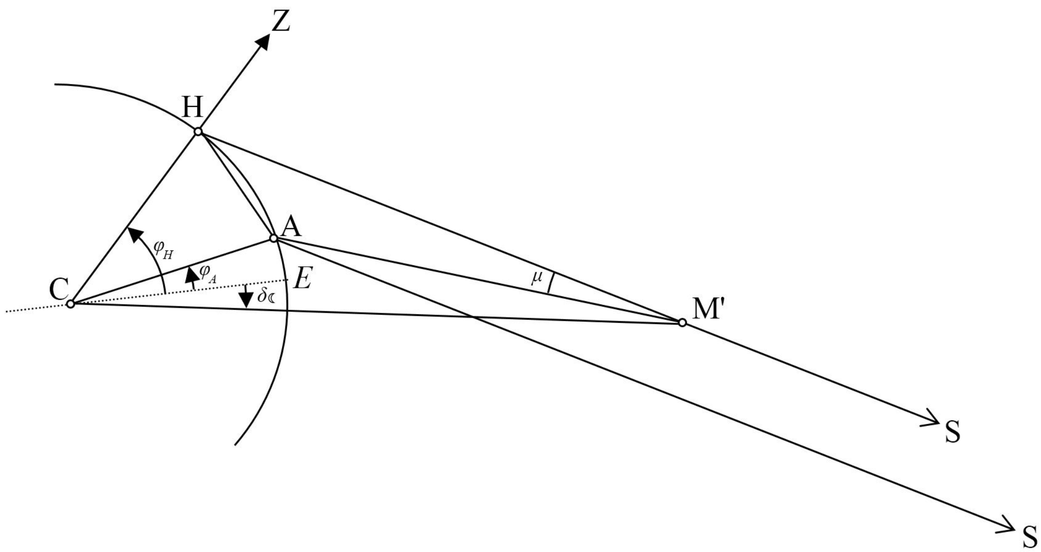


Figure 2. Hipparchus's method for calculating the Moon's distance using a solar eclipse. Similar to figure 2 of Toomer 1974: 132, the center of the earth at *C* and the center of the moon at *B*.

Toomer's reconstruction: the values of the first book.

Toomer (1974) assumed Swerdlow's contribution, but restated the demonstration in a way that he considered closer to what Hipparchus actually did. According to Toomer, when Hipparchus assumes the solar distance of 490 and calculates the lunar distance, he is looking at a maximum lunar distance. Actually, as I will show with more detail later, in the lunar eclipse method the distances are inversely proportional. If one assumes the least possible solar distance, one will find the greatest possible lunar distance. Therefore, the set of values of the second book (62,67⅓,72⅔) must be understood as the upper limit of the lunar distance.

Toomer also noted, however, that one can go further, for, in addition, the lunar eclipse method implies a lower limit for the lunar distance that corresponds to assuming the sun at an infinite distance. I will show the details later, but Toomer found that the lunar minimum distance is 59.12 *e.r.* He remarked that 59 *e.r.* is the value found by Ptolemy for the mean distance at syzygies. Toomer thinks that Ptolemy probably borrowed it from Hipparchus.

So, just as the procedure in book two must be understood as an attempt to find an upper limit for the lunar distance (assuming the sun is as close as possible), the procedure in book one could be understood as an attempt to find the lower limit for the lunar distance, assuming that the sun is as far as possible, i.e., at an infinite distance. For, he says, "in this case (i.e., if the sun is at infinite distance), the method of book 2 is not applicable". (Toomer 1974:131). So, Hipparchus proposed in book 1 a new method, based on the analysis of the solar eclipse that was total at the Hellespont but partial at Alexandria.

In Figure 2 the center of the earth is at *C*, Hellespont region at *H*, Alexandria at *A* and the dotted line *CE* represents the equator. Therefore, the angles *HCE* (φ_h) and *ACE* (φ_a) represent the latitude of Hellespont and Alexandria respectively. The moon is at *M*' and the sun, *S*, is at infinite distance, so that lines *HS* and *AS* are parallel. The moon and the sun are aligned from *H*, i.e., *H*, *M*' and *S* are in the same line. Angle *ECM*' ($\delta_☾$) is the declination of the moon. Angle *HM*'*A* (μ) represents the lunar parallax seen from *H* and from *A*. Because the sun is at an infinite distance and therefore its parallax is 0, the difference in the position of the moon with respect to the sun as seen from *H* and *A* is due entirely to the lunar parallax and, therefore, μ is ⅕ of the apparent size of the sun and moon, i.e. ⅕ of 360/650. Toomer argues convincingly that for Hipparchus the latitude of Alexandria should be around 31° (φ_a =31°) and the latitude of the Hellespont region around 41° (φ_h =41°).

Now, we want to obtain the distance of the moon, i.e., line *CM*'. It is easy to calculate line *AM*' and we know that, approximately, *CM*' is *AM*'+1. Applying the law of sines to the triangle *HAM*', we know that:

5 $$AM' = \frac{AH \cdot \sin M'HA}{\sin \mu}$$

Now, *AH* is the chord of angle *HCA*, which is the difference of the latitudes of Hellespont and Alexandria. The sides *CH* and *CA* are, of course, 1 *e.r.* each. Therefore angles *CHA* and *HAC* are equal to each other and:

6 $$CHA = HAC = 90 - \frac{(\varphi_\mathrm{h} - \varphi_\mathrm{a})}{2}$$

So, one can say that:

7 $$AH = \frac{\sin HCA}{\sin CHA} = 2 \cdot \sin\left(\frac{\varphi_H - \varphi_A}{2}\right)$$

Now, angle *M*'*HA* is equal to *ZHA* minus *ZHM*'. According to Toomer, because HMC is so small, one can assume that *ZHM*' is equal to *ZCM*'. One knows that

8 $$ZCM' = \varphi_\mathrm{h} - \delta_{☾}$$

And *ZHA* is equal to 180° minus *CHA*, that we already have in eq.(6). Therefore:

9 $$ZHA = 180 - CHA = 90 + \frac{(\varphi_\mathrm{h} - \varphi_\mathrm{a})}{2}$$

So, one can express *M*'*HA*

10 $$M'HA = ZHA - ZHM' = 90 - \frac{(\varphi_\mathrm{h} + \varphi_\mathrm{a})}{2} + \delta_{☾}$$

Finally, one can go back to eq.(5) and find *AM*' and therefore, *CM*'. *CM*' depends on the constants φ_a, φ_h and μ; the declination of the moon ($\delta_{☾}$) is the only variable which, in turn, depends on the solar declination and the lunar latitude.

Eclipse n.	*Date*	$\delta_{☉}$	$\beta_{☾}$	$\delta_{☾}$	*CM'*
1	-309 August 15	+16°	+⅓°	+16⅓°	85¾
2	-281 August 6	+18½°	+⅓°	+18⅚°	87
3	-216 February 11	-15⅓°	+⅓°	-15°	57⅔
4	-189 March 14	-4°	+1	-3°	71
5	-173 October 10	-5½°	+½°	-5°	69
6	-128 November 20	-19⅓°	+⅚°	-18½°	53⅓

Table 1. This table reproduces relevant data of tables 1 and 2 of Toomer 1974.

Toomer analyzed all solar eclipses between the foundation of Alexandria and Hipparchus's time that were total seen from close to the Hellespont region but a bit less than total at Alexandria and, calculating their solar declination and lunar latitude, obtained the lunar distance corresponding to each one. Table 1 reproduces relevant data of tables 1 and 2 of Toomer's paper. Among the six possible solar eclipses, he found that a lunar distance consistent with Pappus's values follows only from the eclipse of -189 March 14. Actually, he found for this eclipse a solar declination of -4° and a lunar latitude of 1°; so, $\delta_{☾} = -3$. This implies a lunar distance of 71.07 *e.r.*, almost exactly the value attributed to Hipparchus by Pappus for the lunar minimum distance. He also shows that during this eclipse the moon was close to its minimum distance[3]. Therefore, this must be the eclipse used by Hipparchus.[4]

So, Toomer's step was as important as Swerdlow's, helping us to understand the calculation that is behind the first set of values. He finishes his paper summarizing Hipparchus's procedure (Toomer 1974: 139-140):

> Starting from the fact that there is no observable solar parallax, in "On Sizes and Distances", book 1, he took the extreme situation, assuming that the solar parallax was zero, that is that the sun was (for practical purposes) infinitely distant. Then using the data from the eclipse of -189, March 14, he derived a *minimum* distance of the moon (71 earth-radii at least distance). However, he was well aware of the unreliability of his premises... In Book 2 he assumed that the solar parallax was the maximum possible, namely 7', and hence computed the sun's *minimum* distance and the corresponding maximum distance of the moon (using the method elucidated by Swerdlow), the latter being 67⅓ in the mean. He then showed that as the sun's distance increased, the moon's decreased towards a limit of 59 earth-radii, and was thus able to establish the moon's distance between quite close limits. This procedure, if I have reconstructed it correctly, is very remarkable... What is astonishing is the sophistication of approaching the problem by two quite different methods, and also the complete

3 According to Ptolemy's tables, the lunar anomaly was 228° during the solar eclipse. Toomer (1974: 136, n. 36) argued that, probably, Hipparchus's value was around 17° less, i.e., 211°, but he later (Toomer 1998: 224, n. 14) realized that this was wrong and that Hipparchus had a pretty accurate epoch of lunar anomaly. This means that at the moment of the eclipse the moon wasn't exactly at its minimum distance. And, therefore, that the 71 *e.r.* calculated doesn't correspond to the minimum distance, but to a distance between the minimum and the mean. But, because Hipparchus is looking for an upper limit –as I will show later–, it would keep himself on the safe side if he considers that the 71 *e.r.* corresponds to the minimum distance. With a lunar anomaly of 228°, the difference would be around two earth radii. For more details about the scheme and epoch used by Hipparchus for calculating lunar anomaly see Jones 1983, especially pp. 23-27.

4 Toomer's reconstruction assumed that the eclipse took place at the meridian which is not true for the eclipse of -189. See appendix for the consequences of this assumption.

> honesty with which Hipparchus reveals his discrepant results (his "maximum distance" in book 2 turns out to be smaller than his "minimum distance" in book 1).

In what follows, I will try to go one step forward in our comprehension of Hipparchus procedure.

Text analysis

In order to make a new step forward, it is very convenient to take a close look to Ptolemy's and Pappus's texts and see what can be inferred from them about Hipparchus's calculations. This is not an easy task, for, as we already mentioned, the texts are really obscure and probably not totally consistent with each other. Let me start with Ptolemy's. I present the text again, but now with some index letters and some Greek words between brackets for convenience:

> [0] Now Hipparchus used the sun as the main basis of his examination of this problem. For, since it follows from certain other characteristics of the sun and moon (which we shall discuss subsequently) that, given the distance to one of the luminaries, the distance to the other is also given, Hipparchus tries to demonstrate the moon's distance by guessing at the sun's. [A] First [τὸ μὲν πρῶτον] he supposes that the sun has the least perceptible parallax, in order to find its [αὐτοῦ] distance, [B] and then [μετὰ δὲ ταῦτα] he uses the solar eclipse which he adduces; [1] at one time [ποτὲ μὲν] he assumes that the sun has no perceptible parallax, [2] at another [ποτὲ δὲ] that it has an adequate/sufficient [ἱκανὸν] parallax. [C] As a result the ratio of the moon's distance came out different for him for each of the hypotheses he put forward; for it is altogether uncertain in the case of the sun, not only how great its parallax is, but even whether it has any parallax at all.

From [0] we know that Hipparchus used the distance of the sun for calculating the distance of the moon, in the exactly opposite direction taken by Ptolemy, who uses the lunar distance for calculating the solar distance. Even if it is not certain from this paragraph, it seems that Ptolemy is referring to the lunar eclipse method, which he will discuss subsequently, i.e., four sections later, in the same book of the *Almagest*.

In [A] the English *its* of *its distance* is ambiguous, but the Greek αὐτοῦ could only refer to the sun (masculine), for the moon is feminine. Therefore, [A] asserts that Hipparchus supposes the least perceptible parallax in order to find the *solar* distance. [B] comes undoubtedly after [A]. So, he first calculates the minimum solar distance [A] and then he uses the solar eclipse [B]. He uses the eclipse with two different assumptions [1] and [2]. In [1] he assumes that the sun has no perceptible parallax and in [2] an ἱκανὸν parallax. ἱκανὸν means sufficient, big enough, adequate, significant. Toomer (1988: 244), in his translation of the *Almagest*, translated it by *big enough* and adds "[to be perceptible]". In the paper (1974: 126), he translates it by *significant* (and adds the word in Greek). Swerdlow (1969:287) translates it by *sufficient* and adds "[i.e. sufficient to be perceptible]". I think that the only safe thing that we can conclude is that the parallax was significant, probably observable, but there is no reason to think that it is a limiting parallax, i.e., that Ptolemy is talking about the least perceptible parallax mentioned in [A].

Now, let us take a close look to Pappus's text (the text in italics is taken textually from Ptolemy's text):

> [1] *Now, Hipparchus made such an examination principally from the sun*, [and] not accurately. For since the moon in the syzygies and near the greatest distance appears equal to the sun, and since the size of the diameters of the sun and moon is given (*of which a study will be made bel-*

Ptolemy		*Pappus*	
[A]	First [τὸ μὲν πρῶτον] he supposes that the sun has the least perceptible parallax, in order to find its [αὐτοῦ] distance	[2.a]	In the first book "On the Sizes and Distances" it is assumed that the earth has the ratio of a point and center of the [sphere of the] sun
[B]	and then [μετὰ δὲ ταῦτα] he uses the solar eclipse which he adduces	[2.b]	And, first, [καί ποτε μὲν] by means of [διὰ] the eclipse adduced by him
[1]	at one time [ποτὲ μὲν] he assumes that the sun has no perceptible parallax	[2.b.1]	it is assumed that the sun shows the smallest parallax
[2]	at another [ποτὲ δὲ] that it has an adequate/sufficient [ἱκανὸν] parallax.	[2.b.2]	then [ποτὲ δὲ] a greater parallax [μεῖζον]
[C]	As a result the ratio of the moon's distance came out different for him for each	[2.c]	And thus there arose the different ratios of the distances of the moon

Table 2. Comparison between Ptolemy's text and the second part of Pappus's text.

low), it follows that if the distance of one of the two luminaries is given, the distance of the other is also given, as in Theorem 12, if the distance of the moon is given and the diameters of the sun and moon, the distance of the sun is given. Hipparchus *tries by conjecturing the parallax* and the distance of the sun *to demonstrate the distance of the moon*, [but] *with respect to the sun, not only the amount of this parallax, but also whether it shows any parallax at all is altogether doubtful*. For in this way Hipparchus was in doubt about the sun, not only about the amount of its parallax but also about whether it shows any parallax at all. [2] [a] In the first book "On the Sizes and Distances" it is assumed that the earth has the ratio of a point and center of the [sphere of the] sun. [2.b] And, first, [καί ποτε μὲν] by means of [διὰ] the eclipse adduced by him [2.b.1] it is assumed that the sun shows the smallest parallax, [2.b.2] then [ποτὲ δὲ] a greater parallax [μεῖζον]. [2.c] And thus there arose the different ratios of the distances of the moon. [3] For, in book 1 of "On Sizes and Distances" he takes the following observation: an eclipse of the sun, which in the regions round the Hellespont was an exact eclipse of the whole solar disc, such that no part of it was visible, but at Alexandria by Egypt approximately four-fifths of it was eclipsed. [3.a] By means of this he shows in Book 1 that, in units of which the radius of the earth is one, [τὸ μὲν] the least distance of the moon is 71, [τὸ δὲ] and the greatest 83. Hence [τὸ ἄρα] the mean is 77. [3.b] Having shown the foregoing, at the end of the book he says: «In this work we have carried our demonstrations up to this point. But do not suppose that the question of the moon's distance has been thoroughly examined yet. For there remains some matter of investigation in this subject too, by means of which the moon's distance will be shown to be less than what we have just computed». [3.c] Thus Hipparchus himself also admits that he cannot be altogether sure concerning the parallaxes. [3.d] Then, again, he himself in Book 2 of "On Sizes and Distances" shows from many considerations [ἐκ πολλῶν ἀποδείκνυσιν] that, in units of which the radius of the earth is one, [τὸ μὲν] the least distance of the moon is 62, [τὸ δὲ] the mean 67⅓, and [τὸ δὲ] the sun's distance 490. It is clear that [δῆλον δὲ ὅτι καὶ] the greatest distance of the moon will be 72⅔.

Hipparchus's text can be divided in three parts. The first one [1] is mostly a paraphrase of Ptolemy's text (mainly Ptolemy's text [0]). The second one [2] seems to enumerate the different parallaxes assumed in order to justify the fact that Hipparchus arrived up to different distances of the moon. The third one [3] is the most revealing, distinguishing what Hipparchus did in each book of the treatise and providing the different set of values obtained in each one.

Text [1] is not too clear, probably due to the desire of Pappus to paraphrase Ptolemy's text. But, at least, it is useful to confirm what we suspected from Ptolemy's text [0], i.e., that Hipparchus used the eclipse diagram, for Pappus refers explicitly to it as "Theorem 12". Text [2] is very similar to the rest of Ptolemy's texts, and the parallel among the parts is evident. I present them in table 2.

There are, nevertheless, significant differences. While Ptolemy says that Hipparchus supposed *the least perceptible* parallax for calculating the solar distance, Pappus asserts that, in the first book, Hipparchus assumed that *there is no parallax*. While the first part of the text is not exactly parallel, this doesn't mean that there is a contradiction. Ptolemy and Pappus could be talking of different parts of Hipparchus's work. It is possible that Hipparchus, somewhere calculated the smallest perceptible parallax in order to calculate the distance of the sun and, at another, assumed that the sun has no parallax. Then, both authors introduce the reference to the solar eclipse. And both highlight the two parallaxes assumed by Hipparchus. But it seems that they are not exactly the same. While Ptolemy refers to the first one as "no perceptible", Hipparchus says: "smallest". This could mean the same thing: no parallax at all. Something similar happens with the reference to the second parallax: while Ptolemy uses ἱκανὸν, Pappus says that it is greater (μεῖζον) than the previous one. There is no inconsistency here either. We know that a significant parallax is greater than the smallest. Both authors finished mentioning that these two (or three) different parallaxes are responsible for the different values of the lunar distance.

One last detail of this text should be highlighted: it looks as if that all that Pappus is saying in text [2] is contained just in the first book. This seems odd for at least two reasons. First, because text [3] seems to make explicit text [2], but in text [3] book one and two are openly mentioned. Second, because this would imply that already in the first book there were more than one set of lunar distance values. It is also possible that what Pappus pretends to situate in book one is just the assumption of no parallax of text [2.a].[5]

Text [3] is the clearest and most informative of the three. He says that in book one, Hipparchus uses a solar eclipse and he gives us details of the eclipse: it was total in the Hellespont but at Alexandria just four-fifths of it was eclipsed. [3.a] gives us a set of lunar distances. It seems that the first two were obtained by Hipparchus himself, while the third one was provided by Pappus. This is the natural way of interpreting the ἄρα (hence). So, while Hipparchus in book one obtained the minimum and the maximum distance of the moon, Pappus adds the mean one. Because no solar distance is mentioned, it is natural to suppose that in this calculation the sun had an infinite distance and, therefore, to link this calculation with the parallax referred by Ptolemy like no perceptible parallax and by Pappus like the smallest. Moreover, this is the first of the two mentioned, so the order would be also respected.

Text [3.b] quotes textually the end of the first book of Hipparchus in which he says that the question of the lunar distance is not finished as it has been dealt with in book one and that in book two he will find a smaller distance. In text [3.c], Pappus confirms what Ptolemy said: that Hipparchus admits that he is not sure concerning parallaxes.

Text [3.d] offers information about the calculation of book two. Unfortunately, Pappus is not explicit at all in his reference about the method of calculation (he only says that the values have been obtained "from many considerations"). In this case, three values seems to be obtained

5 It should be noted that, in the text that Hipparchus is paraphrasing, Ptolemy uses πρῶτον meaning "in the first place", so maybe there is a corruption in the text that goes from ὑπέθετο πρῶτον ἐν περὶ μεγεθῶν καὶ ἀποστημάτων to ὑπέθετο ἐν τῷ πρώτῳ περὶ μεγεθῶν καὶ ἀποστημάτων.

explicitly by Hipparchus himself and one added by Pappus. Hipparchus obtained the minimum and the mean distance of the moon, and a solar distance (490). Pappus adds that it is clear what would be the greater lunar distance. Because a solar distance is mentioned here, it is natural to associate this calculation with that of the second parallax, called ἱκανὸν by Ptolemy and μεῖζον by Pappus.

Two things about this text are still worth mentioning. First, that the solar distance seems to have been the result (and not an input value) as much as the other two lunar distances mentioned in the text. Second, that the "many considerations" used by Pappus to refer to the method of calculation does not exclude the use of the solar eclipse. Actually, if the parallel between the two parallaxes mentioned both by Ptolemy and Pappus, and the sets of values provided by Pappus are taken seriously, one has to recognize that both sets of values have been obtained using the solar eclipse, for both parallaxes have been used in relation with the solar eclipse. The "many considerations", nevertheless, could suggest that not only the solar eclipse was used, but something else. Of course, this "something else" could be anything, but a good candidate is the lunar eclipse method mentioned both by Ptolemy and Pappus and that until now had no room in our story.

From the analysis of both texts, therefore, what we know about Hipparchus´s method could be summarized thus: Hipparchus in *On the Sizes and Distances* tried to calculate the lunar distance by conjecturing the solar distance. He made at least two different calculations, based on two different assumptions and obtained two different sets of values. In the first book, he first calculated the solar distance that follows from the least perceptible parallax, probably for establishing a lower limit for the sun values: because it seems that the solar parallax is not perceptible, one could not use a solar distance smaller than that. After this, by means of the solar eclipse and assuming that there is no parallax, Hipparchus calculated the minimum and maximum lunar distance, obtaining 71 and 83, respectively. At the end of the book, he said that the research was not finished and that in the next book he would find that the lunar distance is actually smaller. In book two, he made a new calculation from many considerations that surely included again the solar eclipse but also almost certainly something else, probably the lunar eclipse method. In this calculation he used a significant or adequate parallax, greater than the previous one and obtained the minimum and mean lunar distances and the solar distance, 62, 67⅓ and 490 respectively.

Swerdlow's proposal is surely an important step for understanding the calculation of book two. He shows us that Hipparchus used the lunar eclipse method and, using as input value the solar distance of 490, he obtained the mean distance of 67⅓. Nevertheless, his account does not explain (a) the role that the solar eclipse surely played in the calculation, (b) the fact that the solar distance seems to be the result of the calculation as much as the minimum and mean lunar distance, and not an input value and, (3) that Hipparchus obtained not just the mean distance, but also the minimum one or, at least, for some reason, he considered important to make explicit the mean distance and not the maximum one.

Toomer's proposal represents also a very significant step forward in our comprehension of the method used by Hipparchus in book one. Nevertheless, he fails to explain (1) why Hipparchus makes explicit not only the minimum but also the maximum distance and I think this is due mainly to (2) his interpretation of the sets of lunar values of book one as a *minimum*. I will try to show that it is actually, an upper limit. This would solve (3) another oddity of his interpretation, i.e., that Hipparchus obtained in book one an upper limit smaller than the lower limit that he will find in book two. Toomer interprets this inconsistency as a sign of the laudable honesty of Hipparchus but at least it seems odd not just that Hipparchus would have published

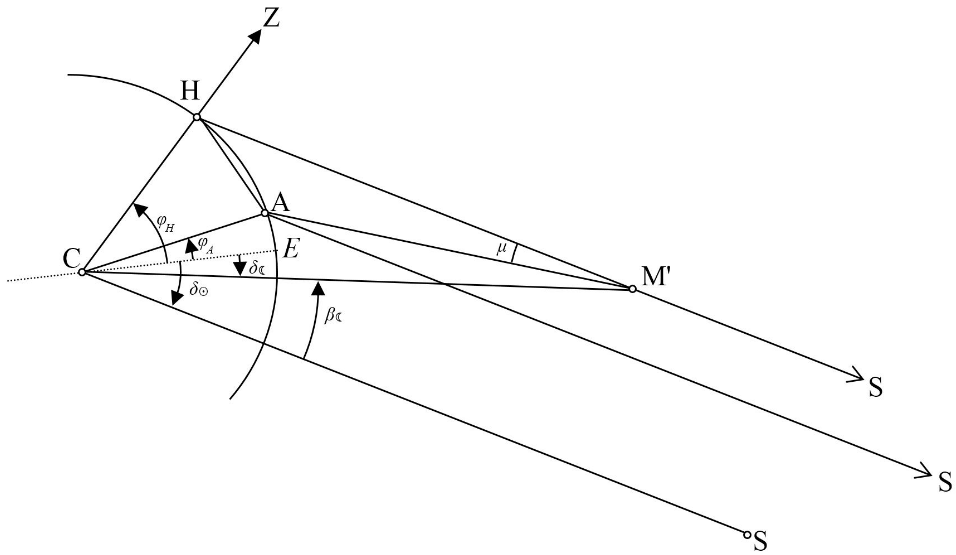


Figure 3. Hipparchus's method for calculating the Moon distance using a solar eclipse. Similar to Figure 2 but with the solar declination ($\delta_\odot$) and lunar latitude ($\beta_☾$) added.

inconsistent results in a work, but still more so that both Ptolemy and Pappus, who clearly are criticizing Hipparchus, do not mention this obvious inconsistency. As I will show, if we interpret the values of book one as an upper limit, this problem disappears. Finally (4) Toomer (1974:131) seems to justify the necessity of the solar eclipse method because, he says, the lunar eclipse method used in book two is not applicable to an infinite solar distance. But this is not correct, for, as he shows one paragraph above, the lunar eclipse method allows one to find the lower limit of the lunar distance (59.12 *e.r.*), when it is assumed that there is no solar parallax. Therefore, if it is possible—as it is—to obtain both a lower and upper limit for the lunar distance using just the lunar eclipse method and assuming an upper and lower limit for the solar distances, what sense could make for Hipparchus to introduce another method that, besides, produces a set of values inconsistent with the others?

In what follows I will try to make one step further in our comprehension of Hipparchus's calculation. In order to do so, I will start by showing some problems that the geometrical calculation followed by Hipparchus according to Toomer presents and I will offer an alternative approach. Fortunately, this new approach doesn't change Toomer's main achievements: the -189 solar eclipse identified by him is still the eclipse used by Hipparchus. But this new approach would show that the values obtained in book one must be understood as an upper limit and also would allow us to apply the solar eclipse method to finite solar distances. Finally, I will show that this solar eclipse method for finite solar distances could be applied in conjunction with the lunar eclipse method and produce suitable results. I will conclude presenting a new reconstruction of Hipparchus's calculations that, I hope, even if strongly inspired in Toomer's and Swerdlow's significant achievements solves many of the problems found in their reconstructions.

Revisiting Toomer's reconstruction of Hipparchus' procedure (1): The identification of ZHM' and ZCM'

In Figure 3 I have added to Fig. 2 the solar declination and the lunar latitude, which is implicit in Toomer's calculation. So, Angle *ECS* ($\delta_{\odot}$) is the solar declination and angle *SCM*' ($\beta_{☾}$) is the lunar latitude. According to Toomer's calculation, $\delta_{\odot} = -4°$ and $\beta_{☾} = 1°$. The difference between them ($\delta_{\odot} - \beta_{☾}$) is, according to Toomer, the lunar declination ($\delta_{☾} = -3°$).[6]

Let us start analyzing a simplification made by Toomer that could have significant consequences in the final result. As we already said, he assumes that *ZHM*' is equal to ZCM' because the angle HM'C –the difference between them– is so small. But looking at Figure 3, it is easy to see that angle HM'C is equal to $\beta_{☾}$. The simplification doesn't make sense, not only because a difference of 1° could be significant but mainly because, according to Toomer, Hipparchus knew the value. So, it is still true that

8 $$ZCM' = \varphi_h - \delta_{☾}$$

But now

11 $$ZHM' = \varphi_h - \delta_{☾} + \beta_{☾}$$

And $\beta_{☾} - \delta_{☾} +$ is $- \delta_{\odot}$. Consequently:

11.1 $$ZHM' = \varphi_h - \delta_{\odot}$$

While *ZCM*' depends on the lunar declination, *ZHM*' depends on the solar declination. If this correction is applied also to *M*'*HA*, one obtains:

10.1 $$M'HA = ZHA - ZHM' = 90 - \frac{(\varphi_h + \varphi_a)}{2} + \delta_{\odot}$$

With this new equation applied again to eq.(5), *AM*' is now: 69.06 *e.r.* and, therefore, *CM*' approximately 70.06, one earth radius short. This is not terribly bad. The difference is due to the fact that, while *ZHM*' in Toomer's reconstruction depends on $\delta_{☾}$ (-3°), in mine it depends on $\delta_{\odot}$ (-4°). Therefore, If one assumes that $\delta_{\odot} = -3°$, the previous result is restored. And, actually, this assumption is perfectly reasonable, for we know that the solar declination at the moment of the eclipse was between -3 and -4. So, while it is true that it was closer to -4, it is still reasonable to assume that Hipparchus, instead of rounding the value, simply truncated the fractional part, a well attested practice in ancient Greek mathematics. Also, we know that Hipparchus sometimes made mistakes in the determination of the position of the sun, in some cases reaching up to half a degree.[7] So, after all, the simplification introduced by Toomer is not catastrophic, we simply must be ready to modify slightly the value of $\delta_{\odot}$.

Revisiting Toomer's reconstruction of Hipparchus' procedure (2): The determination of $\beta_{☾}$

Nevertheless, eq.(10.1) implies that *CM*' (the lunar distance) does not depend on the lunar declination ($\delta_{☾}$), but only on the solar declination ($\delta_{\odot}$), which *a priori* seems odd. Is it possible for the

6 In strict sense, this is only true if the ecliptic is parallel to the horizon at the meridian. See appendix.

7 According to Ptolemy (Almagest IV, 11, Toomer 1998: 211-216) this is the reason why he found two different proportions for the *r*/*R* using two different sets of eclipses. See Toomer 1967.

lunar declination, or, equivalently, the lunar latitude to play no role at all in the determination of the lunar distance?

Yes, it is possible, because the moon parallax is already between two points, *A* and *H*: assuming that the sun is at infinite distance, the parallax is μ. This is enough for determining the lunar distance. The difference between $\delta_{\odot}$ and $\delta_{☾}$, i.e., $\beta_{☾}$ is another parallax. And, of course, only one is necessary for obtaining the lunar distance. Therefore, having $\beta_{☾}$ as a datum (obtained from the lunar theory) as in Toomer's approach, the problem is *over-determined*.

For example, it is enough to know that the eclipse was total at the Hellespont and $\beta_{☾}$ at the moment of the eclipse for calculating the lunar distance, ignoring the eclipse magnitude at Alexandria.

So, let us ignore what happens at A and work with the Hellespont and the center of the earth. Applying the law of sines to triangle *CHM*':

12 $$CM' = \frac{CH \cdot \sin CHM'}{\sin HM'C}$$

Now, *CH* is one earth radius, *HM*'*C* is equal to $\beta_{☾}$ and *CHM*' is equal to *ZHM*', known from eq.(15.1). Therefore:

12.1 $$CM' = \frac{\sin(\varphi_h - \delta_{\odot})}{\sin \beta_{☾}}$$

In this case, *CM*' depends exclusively on φ_h, $\beta_{☾}$ and $\delta_{\odot}$. The latitude of Alexandria (φ_a) and the lunar parallax μ play no role at all.

Assuming that $\beta_{☾} = 1°$, as Toomer did, then, *CM*' is 40.51 *e.r.*, around a half of the result previously obtained with the same set of data![8] This shows a serious inconsistency in Toomer's reconstruction for, depending on the geometrical path that one decides to follow, using the same set of data, one obtains different results.

Thus, it can be inferred from the geometrical configuration that, if one wants to use all the data transmitted by Pappus, it is not necessary to use the lunar theory for calculating $\beta_{☾}$. This is the way to block the over-determination. I guess Hipparchus did that, otherwise, part of the data is useless. $\beta_{☾}$ can be easily obtained from eq.(7.1).

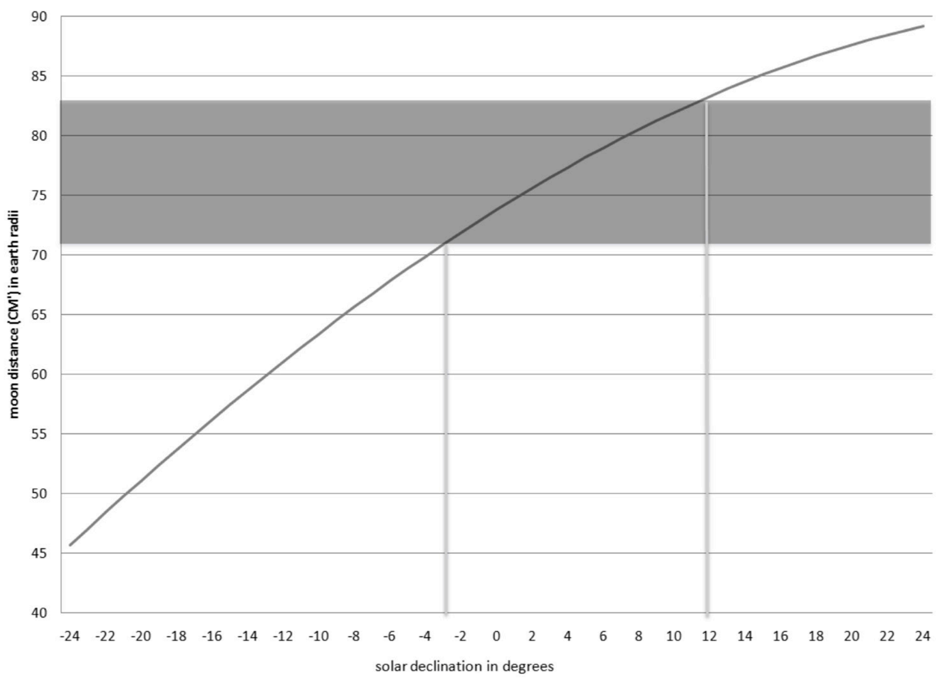
12.2 $$\sin \beta_{☾} = \frac{\sin(\varphi_h - \delta_{\odot})}{CM'}$$

Taking *CM*' = 71 *e.r.*, the corresponding $\beta_{☾}$ is, in this case, 0.57°. Hence, *CM*' is very sensitive to small changes in the $\beta_{☾}$, another reason for obtaining the adequate $\beta_{☾}$ from the figure instead of calculating it from the lunar theory.

Thus, one can calculate *CM*' without reference to $\beta_{☾}$, adding one earth radius to *AM*' (obtained in eq.1).[9] Therefore, eq.(7.2) offers a way to calculate $\beta_{☾}$.

8 If we decide to use Eq. 6 instead of 6.1, *CM*' would be 40,51 *e.r.* So, the difference is not due to the simplification analyzed previously.

9 It is possible to calculate *CM*' in a more precise way Applying the rule of cosine to triangle *CAM*', we obtain that $CM'^2 = AM'^2 + 1 - 2 \cdot AM' \cdot \cos(CAM')$. And $CAM' = 360° - (HAC + HAM')$. *HAC* is obtained in eq.(6) and $HAM' = 180 - (\mu + M'HA)$. Finally, *M*'*HA* is obtained in eq.(10.1). In our analysis we will use this more precise equation.



Graph 1. Lunar distance depending on the Sun declination. Only with solar declinations between -3° and +12° it is possible to obtain values for *CM*' between 71 and 83 e.r.

Revisiting Toomer's reconstruction of Hipparchus' procedure (3): The same eclipse

The fact that *CM*' can be determined only using the solar declination renders it even easier to check which one(s) of the eclipses that Toomer found could have been used by Hipparchus. In graph 1 you have *CM*' depending on $\delta_{\odot}$. The graph shows that It is only possible to obtain values for *CM*' between 71 and 83 *e.r.* from eclipses with solar declinations between -3° and +12°. Fortunately, there is only one eclipse that fits the description, the eclipse at -189 March 14. Also fortunate was that this is the same eclipse that Toomer had identified as candidate. One simply needs to assume that $\delta_{\odot}$ was -3° and that $\beta_{☾}$ (if it was used) was around 0.57°.

I will show in the next section that, using this new approach to the problem, it is easier to calculate the lunar distance assuming finite solar distances.

Lunar distance from a finite solar distance

Figure 4 does not suppose any longer that the sun is at an infinite distance. Therefore, line *HS* is not parallel to line *CS*. The moon (*M*) is at the intersection of *HS* and *CM*'. It is easy to see that if the solar distance diminishes, it also diminishes the lunar distance. *CM*' is, consequently, the maximum distance that the moon could reach, assuming that the sun is at an infinite distance. In this new situation, because the sun is not at an infinite distance, μ is not *HM*'*C* but *MAS*, i.e., the angular difference between the sun and moon observed from Alexandria.

Triangles *HM*'*M* and *SCM* are similar. Therefore,

13 $$\frac{CS}{CM} = \frac{HM'}{MM'}$$

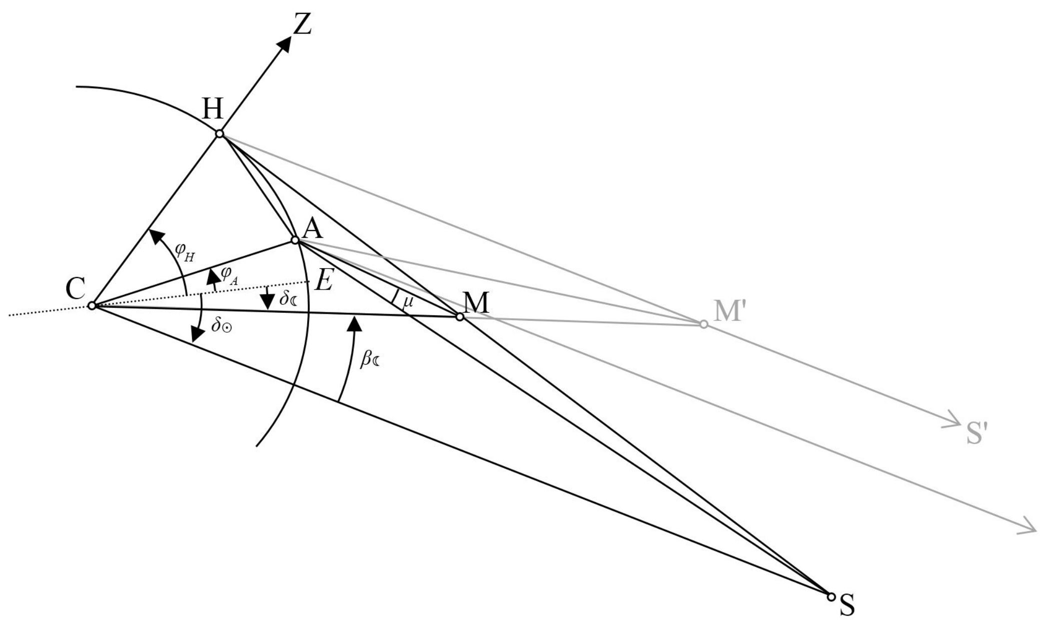


Figure 4. Hipparchus's method for calculating the lunar distance using a solar eclipse, but without assuming that the Sun is at an infinite distance.

And since *MM*' is equal to *CM*' – *CM*,

13.1 $$\frac{CS}{CM} = \frac{HM'}{CM' - CM}$$

which leads to:

13.2 $$CM = \frac{CM'}{\left(\frac{HM'}{CS} + 1\right)}$$

Applying the sine rule to triangle *CHM*' and noting that *CH* is one *e.r.* and *HM*'*C* is equal to $\beta_☾$:

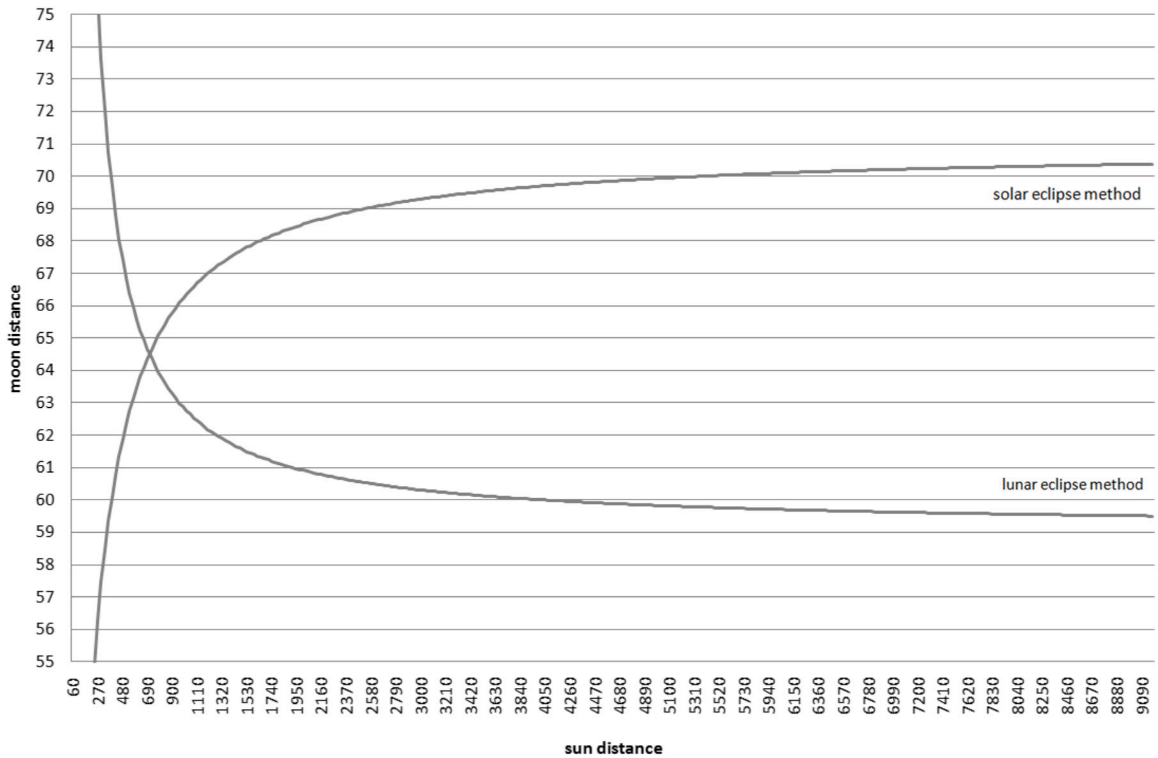
14 $$HM' = \frac{\sin HCM'}{\sin \beta_☾}$$

HCM' is equal to *ZCM*', already obtained at eq.(8) as $\varphi_h - \delta_☾$. Hence, the lunar distance can be obtained from eq.(13.2) knowing the solar distance.

Comparison with the lunar eclipse method

In an interesting sense, the solar eclipse method and the lunar eclipse method are complementary.

As I have already shown, if the lunar apparent radius is $\rho_☾$ and the earth shadow apparent radius is ρ_s, then the lunar eclipse diagram is expressed by the formula:



Graph 2. Lunar distance in function of the Sun distance for the solar eclipse and lunar eclipse method. The lunar eclipse method gives the mean distance of the moon, the solar eclipse method gives the minimum distance of the moon.

4 $$CM = \frac{1}{\sin\left(\rho_☾ + \rho_s - \sin^{-1}\frac{1}{CS}\right)}$$

A good enough approximation of this formula is:[10]

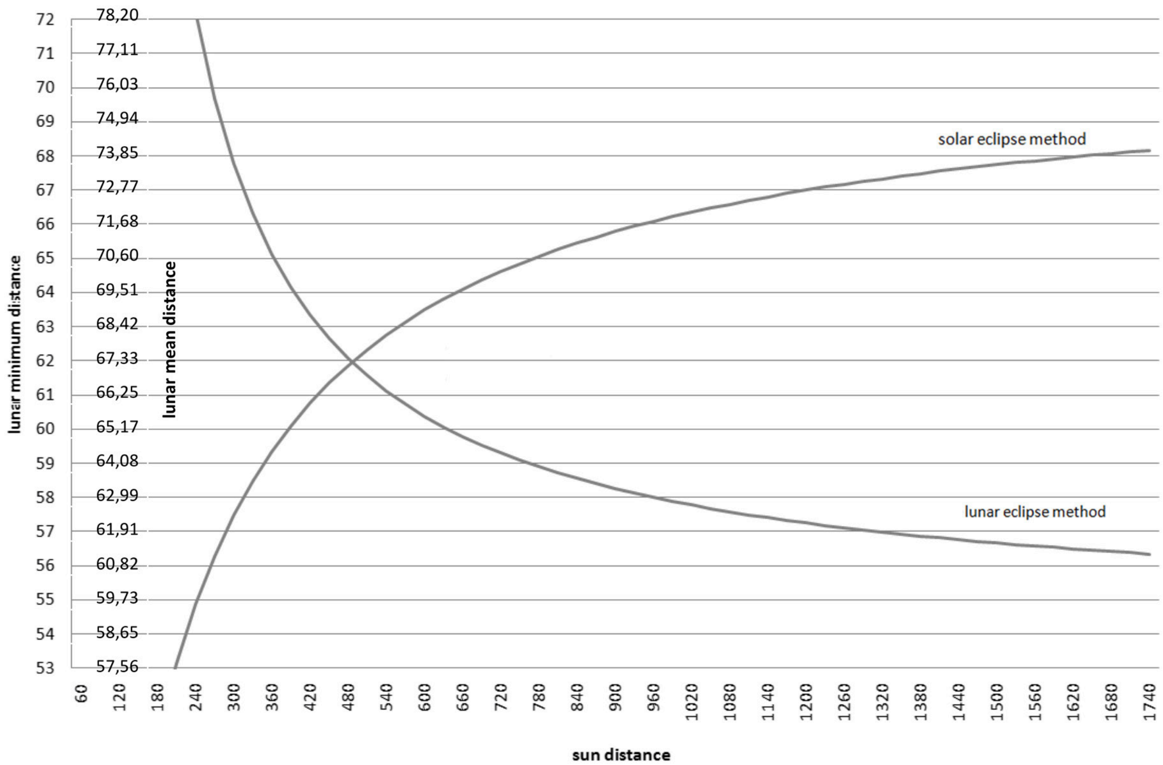
15 $$CM = \frac{1}{\sin(\rho_☾ + \rho_s) - \cos(\rho_☾ + \rho_s)\cdot\frac{1}{CS}}$$

Clearly, when *CS* grows, $\cos(\rho_☾ + \rho_s)\cdot 1/CS$ diminishes and, therefore, $\sin(\rho_☾ + \rho_s) - \cos(\rho_☾ + \rho_s)\cdot 1/CS$ also grows, making *CM* to be smaller. So, *CS* and *CM* are inversely proportional. When *CS* is infinite, *CM* would reach its minimum, being

16 $$CM_{min} = \frac{1}{\sin(\rho_☾ + \rho_s)} \approx 59.12$$

Toomer had already found this result. Exactly the opposite happens with the solar eclipse diagram. It is clear from eq.(13.2) that when *CS* grows, $HM'/CS + 1$ diminishes and, therefore, *CM*

10 I assume that $\sqrt{(1-1/CS^2)} = 1$.



Graph 3. Lunar distance in function of the Sun distance for the solar eclipse and lunar eclipse method. Both methods give the lunar mean distance. They meet at a lunar mean distance of 67⅓ e.r. and the solar distance of around 490 e.r.

grows. The maximum *CM* is obtained when *CS* is infinite. In this case *CM* = *CM*'. For Hipparchus's values (using $\delta_{\odot} = -3$), *CM*' is 70.91 ≈ 71 earth radii.

Therefore, the 71 *e.r.* found by Hipparchus using the solar eclipse method in book one should be understood as an *upper limit* and not a lower limit as Toomer suggested[11]: the lunar distance could not be greater than that.

Graph 2 clearly shows that, when working with both diagrams together, one obtains a maximum and minimum distance for the moon: the maximum around 71 *e.r.* and the minimum around 59 *e.r*. Also, there is one and just one distance of the moon and one and just one distance of the sun at which both diagrams meet. Graph 2 shows that the lunar distance is around 64.5 *e.r.* and the solar distance, 700 *e.r.*[12] But, while the lunar eclipse method gives the mean distance of the moon (because at mean distance sun and moon have the same apparent size), the solar eclipse method gives the minimum distance of the moon (for during the eclipse of -189 the moon was close to its minimum distance and the value corresponds to the minimum distance provided by Pappus). So, the 64.5/700 means nothing.

But let us plot both graphs at the minimum distance. In order to do so, from each result of the lunar eclipse method, which is originally expressed in mean distance, I will subtract the

11 Toomer (1974:135) asserts that it is a lower limit because the geometrical configuration assumes that the eclipse took place at noon. In any other moment of the day, *CM* would be greater. This is not correct, see appendix. But, nevertheless, with respect to the distance of the sun (which is the only relevant respect in this situation) it is an upper limit.

12 These values are curiously close to the distances that Ptolemy offers in the *Canobic Inscription*. (See Jones 2005). But I think that it is no more than a coincidence.

corresponding proportion for converting it to the minimum distance. If *m* is the original result, I will plot ($m - m \cdot r/R$), taking r/R = 247.5/3122.5. The new graph looks like Graph 3. The coincidence of both solar and lunar eclipse diagrams now happens when the lunar minimum distance is 62, consequently the mean distance is $67^1/_3$ and the solar distance is around 490. These three values are the results that Pappus says that Hipparchus obtained in his second book!

Therefore, Swerdlow's very smart discovery is just part of the story. It is true that, using the lunar eclipse method and starting with 490 *e.r.* for CS, one finds $67^1/_3$ *e.r.* for the lunar (mean) distance. But it is also true that, using the solar eclipse method and starting with 490 *e.r.* for *CS*, one finds 62 *e.r.* for the lunar (minimum) distance. Either this is an incredible cosmic coincidence or Hipparchus chose this set of values because they fit with both, the lunar and solar eclipse methods at the same time.

Hipparchus had two equations (lunar and solar eclipse methods) with two variables (*CS* and *CM*). Instead of conjecturing one of the values, he *solved* the system of equations. So, while in book one he established the upper and lower limit of each equation, in book two he solved the system of equations. I think that this reconstruction offers a much more coherent procedure.

The analytical solution consists in equating eq.(13.2) for the solar eclipse diagram and eq.(15) for the lunar eclipse diagram. Nevertheless, while eq.(13.2) gives the minimum distance of the moon, eq.(15) gives the mean one. So, in order to obtain the minimum distance from eq.(15), one has to multiply it by 1/ (1-r/R). Therefore:

15.1 $$CM_{min} = \frac{\left(1 - \frac{r}{R}\right)}{\sin(\rho_☾ + \rho_s) - \cos(\rho_☾ + \rho_s) \cdot \frac{1}{CS}}$$

Now, from eq.(15.1) and eq.(13.2):

17 $$\frac{CM'}{\left(\frac{HM'}{CS} + 1\right)} = \frac{\left(1 - \frac{r}{R}\right)}{\sin(\rho_☾ + \rho_s) - \cos(\rho_☾ + \rho_s) \cdot \frac{1}{CS}}$$

And for *CS*:

18 $$CS = \frac{HM' \cdot \left(1 - \frac{r}{R}\right) + \cos(\rho_☾ + \rho_s) \cdot CM'}{CM' \cdot \sin(\rho_☾ + \rho_s) - 1 + \frac{r}{R}} = 486.16 \approx 490$$

And, applying this solar value in eq. (13.2), *CM* is 61.97 for the minimum distance of the moon and in eq.(15), *CM* is 67.3 for the mean distance of the moon.

Of course, it is not necessary for Hipparchus to solve the system of equations, it would not be hard to find the result by trial and error, knowing the limits for *CM*.

Conclusion

Let me finish summarizing what I think was Hipparchus's procedure and how it solves the problems found in both Toomer's and Swerdlow's proposals. Hipparchus procedure consists in establishing first an upper and lower limit for the lunar distance assuming that the sun has no

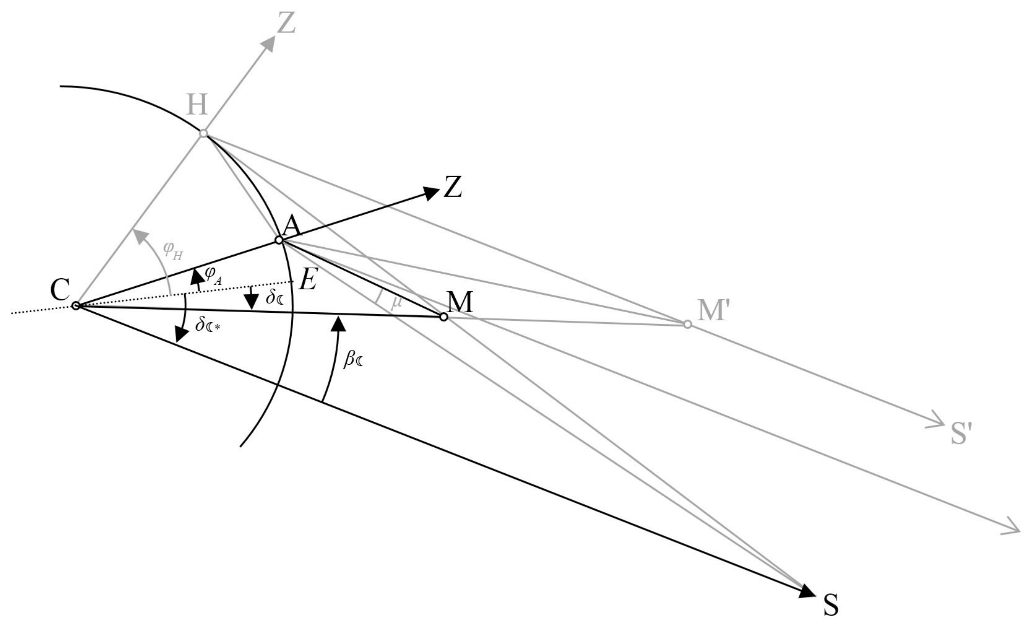


Figure 5. Hipparchus's method for calculating the Moondistance using a solar eclipse, (without assuming that the Sun is at aninfinite distance) used by Ptolemy for calculating the lunar parallax.

parallax: he first applied the solar eclipse method to the eclipse identified by Toomer and found that the upper limit for the lunar distance is 71 *e.r.* But this is the upper limit of the minimum distance (for, as we said, at the moment of the eclipse the moon was close to its minimum distance). Therefore, Hipparchus, using his ratio r/R naturally calculates also the maximum distance, 83 *e.r.*, so that he can offer the upper limit in an absolute sense: the moon could not be farther than 83 *e.r*. This explains why Hipparchus explicitly mentioned in book one the minimum (71) and the maximum (83), but not the mean one. The minimum because it happens to be the result of the calculation, and the maximum because he is suggesting an upper limit. I agree with Toomer than, even if we do not have textual evidence, we can suppose that Hipparchus probably calculated later the lower limit of the lunar distance that follows again from assuming that the sun is at infinite distance but now from the lunar eclipse method. He found 59.12 *e.r.* and he probably offered also the corresponding minimum distance (54.4 *e.r.*). I would like to suggest that both limit calculations have been performed in book one, while he reserved book two for the solution of the system of equations. Pappus' quotation at the end of book one, however, suggests that Hipparchus still didn't calculate the lower limit for he says that the value that he will find in book two is smaller than and not between the values found in book one So, probably book one is entirely devoted to the solar eclipse method and how to find the upper limit. Then, in book two he introduces the lunar eclipse method and, first, he calculated the lower limit and then he solved the system of equations finding at the same time that the only set of values is 62 for the minimum distance in the solar eclipse method, 67⅓ for the mean distance in the lunar eclipse method and 490 for the solar distance that follows, obviously, from both methods. These, again, are the three values that Hipparchus mentioned as the final result in book two. I think, therefore, that this last set must not be understood as a limiting case, but as the final result that is between the upper (83) and lower (54.4) limits. Needless to say that Hipparchu's development for solving the system of equations should have been as complex as to make Pappus prefer to say simply "from many considerations" than to try to explain it in a few words.

This account explains at the same time all the things that were left unaccounted for in Swerdlow's account: the role that the solar eclipse method played in the calculation of book two, the fact that the solar distance (490) is not an input value but a result obtained together with the minimum and mean lunar distances, and the reason why Hipparchus mentions both the minimum and the mean distance (but not the maximum) as the results of book two. It also explains why in book one, Hipparchus mentioned the minimum (71) and the maximum (83), but not the mean distance and it shows that this set of values must be interpreted as an upper limit and, therefore, it solves the inconsistency in Hipparchus's sets of values that Toomer's account implied. Finally, it explains the convenience of using both methods. I have already mentioned that, if one wants to find and upper and lower limit, it would be enough to apply the lunar eclipse method to a maximum and minimum solar distance. There is no need of the solar eclipse method. But if one wants to find a particular set of values, both methods have to be combined, rendering also necessary the use of the solar eclipse method.

The solar eclipse method in Ptolemy.

As far as we know, Ptolemy did not use the solar eclipse method at all but calculated the lunar distance measuring its parallax and then applied this value to the lunar eclipse method for finding the solar distance.

There are obvious reasons why Ptolemy could decide not to use the solar eclipse method. First of all, he could have realized that the input values are not too trustworthy: mainly exactly at what latitude of the Hellespont region the eclipse was total and what exactly was the eclipse magnitude at Alexandria. But, I think he discarded the solar eclipse method for a stronger and simpler reason: as I have already shown, if one can calculate the lunar latitude, it is an over-determined method. Ptolemy was able to calculate the lunar latitude, therefore, he did not need any datum from the Hellespont, it was enough with just one angle measured from Alexandria.

Figure 5 represents again the solar eclipse method but leaving in black just the part usable by Ptolemy. In his method, Ptolemy measured angle *ZAM* (i.e., the angular distance between the moon and the zenith at Alexandria) obtaining 50;55° and calculated angle *ZCM* (=49;48°) knowing the latitude of Alexandria (φ_A = 30;58°), the lunar latitude ($\beta_☾$= 4;59°) and the declination of the ecliptic at the moon longitude ($\delta^*_\odot$ = -23;49°). Knowing both angles it is easy to calculate *CMA* for it is the difference of both angles (1;7°). The problem is solved, one can easy calculate *CM*. For example, applying the rule of sine to the triangle *CAM*, knowing that *CAM* = 180 – *ZAM* and that, of course, *CA* = 1 *e.r.*. Then:

19 $$CM = \frac{\sin(180 - ZAM)}{\sin CMA} = 39;49\ er$$

Ptolemy obtains 39;45 *er*.

So, in a sense, the method used by Ptolemy for calculating the lunar parallax could be understood as a simplification of the solar eclipse method, once one realizes that the method is over-determined. So, even if Ptolemy didn't use the solar eclipse method, he could have been inspired by it for calculating the lunar distance.

There is another fact that could indicate that Ptolemy had in mind the solar eclipse method when he measured the lunar distance. The observation for measuring angle *ZAM* was made by Ptolemy himself in Alexandria 50 minutes after noon of 135 October 1st. It happens that the interval between the solar eclipse used by Hipparchus (-189 March 14) and Ptolemy's observation is 118.542 days which is exactly 18 Saros cycles (or 6 Exeligmos cycles) plus 6 days. Ptolemy's observation is close to a quarter moon, 6 days after the new moon at which, according to the Exeligmos period, a solar eclipse with similar characteristics of that observed by Hipparchus should have taken place. So allow me to suggest a hypothesis that I know that is very speculative and, at any event, impossible to check, but so attractive that I cannot resist offering it. The previous full moon of Ptolemy's observation, i.e., 135 September 25, was a spectacular opportunity (actually, the only one during the active life of Ptolemy) to check Hipparchus's premises in order to use the solar eclipse method. Because he had not yet calculated the lunar distance and parallax, he could not know whether the solar eclipse would be visible at Alexandria before observing it. But the eclipse was not visible at all at Alexandria, because it went too south. Therefore, Ptolemy decided to abandon this method and 6 days later, observed the moon for calculating its distance.

Appendix: assumptions

In all our calculations I simplified the geometrical situation assuming, like Toomer, two suppositions: 1) that the eclipse took place at noon and 2) that at the moment of the eclipse, the ecliptic was perpendicular to the vertical plane. The first assumption allows us to add and subtract declinations and geographical latitudes, as well as to identify the sun altitude with the geographical latitude of the place minus the solar declination. The second assumption, assuming that the Sun and moon are in the same vertical plane, allows as to obtain the angle μ just as the difference between the eclipse magnitudes from these two places (and the sun's apparent size).

Both suppositions are assumed by Ptolemy in his calculation of the lunar distance but, in order to minimize the effect, he explicitly chooses an observation in which the moon is close to these conditions. Of course, solar eclipses are really infrequent, and it would be still rarer for records to exist of a solar eclipse seen from two different places on the same meridian, therefore, Hipparchus could not have followed Ptolemy's strategy: he would have had to simply assume these conditions even if the difference compared to the real eclipse was not truly negligible, or he could try to work out the more complicated geometry assuming different planes. We suppose with Toomer that this would have been beyond Hipparchus's ability. Therefore, the aim of this appendix is just to complete the geometrical analysis.

In this appendix I will analyze the effect that making these two assumptions produces in the value of the distance of the moon. In the first place I will obtain a formula for calculating the lunar distance using as input: the magnitude of the eclipse from two different places, the zenith distance of the Sun, and the angles of the ecliptic with the horizon at H and A (these latter angles can to a very good approximation be found using only the time and the longitude of the Sun). The key point is to obtain a formula in which the lunar theory plays no role, as in eq.(5) if, as I suggested, angle M'HA is obtained from eq.(10.1), i.e., using just the geographical latitudes and the declination of the sun. Then, I will show the equivalence between this new formula and eq.(5), if we assume the two suppositions. Finally, we will apply this formula to analyze the effect that our assumptions produce on the distance of the sun.

1) The formula

For a solar eclipse the magnitude as seen by an observer at a site with geographical latitude φ is

20 $$m = \frac{r_S + r_M - \gamma}{2r_S}$$

or

21 $$\gamma = r_S + r_M - 2r_S m$$

where γ is the distance between the center of the Sun and the center of the apparent (topocentric) Moon as seen by the observer at the moment of maximum obscuration. Note that γ, being a distance, is always positive.

On the other hand, to a very good approximation,

22 $$\beta' = \beta + \pi_\beta \approx \gamma \cdot \mathrm{sgn}(\beta') = \gamma'$$

where β and β' are the true and apparent latitudes of the Moon and sgn(β') is the sign (+ if the obscuration is north of the ecliptic, – if it is south) of β', so γ', unlike γ, is signed. The lunar parallax in altitude π_h is related to π_β by

23 $$\pi_h = -\frac{\pi_\beta}{\sin\psi} = \frac{\beta - \gamma'}{\sin\psi}$$

where ψ is the angle of the ecliptic to the vertical direction at the apparent Moon. This angle is tabulated in Almagest II 13. Since the various parallaxes and both β and β' are small at an eclipse, we have to a very good approximation

24 $$\sin\pi_M = \frac{\sin\pi_h}{\sin\zeta'_M}$$

Then the geocentric distance to the Moon is

25 $$\begin{aligned} D_M &= \frac{1}{\sin\pi_M} \\ &= \frac{\sin\zeta'_M}{\sin\pi_h} \\ &= -\frac{\sin\zeta'_M \sin\psi}{\sin\pi_\beta} \\ &\approx \frac{\sin\zeta'_M \sin\psi}{\sin(\beta-\gamma')} \\ &\approx \frac{\sin\zeta'_M \sin\psi}{\beta-\gamma'} \end{aligned}$$

Now in this expression β is a geocentric value while ζ'_M, γ, and γ' are topocentric (dependent on the latitude of the observer). Thus we may write for the two locations Hellespont (*H*) and Alexandria (*A*),

26 $$(\beta - \gamma'_H)D_M = \sin\zeta'^H_M \sin\psi_H \approx \sin\zeta^H_S \sin\psi_H$$

27 $$(\beta - \gamma'_A)D_M = \sin\zeta'^H_A \sin\psi_A \approx \sin\zeta^A_S \sin\psi_A$$

where we can to a very good approximation write $\zeta'_M = \zeta_S$ since we are at an eclipse. Then subtracting we can eliminate β and get

28 $$D_M = \frac{(\sin\zeta_S^H \cdot \sin\psi_H - \sin\zeta_S^A \cdot \sin\psi_A)}{(\gamma'_A - \gamma'_H)}$$

29 $$D_M = \frac{(\sin\zeta_S^H \cdot \sin\psi_H - \sin\zeta_S^A \cdot \sin\psi_A)}{(\gamma'_A - \gamma'_H)}$$

For completeness, to calculate ψ first calculate the longitude Λ of the ecliptic rising at the moment of the eclipse using

30 $$\tan\Lambda = \frac{\cos\theta}{-\sin\varepsilon\tan\varphi - \cos\varepsilon\sin\theta}$$

and then

31 $$\cos\psi = \frac{-1}{\tan\zeta_S \tan(\Lambda - \lambda_S)}$$

2) the reduction to the other formula

I will show that when we assume that $\psi = 1$, and that the eclipse take place at meridian, both equations eq.(5) and eq.(20) are equal.

The equation for obtaining AM' is:

5 $$AM' = \frac{AH \cdot \sin M'HA}{\sin\mu}$$

We will first compare both denominators and then both numerators. Let me start with the denominator. At the meridian we know that:

32 $$(\gamma'_A - \gamma'_H) = \mu$$

Actually,

33 $$\gamma'_A = r_s + r_m - 2r_s m_A$$

and we know that $r_s = r_m = 360/(650{\cdot}2) = 0.2769$ and the magnitude m_A at A was 4/5. So,

34 $$\gamma'_A = 2r_s - 2r_s \cdot 0.8 = 0.4\, r_s \approx\ 0.11$$

We also know that γ' at H is 0 because m_H at H was 1. Therefore,

35 $$(\gamma'_A - \gamma'_H) = \gamma'_A =\ \mu$$

So, the difference between the denominator of both equations is just the difference between $\sin(\mu)$ and μ, which is really small, being μ also very small.

Now, let me compare the numerators. Because both ψ are 1, we have:

36 $$(\sin\zeta_S^H \cdot \sin\psi_H - \sin\zeta_S^A \cdot \sin\psi_A) = (\sin\zeta_S^H - \sin\zeta_S^A)$$

Now, according to figure 3:

37 $$\zeta_S^H = ZHM' = \varphi_H - \delta_S$$

38 $$\zeta_S^A = ZAS = \varphi_A - \delta_S$$

Therefore,

39 $$(\sin\zeta_S^H - \sin\zeta_S^A) = (\sin(\varphi_H - \delta_S) - \sin(\varphi_A - \delta_S))$$

Applying some trigonometric identities, we arrived at the following equation:

40 $$(\sin(\varphi_H - \delta_S) - \sin(\varphi_A - \delta_S)) = 2 \cdot \sin\left(\frac{\varphi_H - \varphi_A}{2}\right) \cdot \cos\left(\frac{\varphi_H + \varphi_A}{2} - \delta_S\right)$$

Now, from eq.(7), we know that

41 $$2 \cdot \sin\left(\frac{\varphi_H - \varphi_A}{2}\right) = AH$$

and, from eq.(10.1), we know that:

42 $$\cos\left(\frac{\varphi_H + \varphi_A}{2} - \delta_S\right) = \sin\left(90 - \frac{\varphi_H + \varphi_A}{2} + \delta_S\right) = \sin M'HA$$

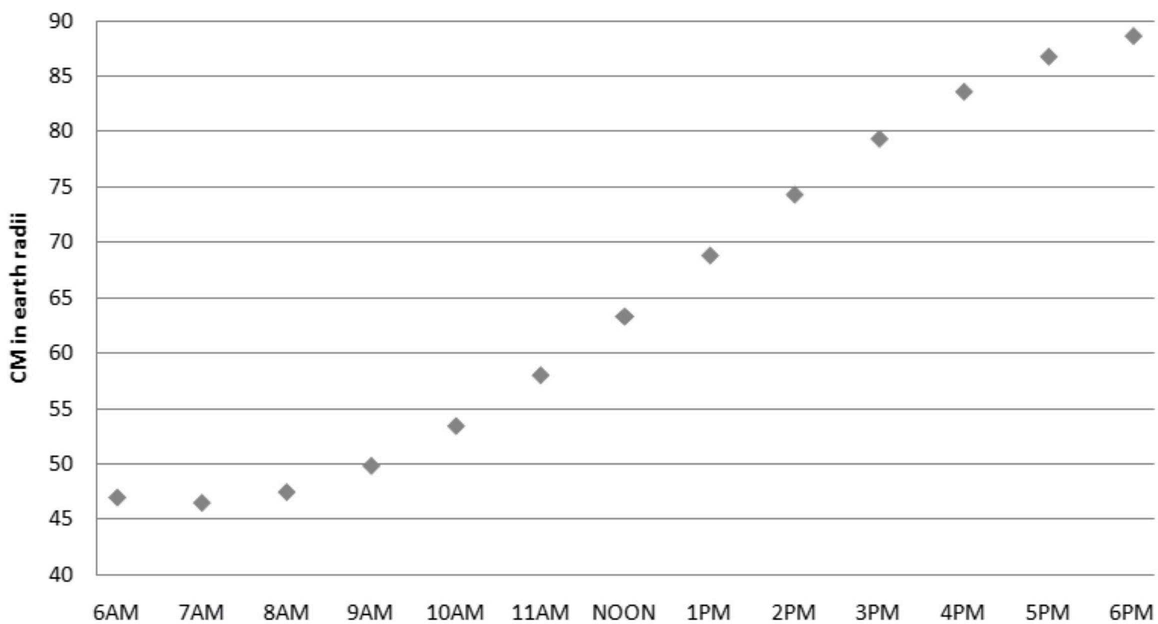
Therefore,

43 $$(\sin\zeta_S^H - \sin\zeta_S^A) = AH \cdot \sin M'HA$$

We have shown, therefore, that if we assume that $\psi = 1$ and the eclipse took place at noon, then $D_M = AM'$, leaving aside the small difference between $\sin(\mu)$ and μ. So, curiously, D_M gives us the distance from Alexandria, not from the center of the earth. This is due to the fact that for obtaining D_M, we used the approximation $\zeta_{mp} = \zeta_s$. In that case D_M from *A* and D_M from *C* are equal. So, if we add 1 earth radius to D_M, we will have exactly our *CM*' (assuming that *CM*' = *AM*'+1).

3) the effect on CM

In the next graph I will show the variation of CM' assuming that the eclipse took place at different times of the day of the eclipse. Therefore, using the new equation, I will keep constant the magnitudes of the eclipse from A and H (and use the Hipparchian value of the apparent size of the sun and moon), but I will take for ζ_s (from H and A) and for ψ (for H and A), the value that corresponds to this time of the day. In this way we can simulate the value for the lunar distance



Graph 4. Variation of *CM*' assuming that the eclipse took place at different times of the day of the eclipse.

that Hipparchus what would have obtained if he had assumed that the eclipse took place at different times.

The value at noon is not 71 because we are not assuming that ψ is 0 and, also, because of the small differences between the real values and the values we suppose Hipparchus assumed for the declination of the Sun and the geographical latitudes of Alexandria and the Hellespont. Nevertheless, the graph is useful for noting two things: first that, again, because the eclipse took place at morning, the noon value could be understood as an upper limit, and not a lower limit, as Toomer (1974:135) proposed; second, that the error due to the assumptions assumed is really significant, on the order of about 15 earth radii (this is the difference between 9 AM and noon).

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Geminos and Babylonian astronomy

J. M. Steele

Introduction

Geminos' *Introduction to the Phenomena* is one of several introductions to astronomy written by Greek and Latin authors during the last couple of centuries BC and the first few centuries AD.[1] Geminos' work is unusual, however, in including some fairly detailed—and accurate—technical information about Babylonian astronomy, some of which is explicitly attributed to the "Chaldeans." Indeed, before the rediscovery of cuneiform sources in the nineteenth century, Geminos provided the most detailed information on Babylonian astronomy available, aside from the reports of several eclipse and planetary observations quoted by Ptolemy in the *Almagest*. Early-modern histories of astronomy, those that did not simply quote fantastical accounts of pre-Greek astronomy based upon the Bible and Josephus, relied heavily upon Geminos for their discussion of Babylonian (or "Chaldean") astronomy.[2] What can be learnt of Babylonian astronomy from Geminos is, of course, extremely limited and restricted to those topics which have a place in an introduction to astronomy *as this discipline was understood in the Greek world*. Thus, aspects of Babylonian astronomy which relate to the celestial sphere (e.g. the zodiac and the rising times of the ecliptic), the luni-solar calendar (e.g. intercalation and the 19-year ("Metonic") cycle), and lunar motion, are included, but Geminos tells us nothing about Babylonian planetary theory (the planets are only touched upon briefly by Geminos), predictive astronomy that uses planetary and lunar periods, observational astronomy, or the problem of lunar visibility, which formed major parts of Babylonian astronomical practice.

In this article I address two questions relating to Geminos' discussion of Babylonian astronomy. First, what material in Geminos' *Introduction to the Phenomena* has a Babylonian origin? This question has already been addressed by several authors, in particular Neugebauer,[3] Jones,[4] and Evans and Berggren,[5] and I add only a few comments to their findings. The second question is more interesting and has not been considered in detail before: where did Geminos obtain his knowledge of Babylonian astronomy? Did Geminos himself have experience of Babylonian astronomy or did he learn about it from earlier Greek authors? Did Geminos know that all of the things we can identify as Babylonian were Babylonian or had some of this material been incorporated into the general Greek astronomical tradition and its Babylonian origin been forgotten?

1 My study of Geminos is based upon the translation by Evans and Berggren 2006. I have also relied extensively upon their introduction and commentary.

2 See in particular Costard 1748 and Montucla 1758. On eighteenth century histories of astronomy and their accounts of Babylonian astronomy, see Steele 2012, 45–57.

3 Neugebauer 1975, 2.581–587.

4 Jones 1983, 23-24.

5 Evans and Berggren 2006, 15.

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The Zodiac

Geminos begins the *Introduction to the Phenomena* by discussing the signs of the zodiac. In I.1–8, Geminos explains that the signs of the zodiac differ from the zodiacal constellations in that the signs are a division of the zodiacal band into twelve equal 30 degree parts. This concept of the uniform zodiac was invented in Babylonia sometime around the end of the fifth century BC,[6] and was already widely used in Greek astronomy by the time Geminos was writing. Unsurprisingly, therefore, Geminos does not attribute the zodiac to the Babylonians, but he does implicitly acknowledge that the Babylonians knew about it at I.9 where he says that the "Chaldeans" placed the solstices and equinoxes at 8 degrees within the signs of Aries, Cancer, Libra and Capricorn in contrast to Greek astronomers who place them at the beginning of these signs. The placement of the solstices and equinoxes at 8 degrees within their zodiacal signs is in agreement with the Babylonian System B lunar theory and is widely attested in Greek and Latin sources from the first century BC onwards.[7]

Geminos returns to the zodiac in V.51–53 where he explains that the zodiac is an oblique circle composed of three lines, two of which define the edges of the zodiacal band and the third of which is called "the circle through the middle of the signs."[8] He continues by stating that the width of the zodiacal band is 12 degrees. This description of the zodiac as a band 12 degrees in width parallels the Babylonian conception of the zodiac as a band through which the moon travels which is 12 degrees in width. Four cuneiform tablets contain copies of a text which describes the 12 degree wide path of the moon relative to the Normal Stars (a group of reference stars distributed unevenly around the zodiacal band),[9] and a procedure text states that "the width of the path of the moon is 12 (degrees)."[10] In cuneiform texts, the middle of this band is named "the ribbon of the middle" (DUR MÚRUB),[11] a close parallel to Geminos' "circle through the middle of the signs."

Astrology

Chapter 2 of the *Introduction to the Phenomena* discusses astrological aspects of the signs of the zodiac. Geminos provides a short explanation of four geometrical arrangements of zodiacal signs which have astrological significance: opposition, trine, quartile and syzygy. Geminos' discussion of astrological aspect is predicated on the idea of the signs of the zodiac being arranged in a circle. First he describes the concept of opposition, which refers to signs that are on opposite ends of a diameter of the circle, or, in other words, signs that are six signs apart and that rise and set simultaneously. Geminos states that "signs in opposition are considered by the Chaldeans in connection with sympathies in nativities" (2.5).[12] Geminos next describes trine aspect, where signs of the zodiac form equilateral triangles and are separated by 4 signs, and he links each

6 Britton 2010, Steele 2007.

7 Neugebauer 1975, 2.594–598.

8 Evans and Berggren 2006, 157.

9 Steele 2007, 304–308.

10 BM 32167 Obv. I 20; edited by Ossendrijver 2012, text 53. See also BM 41004 Obv. 15 (Neugebauer and Sachs 1967, Text E) which contains a parallel statement but using cubits rather than degrees (1 cubit = 2 degrees).

11 Steele 2007, 315.

12 Evans and Berggren 2006, 125.

triplicity with wind directions and says that they too imply sympathies when interpreting nativities. He continues with quartile aspect, where the signs form squares and are separated by 3 signs, and syzygy, where the signs are linked pairs which rise and set in the same place.

Geminos only explicitly links the "Chaldeans" with opposition but it is quite possible that he considers all four astrological aspects to be of Babylonian origin. So far, only trine aspect has been identified in cuneiform sources. Triplicities of zodiacal signs and months in the schematic 360-day calendar are found quite widely among Babylonian and Assyrian astrological texts, including (in month form) from considerably earlier than the invention of the zodiac.[13] One text, BM 36747, seems to link the triplicities with wind directions, in accord with II.8–11.

Our knowledge of Babylonian astrology from the late period remains very incomplete and it strikes me as quite possible that the other astrological aspects may yet be discovered in cuneiform sources.

The Rising Time of the Zodiacal Signs

Geminos devotes chapter VII of the *Introduction to the Phenomena* to a discussion of the variation in the rising time of the different signs of the zodiac. The rising times of the zodiac can be used to determine the length of daylight on a given day by adding together the rising time for the stretch of the ecliptic which extends for 180 degrees from the sun's position on that day. Geminos does not discuss the calculation of daylength from the rising times, but, as Evans and Berggren have shown, his statement in chapter VI that the second differences in the length of day are constant (VI.38) and his claim in chapter VII that the total rising and setting time for a zodiacal sign is always equal to 4 equinoctial hours (VII.36) are consistent with the use of a rising time scheme in which the rising time of the zodiacal signs varies according to a linear zigzag function whose maximum and minimum are at the beginning of Aries and Libra.[14]

Several rising time schemes are known from Babylonian astronomy. In the System A and System B lunar theories, a rising time scheme underlies the calculation of the length of daylight given in column C.[15] In System A, the rising times follow a linear zigzag function with minimum 20 UŠ (= time degrees) and maximum 40 UŠ and a difference between the signs of 4 UŠ. System B follows a broken zigzag function with maximum 21 UŠ and minimum 39 UŠ but with differences between the signs of 3 UŠ except in the months around the solstices months where it is 6 UŠ.[16] System A-type rising time schemes are quite widely attested in Greek sources beginning with Hypsikles in the second century BC, and it is most likely that Geminos drew upon these Greek sources for what he knew about rising times.

The 19-Year Cycle

Chapter VIII of the *Introduction to the Phenomena* concerns the lunar month and luni-solar calendars. Lines VIII.50–58 describe the 19-year intercalation cycle often referred to in modern

13 Rochberg-Halton 1984.

14 Evans and Berggren 2006, 73–82.

15 Neugebauer 1953.

16 A third rising time scheme is known from a group of texts which divide each zodiacal sign into twelve 2½ degree parts; in this scheme, the rising times of the zodiacal signs are 20 UŠ for six signs of the zodiac and 40 UŠ for the other six signs. See Rochberg 2004 and Steele 2017.

scholarship as the "Metonic cycle."[17] Geminos explains that in 19 years there 235 months containing a total of 6940 days, implying that in the 19-year period there are 7 intercalary months. Of the 235 months, 110 are 29-day months and 125 are 30-day months. Geminos then describes a calendar in which every 63rd day is removed to determine whether months have 29 or 30 days.[18]

Geminos attributes the 19-year cycle to "the astronomers around Euktemon, Philippos and Kallippos" (VIII.50). The cycle is often attributed to Meton in other sources. The 19-year cycle was employed in the Babylonian calendar from the early fifth century BC,[19] and it has often been suggested that Greek knowledge of this cycle came from the Babylonians. Whilst this is quite possible, it is worth noting that the cycle is quite simple to identify and was discovered and used independently in early Chinese calendrical astronomy from at least the second century BC onwards.[20] Thus, an independent Greek discovery should not be ruled out.

Lunar Theory

The final chapter of the *Introduction to the Phenomena* is considerably more technical than the rest of the work. It deals with two aspects of lunar theory: the Exeligmos cycle of 669 months and a scheme for the lunar velocity of the moon. Geminos presents the Exeligmos as a cycle containing a whole number of days, synodic months and anomalistic months (19756 days = 669 synodic months = 717 anomalistic months) (XVIII.1–3). He also notes that over one Exeligmos the moon makes 723 passages through the zodiac plus 32 degrees. It is interesting to note that Geminos does not mention that the Exeligmos also contains close to a whole number (726) of draconitic months and may therefore be used as an eclipse cycle. Indeed, the Exeligmos was certainly derived by tripling the Saros cycle of 223 synodic months = 242 draconitic months = 239 anomalistic months = approximately 6585 1/3 days to eliminate its 1/3 day excess in a whole number of days. The Saros cycle from the basis of Babylonian methods of eclipse prediction and was also widely known in the Greek world in this context.[21]

Geminos uses the Exeligmos to derive the parameters for a linear zigzag function to describe the daily change in lunar velocity. He presents the results in sexagesimal format, explaining the (originally Babylonian) number system at XVIII.8. Geminos derives the value 13;10,35 degrees for the mean daily motion of the Moon, remarking that this value "has been found by the Chaldeans" (XVIII.9). He also determines that the maximum and minimum values of the zigzag function are 15;14,35 degrees and 11;6,35 degrees, with a daily difference of 0;18 degrees.[22] As is well known, the zigzag function Geminos describes is column F* of the Babylonian System B lunar theory.[23] Geminos' derivation of its parameters from the Exeligmos is clearly an after-the-fact

17 On the Metonic cycle, see Neugebauer 1975, 2.622–624.

18 A version of this calendar is found in the upper back dial of the Antikythera Mechanism; see Freeth, Jones, Steele and Bitsakis 2008.

19 Britton 2007a.

20 For the use of the 19-year cycle in China, see, for example, Sivin 1969. Various claims have been made for a Babylonian origin of early Chinese astral science, mainly by scholars who do not control one or both of the Chinese and the Babylonian sources, but these claims do not stand up to close scrutiny; see Pankenier 2014 and Steele 2013 and in press.

21 On the Babylonian use of the Saros, see Steele 2000a. On Greek knowledge of the Saros, see Steele 2000b, 88–91.

22 For Geminos' derivation of these values, see Neugebauer 1975, 2.584–587 and Evans and Berggren 2006, 96–99.

23 On this theory, see Neugebauer 1975, 1.480–481 and Ossendrijver 2012, 188–189.

justification of the already existing function and almost certainly does not reflect the original Babylonian route to the construction of the function.[24]

What Did Geminos Know about Babylonian Astronomy and How Did He Know It?

The preceding summary shows that in several chapters of the *Introduction to the Phenomena* Geminos discussed astronomical concepts and techniques which have a Babylonian origin. This raises the question of whether Geminos knew that this material was Babylonian and from where he might have obtained it. Geminos only directly attributes three things to the "Chaldeans": the placement of the solstices and equinoxes at 8 degrees within their zodiacal signs, the concept of oppositions within astrology, and 13;10,35 degrees for the mean value of the daily motion of the moon. The other material with a Babylonian origin is simply presented as basic astronomical fact without any attribution to either a Babylonian or a Greek source.

A considerable amount of Babylonian astronomy and astrology was transmitted to the Greek world, including reports of lunar eclipses and planetary observations used by Hipparchos and Ptolemy,[25] large parts (or perhaps all) of the lunar System B,[26] many of the System A and System B planetary schemes,[27] and many ideas from later Babylonian astrology,[28] as well as basic ideas and methods such as the zodiac, the sexagesimal number system, and step and zigzag functions. The transmission of much of this material, in particular the observations and the various systems of mathematical astronomy, must have involved direct contact between Babylonian and Greek astronomers: cuneiform astronomical texts are sufficiently technical that they probably could not have been translated into Greek by a Babylonian scribe who was not himself an astronomer,[29] but they are sufficiently formulaic, employing a limited vocabulary written with a comparatively small number of cuneiform signs (and in the case of the tabular material, no grammar) that a Greek astronomer would probably have been able to learn to read cuneiform astronomical texts after only a relatively short period of instruction by a Babylonian astronomer. How often this happened, and whether the transmission of Babylonian astronomy was a gradual process or took place more or less at one time, remains an open question.

According to Jones and others,[30] Geminos wrote the *Introduction to the Phenomena* sometime during the first half of the first century BC. Many aspects of Babylonian astronomy are already attested in Greek sources well before this time. For example, the zodiac already appears as a well known concept in the works of Autolykos and Euclid around 300 BC,[31] the 19-year cycle was known already in the fifth century BC (and may be an independent discovery anyway), System

24 The origin of column F* in System B is, like all other functions in Babylonian mathematical astronomy, never discussed in cuneiform texts. For a possible reconstruction of how the lunar anomaly functions of System A and B were derived, see Britton 2007b and 2009.

25 On the lunar eclipse observations and their transmission, see Steele 2000, 91–100 and 2011. On the planetary observations, see Jones 2006.

26 Jones 1993 and 2002.

27 Jones 1998.

28 Rochberg-Halton 1988.

29 Steele 2004.

30 Jones 1999, Evans and Berggren 2006, 17–22.

31 Neugebauer 1975, 2.593.

A-style rising time schemes were discussed by Hypsikles in the second century BC,[32] and Hipparchos clearly had knowledge of at least parts of the System B lunar theory, including its mean length for the synodic month and the period relation for column F and the 248-day scheme underlying column F*.[33] Thus all of the Babylonian astronomy which appears in Geminos' book, with the possible exception of the astrological aspects discussed in chapter II, was already known to Greek astronomers.

Given that all of the Babylonian astronomy that underlies parts of Geminos' book was already in circulation in the Greek world, there is no reason to suppose that Geminos had any contact with Babylonian astronomers himself. It is much more likely that he obtained this material from Greek sources. Indeed, as I have discussed, much of it had been assimilated into the general astronomical knowledge of the time and was no longer distinctly "Babylonian." Only the discussion of the lunar theory in chapter XVIII draws on what may not have been assimilated into this general astronomical knowledge. Nevertheless, even this material was fairly widely known among Greek astronomers: it was known to Hipparchos in the middle of the second century BC and appears in papyri astronomical tables, the earliest preserved of which dates to the latter part of the first century BC, but there is no reason to suppose that tables were not produced already at the time of Geminos. Evans and Berggren suggest that Geminos' discussion of the lunar theory is based upon his knowledge of such tables of lunar velocity.[34] While this is possible, it seems to me to be just as likely that Geminos knew the parameters for column F* from Hipparchos. Geminos employs the System B value for the mean length of the synodic month, a value which Hipparchos reports (along with a pretended derivation from observation), in the calculation of the number of days in the Exeligmos (XVIIII.3).[35] Thus, it would appear that Geminos knew not only column F* from System B but also the mean value of column G which gives the mean length of the synodic month. Geminos could in theory have determined the mean value of column G from analyzing a papyrus table containing calculated values of this function, but it seems more likely that he would simply have taken this value from Hipparchos.

There remains the question of why Geminos sometimes attributes items of knowledge to the "Chaldeans." Geminos cites many authorities throughout the *Introduction to the Phenomena*,[36] but he is not consistent in his practice: several important sources, including Euclid, Autolykos and Theodosios are not named even though he drew extensively upon their work.[37] As discussed above, only a small selection of the Babylonian material Geminos discusses is attributed to the "Chaldeans": the placement of the solstices and equinoxes at 8 degrees within their zodiacal signs, the astrological aspects, and the mean value of column F*. It is possible that the reference to the "Chaldeans" may be simply a literary device to suggest that this material is "old" or, perhaps, outside of the mainstream of astronomical practice, but given the common association of the term "Chaldean" with astrology in the Greco-Roman world, I wonder if Geminos is indicating that this material reflects the practices of everyday Greek astrologers. The 8 degree norm for the solstices and equinoxes is referred to in Greek and Latin astrological writings and (at least in later times) there is considerable papyrological evidence for the use of Babylonian mathematical

32 Neugebauer 1975, 2.715–718.

33 Jones 1993, 84–85.

34 Evans and Berggren 2006, 100.

35 Evans and Berggren 2006, 288 footnote 6.

36 For a list, see Evans and Berggren 2006, 301–302.

37 Evans and Berggren 2006, 13.

astronomy by practicing astrologers. Thus, it is possible that Geminos is labeling as "Chaldeans" those astronomical methods such as zigzag functions which are employed by everyday astrologers to distinguish them from what we might call "scholarly" astronomers and astrologers who used geometrical models.

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Mathematical discourse in philosophical authors: Examples from Theon of Smyrna and Cleomedes on mathematical astronomy

Nathan Sidoli

Introduction

Ancient philosophers and other intellectuals often mention the work of mathematicians, although the latter rarely return the favor.[1] The most obvious reason for this stems from the impersonal nature of mathematical discourse, which tends to eschew any discussion of personal, or lived, experience. There seems to be more at stake than this, however, because when mathematicians do mention names they almost always belong to the small group of people who are known to us as mathematicians, or who are known to us through their mathematical works.[2]

In order to be accepted as a member of the group of mathematicians, one must not only have mastered various technical concepts and methods, but must also have learned how to express oneself in a stylized form of Greek prose that has often struck the uninitiated as peculiar.[3] Because of the specialized nature of this type of intellectual activity, in order to gain real mastery it was probably necessary to have studied it from youth, or to have had the time to apply oneself uninterruptedly.[4] Hence, the private nature of ancient education meant that there were many educated individuals who had not mastered, or perhaps even been much exposed to, aspects of ancient mathematical thought and practice that we would regard as rather elementary (Cribiore 2001; Sidoli 2015).

Starting from at least the late Hellenistic period, and especially during the Imperial and Late-Ancient periods, some authors sought to address this situation in a variety of different ways—such as discussing technical topics in more elementary modes, rewriting mathematical arguments so as to be intelligible to a broader audience, or incorporating mathematical material directly into philosophical curricula. None of this resulted in the equivalent of a modern textbook, but the results were works that were meant to have an educational, or at least introductory,

1 In this paper, I use the term *mathematician* to denote a practitioner of those disciplines that used mathematical techniques or that investigated mathematical objects—actual or ideal—such as geometry, mechanics, optics, astronomy and astrology, number science (*arithmetikē*), harmonics, computational methods (*logistikē*), spherics (*sfairikē*), sphere-making (*sfairopoiïa*), sundial theory (*gnōmonikē*), and so on. See Netz (1997, 6–9) for a list of the names of very nearly all of the mathematics and mathematical scholars who are known to us from the Greco-Roman period. In this paper, I will argue that a number of the scholars in this list would not have been counted as mathematicians by their peers.

2 A striking counterexample is Ptolemy, who at the beginning of his *Almagest* mentions, with praise, the name of Aristotle, immediately following which he inverts the latter's epistemological hierarchies and asserts that only mathematics can produce real theoretical knowledge and that studies of nature and the divine stand to learn from mathematics—not the other way around.

3 There is a large literature on mathematical Greek prose, for a quick overview of which see Sidoli (2014, 29).

4 Galen recounts that he studied the mathematical sciences from his father in his youth, and Theon of Smyrna explicitly tells us that extensive study from youth was necessary for competence in mathematics; see Sidoli (2015, 395–396) and Jones (2016, 471).

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function. Examples of this type of treatise include Geminos' *Introduction to the Phenomena*, which guides the uninitiated through the basics of mathematical astronomy; Theon of Smyrna's *Mathematics Useful for Reading Plato*, which introduces a medley of mathematical topics divided into number science, geometry, harmonics and astronomy; Cleomedes' *On the Heavens*, which shows how mathematical approaches can be of service in the philosophy of nature and the cosmos; and Proclus' *Commentary on Euclid's Elements Book I*, which expounds the details of deductive geometry as part of a larger philosophical, indeed, spiritual project.

For historians and scholars of the ancient period, works such as these offer a rich source of material for learning about mathematical methods and results that have not been preserved in mathematical sources. They are also, however, fraught with interpretive difficulties because the goals, educational backgrounds, philosophical outlooks, and technical competencies of the authors of such sources are often different from that of the authors whose work they report. In order to make a coherent attempt to read through our sources to the claims and practices of the reported mathematicians, we must both separate the goals of our sources from those on which they report, and also situate the reported work in a context of the methods and research programs actually found in other ancient mathematical works. This must be done on a case-by-case basis, and in many instances we will not be able to say much with real certainty.

In this paper, I will look at two short examples—taken from Cleomedes and Theon of Smyrna—with the aim of articulating the context of ancient mathematical work from which this material originates. Although I will not argue that a full reconstruction of the underlying mathematical models and methods is possible, I hope to show that when we situate the ideas and methods discussed in these sources within a context of ancient mathematical methods reported in other sources, we can develop a clearer picture of both the ancient mathematics reported and of the ways our sources handled this material.

Cleomedes, On the Heavens *I.7*

In his only known surviving work, *On the Heavens*, Cleomedes—a Stoic philosopher who lived sometime between the middle of the 1st century BCE and the end of the 2nd century CE—seeks to introduce cosmography to students of philosophy who he takes to have only the most rudimentary knowledge of mathematics.[5] Hence, he goes to some lengths to simplify geometrical configurations to the extent that they can be followed from his oral exposition alone, and with no mathematical procedures beyond the rule-of-three.

The passage below is his discussion of a computation of the terrestrial circumference, as 250,000 stades, which he attributes to Eratosthenes, *Heavens* I.7. Cleomedes tells us that Eratosthenes' procedure was geometrical and difficult, so the striking simplicity of the configuration and the computation that he goes on to describe may come as somewhat of a surprise.

Furthermore, there are issues involved in reconciling both the numerical value and the overall simplicity of Cleomedes' account with Heron's claim in *Dioptra* 35 that "Eratosthenes, having worked rather more accurately than others, showed in his book entitled *On the Measurement of the Earth*" that the terrestrial circumference is 252,000 stades (Acerbi and Vitrac 2014, 104). If the mathematical methods implied by Heron's *Dioptra* 35 are any indication of what he means by a

5 See Bowen and Todd (2004, 1–17), for an introduction to this source.

mathematician working carefully,[6] then it is difficult to see how he could have been impressed by something so trivial as the configuration described by Cleomedes.

Eratosthenes was a younger contemporary of Archimedes, to whom the latter chose to send his *Method*. This, as well as Heron's assessment, is an indication that Eratosthenes was a serious mathematician whose work developed the efforts of other early Hellenistic mathematicians. Hence, we should regard Eratosthenes' lost *On the Measurement of the Earth* as a treatise in the tradition of Aristarchus' *On the Sizes and Distances of the Sun and the Moon*, or Archimedes' *Sand Reckoner*.[7] That is, it probably made use of somewhat crude, observational hypotheses,[8] extensive geometrical modeling using lettered diagrams, proto-trigonometric inequalities between angles and sides of right triangles, ratio manipulation, and some arithmetic operations.[9] None of this, however, is found in Cleomedes' account.

We should consider the possibility that Cleomedes had never read the original source and was less interested in reporting what Eratosthenes actually did than in drawing out certain mathematical principles so as to make his own philosophical points. Cleomedes compares Eratosthenes' procedure with that of Posidonius, from whom he may well have taken both accounts. Although Posidonius was probably interested in arguing against the claims of mathematicians to be able to produce specialized knowledge about the physical world,[10] Cleomedes seems to have had more restricted goals. He was clearly interested in exhibiting a direct cognitive relationship between assumptions and conclusions,[11] and appears to have reworked Eratosthenes' argument so as to make it amenable to such an approach. The goal of Cleomedes' procedure is to show how geometrical assumptions can be combined with sense perceptions to make true assertions about the physical world that go beyond what our senses alone can directly decide.

After describing Posidonius' procedure, Cleomedes turns to Eratosthenes', saying:[12]

> [1] … That of Eratosthenes involves a geometric procedure (*geōmetrikē efodos*),[13] and it is thought to involve something more obscure. But, the assertions by him will be made clear from the following prior suppositions (*proüpothemenos*) of ours.
>
> [2.1] Let it here have been assumed by us, first, that Soēnē and Alexandria are situated under the same meridian, [2.2] and [second] that the distance between the cities is 5000 stades, [2.3] and third that the rays sent down from different parts of the sun to different parts of the earth are parallel—just as the geometers assume holds. [2.4]

6 See Sidoli (2005) for a discussion of these mathematical methods. This should be compared against Acerbi and Vitrac (2014, 103–115) for some textual corrections.

7 See Dijksterhuis (1987, 360–373), Berggren and Sidoli (2007), Van Brummelen (2009, 20–32), and Carman (2014) for discussions of the mathematical methods of these sources and their use of hypotheses.

8 That is, claims about observational results that may or may not have been the result of carefully made, carefully recorded observations, but which, in the structure of the mathematical argument, are taken simply as assumptions.

9 See Carman and Evans (2015) for a discussion of the details of what such a research program might have involved.

10 See Bowen (2007) for a discussion of these sorts of jurisdictional disputes in ancient authors, in which Posidonius played a role.

11 See Bowen (2003) and Bowen and Todd (2004, 11–15) for discussions of Cleomedes' interest in the structure of demonstrations.

12 My translation can be compared with that of Gratwick (1995, 179–180) or Bowen and Todd (2004, 81–84). I have tried to preserve those places where Cleomedes' prose seems technically awkward.

13 Bowen (2003) gives a justification for translating *efodos* as *procedure*. See below for a discussion of Cleomedes' characterization of Eratosthenes' procedure as "geometrical."

Fourth, let it be further assumed—as is shown by the geometers—that straight lines that fall on parallel [lines] make the alternate angles equal; [2.5] fifth, that arcs that stand upon equal angles are similar; that is, they have the same proportion and the same ratio to their own circle (*oikeios kuklos*)—which is also shown by the geometers, for whenever arcs stand on equal angles, if any one of them is ten parts of its own circle, all of the rest will be ten parts of their own circles. [2.6] One who has mastered these things would have no difficulties understanding the procedure of Eratosthenes, which is as follows.

[3.1] He says that Soēnē and Alexandria are situated under the same meridian. So, since meridians are great [circles] of those in the cosmos, those lying under them will necessarily be great circles of the earth. [3.2] Hence, however much (*hēlikos*) this procedure shows the circle reaching through Soēnē and Alexandria to be of the earth, so much (*tēlikoutos*) is the great circle of the earth.

[4.1] He states—and it holds—that Soēnē lies under the circle of the summer tropic. So, whenever the sun comes to be in Cancer and brings about the summer tropic,[14] exactly at culmination the gnomons of sundials (*hōrologion*) are necessarily shadowless, because the sun is situated perpendicularly above—and it is said that this is three hundred stades in diameter. [4.2] But, in Alexandria, at the same hour, the gnomons of sundials cast shadows, inasmuch as this city is situated further north than Soēnē.

[5.1] Now, since these cities are situated under a great circle meridian, if we produce an arc from the tip of the shadow of the gnomon around to the base of the gnomon of the sundial in Alexandria, this arc will be a part (*tmēma*) of the greatest of the circles in the bowl [of the sundial],[15] since the bowl of the sundial is situated under a great circle. [5.2] So, if we next imagine (*noeō*) straight lines extended through the earth from each of the gnomons, they will meet at the center of the earth. [5.3] So, since the sundial in Soēnē is located perpendicularly under the sun, if we further imagine a straight line from the sun reaching to the gnomon tip of the sundial, it will be one straight line, reaching from the sun as far as the center of the earth. [5.4] So, if we imagine another straight line from the tip of the shadow of the gnomon to the sun, being produced from the bowl in Alexandria, this [straight line] and the aforesaid straight line will be parallels—that is, they are extending from different parts of the sun to different parts of the earth. [5.5] Now, a straight line reaching from the center of the earth to the gnomon in Alexandria falls on these parallels, hence the alternate angles are made equal—one of which is at the center of the earth, at the meeting of the straight lines that were produced from the sundials to the center of the earth, while the other is at the meeting of the tip of the gnomon in Alexandria and the [straight line] produced from the tip of its shadow to the sun through the point of contact. [5.6] And, upon this [angle] stands the arc produced from the tip of the shadow of the gnomon to its base, while the [arc] produced from Soēnē to Alexandria [stands] upon that at the center of the earth.

14 Literally, "makes (*poieōn*) the summer turn."

15 That is, it will be one of the great circles of the bowl. This expression is not usual in Greek mathematical texts, so I have tried to preserve the literal expression.

> [6.1] Now, the arcs are similar to one another—that is, they stand on equal angles. Therefore (*ara*), the ratio which the [arc] in the bowl has to its own circle, is also that ratio which the [arc] reaching from Soēnē to Alexandria has [to its own circle]. [6.2] But, the [arc] in the bowl is found to be, indeed, a fiftieth proper part (*meros*) of its own circle. So, the distance from Soēnē to Alexandria must also necessarily be a fiftieth proper part of the great circle of the earth—and this [distance] is 5000 stades. [6.3] Therefore (*ara*), the whole circle comes to 25 myriads. Such is the procedure of Eratosthenes.
>
> (Todd 1990, 35–37)

Once again, this passage exhibits a number of striking features, when read from the perspective of Greek mathematical sources. The first is the absence of a diagram and letter-names, despite Cleomedes' characterization of the procedure as geometrical. Since diagrams and letter-names are one of the defining characteristics of Greek geometrical prose, it is clear that Cleomedes means something quite specific by calling this procedure *geōmetrikē*. As discussed above, he probably means that the grounds for accepting the claim as true depend on certain hypotheses or knowledge claims that are geometrical, as opposed to physical, or based in sense perception.

Although the deductive language of the passage is fairly standard for philosophical writings, there are peculiarities from a mathematical perspective. For example, the assumptions are set out using impersonal imperatives, while the constructions are imagined using personal verbs, geometrical terminology is used in unusual ways, and there are a number of peculiar expressions that will be discussed in detail below.

Finally, although Cleomedes deliberately structures his account, there is little or no trace of the usual mathematical structure, such as we find in Aristarchus' *On Sizes*, or, less concretely, in Archimedes' *Sand Reckoner*.[16] There is no enunciation of the computational result to be obtained, no exposition of the actual configuration through a lettered diagram, no geometrical constructions, nor any real mathematical argumentation. Instead, Cleomedes has carefully laid out all of the starting points of the reasoning in the beginning, as hypotheses, so that the result can be seen to follow almost immediately from a description of the spatial arrangement of the objects in question. That is, he shows that if certain geometrical propositions are true, they allow us to infer true claims about the world that we cannot perceive, based on the local world that we can perceive, or potentially measure. In order to appreciate Cleomedes procedure, it will be useful to follow his argument in detail.

He begins, in [1], by asserting that the procedure of Eratosthenes is geometrical and difficult, but that he will simplify it, by clearly stating all of its starting points. Bowen and Todd (2004, 78, n. 1), following a suggestion of Gratwick (1995, 178, n. 1), argue that here *geōmetrikos* must be understood as *geodesic*, since Posidonius' procedure also employs geometry. Indeed, modern reconstructions of Posidonius' method often formulate it as parallel to that of Eratosthenes.[17] But these are reconstructions. Cleomedes' account of Posidonius' procedure makes no use of

16 It is well known that Greek propositions rely on structure to convey certain aspects of their deductive force. For discussions of the structure of propositions in the *Elements*, see Mueller (1981, 11–12) and Netz (1999b). Acerbi (2011a, 1–117) bases his discussion of structure on the *Elements* as well, but also treats other Greek mathematical works.

17 For example, Taisbak (1974, 257–259) formulates the two arguments as structurally similar, and provides similar diagrams for each, in which he is followed by Bowen and Todd (2004, 78–85, 181–183).

geometry[18] and it is possible to explicate the argument with a single analemma diagram and no use of the geometrical assumptions that Cleomedes posits for Eratosthenes' procedure.[19]

Whatever our opinion of Posidonius' method, however, it is clear that Cleomedes himself intends us to understand Eratosthenes' procedure as more geometrical than it, because he will go on to preface the latter by discussing five assumptions, three of which he explicitly attributes to "the geometers." Hence, in Cleomedes' opinion, Eratosthenes' procedure relies on a series of assertions and knowledge claims made by a group of specialists—and it is the use of this specialized knowledge, and presumably the use of a specialized way of writing, that marks Eratosthenes' procedure *as geometrical*. Moreover, it is clear from his presentation that Cleomedes intends to simplify this procedure, making it more intelligible to the non-specialist.

The five assumptions that Cleomedes proposes are given in the next paragraph, [2]. The fact that Cleomedes claims these hypotheses as his own is a good indication that he has rearranged the argument. Although some of them, such as [2.3] concerning the rays of the sun, may well have been asserted by Eratosthenes, others, such as [2.4] and [2.5] were most likely assumed without comment, as obvious—just as they would have been by Aristarchus or Archimedes.

The first two hypotheses, [2.1, 2.2], namely that Soēnē and Alexandria are assumed to lie below a great circle through the celestial poles, and that they should be taken as 5000 stades apart, are essentially the same as those assumed by Posidonius, and Cleomedes apparently did not regard them as geometrical. He presumably thought that they were decidable using empirical methods or were idealizations of empirical claims.[20]

The next three hypotheses are those which Cleomedes claims are either assumed or demonstrated by the geometers. The first of these, [2.3], is that the lines joining different parts of the sun with different parts of the earth must all be taken to be parallel—that is, the lines drawn from any point on the earth to any point on the sun are all parallel. Gratwick (1995, 187–188) argued that this hypothesis must have been muddled by Cleomedes because it does not agree with our understanding of the composite nature of shadows. Furthermore, if the sun is taken to be a body at some definite distance, and of some definite magnitude, as was generally held to be the case, then it would be strictly false that these lines would be parallel. Nevertheless, consideration of Aristarchus' hypotheses in his *On Sizes*,[21] shows that mathematicians did, in fact, make assumptions that they knew to be strictly false, in order to see where the reasoning would lead. If Eratosthenes' approach was anything like that found in Aristarchus' *On Sizes* or Archimedes' *Sand Reckoner*, he would have used observational hypotheses as means to constructing an idealized

18 That is, unless any spatial reasoning must be counted as geometry.

19 For example, we can draw both horizons in the same analemma figure, so that the arc between Canopus on the northern horizon of Rhodes and the southern horizon of Alexandria represents at once the difference in the celestial and the terrestrial latitudes.

20 Bowen (2003, 63—64) claims that Cleomedes took Eratosthenes' value of 250,000 to be better than Posidonius' value of 240,000 because he also asserts it in *On Heavens* I.5 and II.1. This then involves us in having to decide why Posidonius' 5000 stades between Rhodes and Alexandria is a weaker assumption than Eratosthenes' 5000 stades between Alexandria and Soēnē. Another possibility is that Cleomedes took these values to be essentially the same and just asserted the round figure. Since, in either case, a difference of about 200 stades (4%) would give the same value, Cleomedes may well have accepted that, given the crudeness of the procedure, this difference was undecidable—especially since, in [4.1], he seems to say that Soēnē lies in a region of 300 stades that can be *assumed* to lie directly below the summer tropic, so that 200 stades would be well within the margin of error of the hypothesis.

21 See Berggren and Sidoli (2007, 231–234) and Carman (2014, 55–58) for discussions of Aristarchus' use of hypotheses.

geometric model, which would then have become the sole object of his reasoning and computation. Moreover, as Carman and Evans (2015) have argued, the assumption of a sun at an infinite distance may have been used to compute a lower bound for the size of the earth, which could then be compared with an upper bound computed under the equally idealized but contrary assumption of a point-sun at a given, or at least bounded, distance. At any rate, in his presentation, Cleomedes calls on this hypothesis in precisely the way that he states it—as involving the lines joining any point of the earth to any point of the sun.

The next assumption, [2.4], is simply *Elements* I.29—“a straight line falling on parallel straight lines makes the alternate angles equal to one another, the external equal to the opposite and internal, and those on the same side equal to two right angles” (Heiberg 1969–1977, 41). Eratosthenes, however, certainly would not have taken this as a hypothesis in the sense that Cleomedes intends. If Eratosthenes assumed that this were true, he did so because it was the subject of geometrical demonstration and could be assumed without further comment. Whether or not Eratosthenes was thinking of Euclid’s text, he certainly would have known that this proposition had been demonstrated. Cleomedes acknowledges as much by pointing out that “the geometers” prove this proposition, but still asserts that he will use it as an explicit assumption of his philosophical argument. That is, he avoids the question of a toolbox of mathematical knowledge and techniques that usually forms the background of any mathematical argument.[22]

The final geometrical assumption, [2.5], is the claim that arcs that subtend equal angles are similar—that is, they compose the same part of, or have the same ratio with, their respective circles. The expression that Cleomedes uses for the circle to which an arc belongs, *oikeios kuklos*, although common in *On the Heavens*, is not standard in Greek mathematical prose. Furthermore, a repetition of the “same proportion” and the “same ratio” is not usual in mathematical authors, and would be strange from a mathematical point of view. Both of these expressions mark this passage as something that a mathematician was unlikely to have actually said. Indeed, I am not aware of any proof of this claim in the elementary geometrical texts, although *Elements* III.def.11 mentions similarity of segments. Nevertheless, this is assumed by Aristarchus in his *On Sizes*—for example in Prop. 7—and, of course, lies at the core of later chord-table trigonometry. The fact that Cleomedes gives a numerical example is an indication that he is thinking of computational work such as we find in *On Sizes*. Eratosthenes may have argued explicitly for this proposition, as part of his development of the proto-trigonometric tradition of Aristarchus and Archimedes, or Cleomedes may simply be using the idea of *showing* loosely.

The subsequent paragraphs develop the logical argument that Cleomedes presents, calling on and applying each of the hypotheses in very nearly the same order as they were presented. Since the procedure itself calls on two further empirical claims, there must be some difference between these and those discussed above for the first two hypotheses, [2.1, 2.2]. As will be discussed below, the two empirical claims presented in the course of the argument are, at least in principle, verifiable with direct sense perception, whereas the distance between the two cities and their location on a single meridian can only be apprehended through a variety of sense perceptions and logical inferences.[23] It is also possible that [2.1] and [2.2] are taken as hypotheses because they are being acknowledged as not strictly true.

After reiterating the first hypothesis, in [3.1], and pointing out that this implies that the two cities lie on a single great circle of the earth, Cleomedes gives a sort of summary of the whole

22 The mathematical toolbox is discussed by Netz (1999a, 216–235), who bases this idea on a research project by Saito (1997, 1998).

23 See the discussion by Bowen (2003), for the distinction between these types of propositions in Cleomedes’ work.

procedure, in [3.2]. Namely, if we can determine what part of the great circle of the earth joins the two cities, then we can determine the size of the whole great circle. Cleomedes' way of putting this is vague on two counts. The first is that he does not use the usual language of ratios, or parts, but more general expressions for relating amounts, or quantities (*hēlikos*, *tēlikoutos*)—here intended to imply a use of the computational rule-of-three.[24] The second is that he uses the word *kuklos* in a non-standard way, as a synonym for *perifereia*, denoting both an arc and a whole circumference.

The next paragraph, [4], introduces the first set of empirical considerations that were not assumed from the start. Namely, the claim that around the summer solstice, each day at midday, a gnomon at Soēnē casts no shadow, in [4.1],[25] while one at Alexandria does, in [4.2]. It also asserts that shadows are said to disappear at midday around the solstice "three hundred stades in diameter" (Todd 1990, 36). This ambiguous expression is repeated in two other places in the treatise, neither of which helps us much in understanding its meaning (Todd 1990, 51, 53). The use of *diametros* implies that we are talking about a circle, or a rectangle, but it is unclear how either of these would be defined. Perhaps we are discussing a region defined by midday at around the same time and noon shadowlessness around the summer solstice.[26] Another possibility is that Cleomedes is referring to a band of midday shadowlessness around the latitude of Soēnē. The width of such a region, however, would be measured by the arc of a great circle through the poles, which he elsewhere calls a distance, *diastēma*. At any rate, Cleomedes gives no indication of where in this region he takes Soēnē to be, which is further indication that the starting points of the argument are meant to be understood as loose idealizations of reality.

The bulk of the argument, [5], consists of a description of the spatial arrangement of the various elements of the model, which attempts to do away with the need for a diagram. The first sentence, [5.1], is a way of expressing the idea of passing a cutting plane through the celestial meridian above the two cities, such that it passes through the sundial and gnomon in Alexandria, producing a great circle in the sundial's bowl—which is implicitly taken to be spherical. Hence, an arc from the tip of the shadow to the base of the gnomon—that is, along the shadow—will be a great circle of the dial's face. All of this follows from the fact that a great circle is concentric with its sphere—as shown in Theodosius' *Spherics* I.6, and assumed implicitly in the texts on spherics by Autolycus and Euclid. The next stage of the argument involves a solid configuration, which is signaled by Cleomedes' use of the word *noeō*—a standard term in Greek mathematical prose used to indicate that we are dealing with something that is three dimensional, or not contained in the diagram.[27] Cleomedes, however, uses the term with a personal expression—"we imagine." We imagine lines extended through idealized gnomons in the two cities meeting in the center of the earth, [5.2]. Hence, that of the gnomon in Soēnē will be produced continuously as a ray of the sun, [5.3], and will be parallel to a ray of the sun through the tip of the gnomon at Alexandria, [5.4], so that the continuation of this gnomon falls on these two parallel rays, making equal alternate

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24 A passage in Galen's *Art of Medicine* directly relates these expressions to proportion: "For, the spinal [cord] is, in this way, proportional to the many, and just as (*tēlikoutos*) the size of the vertebrae, so much (*hēlikos*) is the spinal cord, and so the whole backbone" (Kühn 1821, 132).

25 Gratwick (1995, 192) claims that Eratosthenes must be referring to the exact moment of midday on the summer solstice, but this is not necessitated by the text, nor was anyone likely to have been able to determine the precise day of summer solstice simply by observing the noon shadow of a gnomon.

26 That is, a region in which we cannot perceive any difference of latitude or longitude.

27 See Netz (1999a, 52–56), Sidoli and Saito (2009, 592, n. 41), and Netz (2009) for discussions of this terminology.

angles, [5.5]. Hence, the angle of the shadow at Alexandria is the same as that at the center of the earth, [5.6]. These passages present the core of the geometric model, and almost all of the mathematical reasoning involved in the procedure that Cleomedes describes.

The final paragraph, [6], asserts the conclusion of the procedure—hence the use of *ara*. Since the arcs stand on equal angles, they are the same part of the circles in which they stand, [6.1]. This is stated, here for the first time, as a ratio, but the expression, although not unknown in general usage, is not common among mathematical authors, who usually assert proportionality as a *sameness* of ratio. We are then told that the shadow in the dial at Alexandria is actually found to be 50′ of the great circle, expressed in the Egypto-Greek fractions that would have been familiar to any educated reader—that is, a unit fraction, or proper part (*meros*).[28] This is another empirical datum, but Cleomedes does not assert it as a hypothesis. Hence, unlike the distance between the two cities, he seems to take it as something directly verifiable, and hence true—just as the location of Soēnē below the summer tropic. Thus, the two given values—one assumed and the other found—can be subjected to the rule-of-three to give the value of the circumference of the earth, 250,000, which is stated as the conclusion of the whole procedure, [6.3].

In this way, Cleomedes rearranged Eratosthenes' claims and arguments so as to present them as part of his overall project of demonstrating Stoic procedures to his audience. Hence, it is difficult to reconstruct Eratosthenes' approach directly from Cleomedes' account. To form a sound idea of Eratosthenes' thought, we need to reconstruct his work in the context of authors such as Aristarchus and Archimedes, in order to see how it could have been sufficiently mathematical as to have struck Heron as having been carefully undertaken.[29] Such a project will, however, have been different both in presentation and conception from that of Cleomedes.

Theon of Smyrna, Mathematics Useful for Reading Plato

Theon of Smyrna, a middle Platonist, who can probably be dated to the early part of the 2nd century CE, based on a portrait bust from Smyrna,[30] was a philosopher writing for students of philosophy who wanted to understand Plato, but who had not had much training in mathematics. He only hoped that they should have at least advanced through the "first geometrical elements" (Hiller 1878, 16). Whatever the nature of Theon's own training, he seems never to have developed much understanding of either mathematics or the standard usage of Greek mathematical prose.

Theon took much his material from the philosopher Adrastus, but the technical passages are generally presumed to have originated in the work of a mathematician, such as Hipparchus. In going through Adrastus' discussion of the eccentric and epicyclic solar models, Theon demonstrates that, in the eccentric model, the solar orbit is *given in position and in magnitude*. The property of being *given* was fundamental in theoretical Greek mathematics, and Euclid devoted his *Data* to developing theories of different modes of being given. In broad strokes, an object was said to be given if it were present at the beginning of the mathematical discourse, introduced by the mathematician, or produced from either of these in a determinate way.[31]

28 See Bernard, Proust and Ross (2014, 38–51) and Sidoli (2015) for discussions of mathematics education in the Greco-Roman world. Here and below, I use n' for $1/n$, as is standard in scholarship on Greek mathematical sources. In scholarship on Egyptian sources such parts are usually denoted $\bar{n}$.

29 See Carman and Evans (2015) for an investigation along such lines.

30 Musei Capitolini inv. 529; see Richter (1965, vol. 3, 285).

31 See Acerbi (2011b) for an overview of the way the term is used in Greek mathematical texts and Sidoli (2018)

Theon's argument proceeds by taking the degree position of the solar apogee and the ratio of solar eccentricity as given—but it does not do so in a straightforward way. The solar apogee is taken to be Gem 5,2′°—that is, 65;30°. Theon does not say how this value is derived but it is the same as that in the solar model attributed to Hipparchus by Ptolemy.[32] The ratio of the distance from the earth to the center of the sun's orbit compared to the radius of the sun's orbit is taken to be 1 : 24, as was apparently known "through the treatise *On Sizes and Distances*," probably by Hipparchus (Hiller 1878, 158).

In Ptolemy's account of Hipparchus' procedure, these two values are derived through chord-table trigonometric computation on the basis of assumed observations of season lengths. The following passage of Theon's presentation appears instead to argue that we can use the derived parameters of the model to show that the position and size of the solar orbit is given—that is, determined in place and in size. In fact, however, this passage also includes numbers relating to season length—the sums of the length of spring plus summer and of autumn plus winter. These numbers alone, however, are not sufficient to determine the parameters that Hipparchus derived. The passage we are interested in reads as follows:

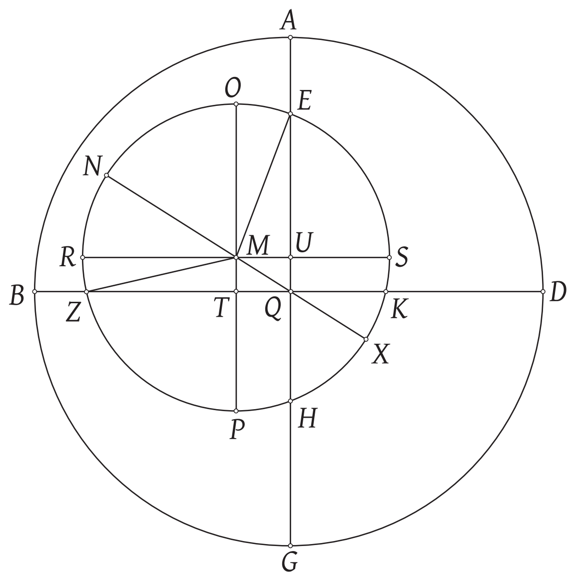

Figure 1. Reconstructed diagram for Theon's eccentric solar model, following Martin (1849, Descriptio III).

> [1] The circle *EZHK* is found *given in position and in magnitude*. [2] For through *M* let parallels to *AG*, *BD* be produced perpendicular to one another, *OP*, *RS*; and let *ZM*, *ME* be joined. [3] Then, it is clear that, the circle *EZHK* being divided into 365,4′ days, arc *EZH* is 187 of such days, and *HKE* is 178,4′ days. [4] So (*ara*), each one of the pairs *EO*, *PH* and *RZ*, *SK* are equal, but the original arcs *SP*, *PR*, *RO*, and *OS* are equal to 91,4′,16′ of such [days]. [5] So (*ara*), the given angle *OMN* will be equal to *QMT*, and likewise,

for a discussion of the concept of given in Greek mathematics.

32 Ptolemy's presentation of Hipparchus' solar model, with which this passage should be compared, is found in *Almagest* III.4.

> angle *RMN* is equal to angle *UMQ*.[33] [6] So (*ara*), the ratio *MT* to *MQ*, or rather (*toutesti*) *MT* to *TQ*, will be [given]. [7] So (*ara*), triangle *MTQ* is *given in form*.[34] [8] And the center of the cosmos, *Q*, to each of the points *N* and *X* is given, for one defines the greatest distance, and the other the least; and *QM* is between the center of the cosmos and [the center] of the solar circle. [9] So (*ara*), the circle *EZHK* is *given in position and magnitude*, since it is found through the treatise *On Sizes and Distances* that the ratio *QM* to *MN* is nearly one to 24.
>
> (Hiller 1878, 157–158)

There are a number of conspicuous features of this passage that give us pause in categorizing it as normal mathematical prose. The first arises only when we look at the manuscript source for this passage, *Marc. gr.* 303. The diagram for this passage, like many of the diagrams for this text in the manuscript, appears to have been drawn as a sort of afterthought (folio 12r, Figure 2). It was squeezed into the bottom margin, where it was later partially trimmed off. It is so poorly drawn that it is unlikely that the text could have been understood on the basis of this diagram alone. Diagrams in mathematical texts are sometimes poorly drawn,[35] but those accompanying Theon's treatise in *Marc. gr.* 303 are particularly inept. Nevertheless, the use of letter-names in the text makes it clear that it was meant to be read with a diagram, and we may presume that the diagram that Theon originally produced was correct. Hence, it seems that the copyists and readers of this treatise thought of it as part of a philosophical tradition and were not much concerned with mathematical details, and the corruption of the diagram was probably due to the accidents of transmission.

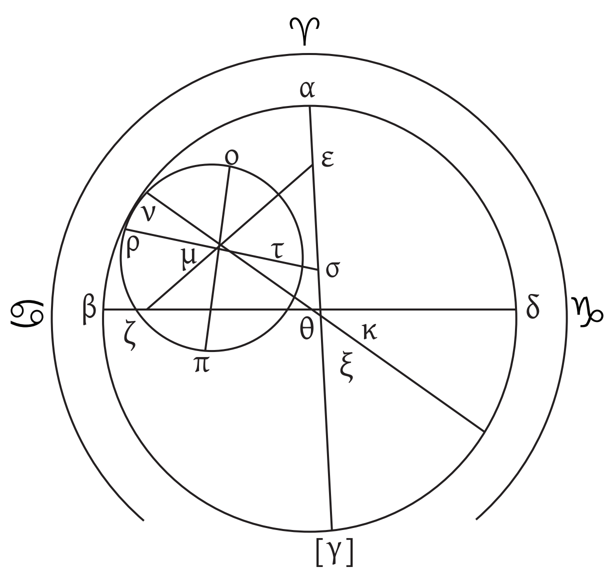

Figure 2. Diagram for Theon's eccentric solar model in *Marc. gr.* 303, f. 12r.

The next conspicuous feature of this passage is its peculiar use of mathematical prose. The passage appears to have been written by someone who was uninterested in following common

33 *Elements* I.15.

34 *Data* def.3.

35 See Saito and Sidoli (2012) for an overview discussion of the diagrams in the manuscripts of Greek mathematics.

mathematical usage and perhaps did not fully understand the direction and force of the original argument. While each of his statements is valid, why this is so is often not clear from Theon's exposition. For example, the particle *ara* usually denotes strict deductive force from the forgoing argument and is translated with *therefore*.[36] For Theon, however, it introduces nearly every statement and rarely indicates strict logical dependence; hence, I have translated with *so*, indicating a merely temporal transition. Another example is Theon's use of *toutesti*, which I have translated with *or rather* in sentence [6]. This expression usually indicates strict equality, or sameness. Theon, however, uses it to indicate a given ratio which is not the same, but which is given for the same reason.

The final feature that marks this passage as non-mathematical is its overall lack of structure. Structure is one of the most conspicuous features of Greek mathematical prose—it is structure that tells us what we are given and what we wish to show or to do, where we are in course of the argument, what we have done and what remains to do.[37] The lack of structure in Theon's account makes it difficult to understand what he thinks he is doing and how he intends to do it. In order to understand Theon's passage, we must read it in the context of coherent works of Greek mathematics, such as Euclid's *Data* and Ptolemy's *Almagest*. These works give us a sense of the meaning of *given* with which Theon is working, and the underlying computational practice, which he ignores.

The opening sentence, [1], states the claim to be shown—namely, that a certain circle is fixed in place and in size. Then, Theon begins his treatment of the problem, in [2], by assuming, without comment, that the center of the sun's eccentric orbit is located at *M* and producing lines through this point parallel to the lines joining the earth with the cardinal points of the ecliptic. In these sentences, he uses the usual expressions of geometric constructions.

In [3], Theon divides a solar year of $365{,}4'^{\mathrm{d}}$ into two parts such that $\mathbf{Arc}(EZH) = 187^{\mathrm{d}}$ and $\mathbf{Arc}(HKE) = 178{,}4'^{\mathrm{d}}$.[38] Theon seems to imply that these latter numbers follow as a matter of course from the year length. In fact, however, they come from season lengths that Hipparchus claims to have observed. According to Ptolemy, in *Almagest* III.4, Hipparchus derived the parameters for his solar model, $e : R = QM : MN$ and $\lambda_A = \mathbf{Ang}(OMN) = \mathbf{Ang}(AQN)$, under the assumption that the interval from the spring equinox to the summer solstice is $94{,}2'^{\mathrm{d}}$ while that from the summer solstice to the autumnal equinox is $92{,}2'^{\mathrm{d}}$. Theon quotes these season lengths a few pages earlier in his treatise (Hiller 1878, 152–154), but the fact that he does not give the spring and summer separately here indicates that he may have been unaware that a division of the year into 187^{d} and $178{,}4'^{\mathrm{d}}$ is insufficient for the determination of the model. Moreover, it is not clear from Theon's presentation how he intends the double season lengths that he asserts to be related to the rest of the argument.

In [4], the geometry of the figure is used to infer that $\mathbf{Arc}(EO) = \mathbf{Arc}(PH)$, so $\mathbf{Arc}(RZ) = \mathbf{Arc}(SK)$, and that $\mathbf{Arc}(SP) = \mathbf{Arc}(PR) = \mathbf{Arc}(RO) = \mathbf{Arc}(OS)$ are each $365, 4'^{\mathrm{d}} \div 4 = 91, 4', 16'^{\mathrm{d}}$, which is again written in the usual form for Egypto-Greek fractions, proper parts. Theon's use of *ara*, however, indicates that he took all this to be implied, somehow, from the double season lengths—which is not the case.

Theon next states, in [5], that $\mathbf{Ang}(OMN)$ is given. This is so because, in the previous discussion, he has remarked that the solar apogee is Gem $5, 2'^{\circ}$, so that $\mathbf{Ang}(AQN) = \mathbf{Ang}(OMN) =$

36 See discussions of *ara* by Mugler (1958, 82–83) and Acerbi (2012, 173–174).

37 For discussions of the importance of structure in Greek prose see Netz (1999a, chaps. 4 and 5) and Acerbi (2011a).

38 I use the following abbreviations: $\mathbf{Arc}(a)$ for arc a, $\mathbf{Ang}(b)$ for angle b, and $\mathbf{T}(c)$ for triangle c.

65; 30°. Hence, all three angles of $\mathbf{T}(MTQ)$ are given in degrees. Again, the lack of structure makes it difficult to see that this is being taken as a given in the argument.

Theon then claims, in [6], that $MT : MQ$ and $MT : TQ$ are both given. From a purely geometric perspective, these would follow as a result of *Data* 40, which shows that a triangle which has three given angles, is *given in form*, and the definition of given in form, *Data* def.3, which states that a figure that is given in form has its angles given, and the ratios of its sides given. Theon will go on, however, in [7], to state that $\mathbf{T}(MTQ)$ is *given in form*, so it is unclear, again, how he understands the progression of the deduction.

Whatever the intended order of the reasoning, *Data* 40 is a purely geometric argument that relies on constructing a similar triangle and gives us no means of stating the ratio of the sides of a triangle as a pair of numbers. That is, *Data* 40 provides no way of treating the ratios of a triangle *given in form* other than by laying out a set of line segments. What is required here, however, is some method, presumably by means of a chord table, of using the values of the angles in $\mathbf{T}(MTQ)$ to derive the ratios of the sides as values, or relations between values. When Theon states, in [7], that $\mathbf{T}(MTQ)$ is *given in form* he means the same thing that Ptolemy would have meant if he had used that expression—that is, its angles and the ratios of its sides are both geometrically contractable and are also expressible by determinate numerical values for the purposes of calculation.[39]

The rest of the passage is again muddled but the overall sense is clear. Sentence [8] asserts that $NQ : XQ$ is given because, as [9] states, $MQ : MN = e : R = 1 : 24$. We can flesh this out by noting that if $MQ : MN$ is given, then *Data* 5 implies that $QX : MN$ is given, while *Data* 6 implies that $NQ : MN$ is given. Finally, by *Data* 8, $NQ : XQ$ is given.[40]

Again, $1 : 24$ is one of the parameters of the Hipparchus' solar model, and Theon is assuming it as given. The position and magnitude of circle *EZHK* is then given in relation to *Q* by the fact that the two ratios $MT : TQ$ and $MQ : MN$ are both given.[41] Because the fact that the ratio $MQ : MN$ is given involves expressing it as a relation of two values, we should understand the fact that $MT : TQ$ is given in the same way.

It is difficult to reconstruct the meaning or purpose of this argument because we do not have any sources that provide us with examples of the pre-Ptolemaic chord-table trigonometric practice that would have been found in Adrastus' sources.[42] In order to understand this passage, it is necessary to read it in the context of the extant work of mathematicians like Euclid and Ptolemy. It is possible that Theon is trying to construct an argument of his own to the effect that taking the numerical parameters of the model as fixed implies that the geometrical model is given—that is, determinate and knowable. Or, more likely, he may be attempting to familiarize students of philosophy with the language of givens used by mathematicians.

39 See Sidoli (2018, 387–391) and Sidoli (2020) for discussions of arguments by givens in Ptolemy's *Almagest* and *Analemma*.

40 That is, $NQ : XQ = 25 : 23$, although these values are not mentioned by either Theon or Ptolemy.

41 Strictly speaking we would probably say that the relative magnitude of the circle is given, but such expressions are not found in ancient sources.

42 The overall incoherence of this passage is further evidence that Theon of Smyrna cannot have been the man that Ptolemy refers to as "Theon the mathematician." This point has already been argued by Martin (1849, 8–10) and Jones (2015, 2016, 468, n. 11; 76, n. 2).

Conclusion

As this reading of these two texts has confirmed, neither Cleomedes' *On the Heavens* nor Theon's *Mathematics Useful for Reading Plato* are treatises of mathematics, although they contain mathematical topics. Neither of these authors had the inclination, or perhaps the competence, to express himself in the manner adopted by the mathematicians. Nevertheless, as treatises about mathematical subjects they are valuable to us in giving evidence for mathematical traditions for which we might otherwise have had little evidence.

Cleomedes' discussion of Eratosthenes' mathematical approach makes it clear that the latter continued the traditions of Aristarchus and Archimedes in his investigations of the size of the earth. That is, he started with hypotheses that involved idealizations of observational claims, some of which gave rise to a geometric model and others of which produced numerical starting points, then he applied elementary geometry, ratio manipulations and computations to produce numerical values, and probably bounds, for something beyond the purview of our senses—namely, the size of the earth. This was probably one of the final chapters in the proto-trigonometric work of the early Hellenistic period.

Theon's discussion of Hipparchus' solar model gives us further evidence for the blending of computational procedures and justificatory practices that we find in the work of Heron and Ptolemy, in the Imperial period. The fact that this language is associated with Hipparchus gives us reason to believe that these kinds of arguments were already being made by Hipparchus and the mathematicians who followed him. Hence, we can take this as one of the opening episodes in the development of the chord-table trigonometry of the late Hellenistic period.

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The origins of the Ṭūsī-couple revisited

F. Jamil Ragep

Among the many contributions by James Evans to the history of astronomy is his clear and elegant paper on the origin of Ptolemy's equant.[1] As has been his hallmark, he there brought his considerable talent as a modern scientist together with his sophisticated historical sensitivity. The result was an important contribution to the vexed problem of the origins of this problematic device.[2]

The equant itself, despite its success in resolving observational issues related to the retrograde arcs of the planets, evoked considerable controversy among Islamic astronomers because of the violations resulting from it of the strictures of uniformity and circularity in the heavens. Among the devices proposed for dealing with these violations was the Ṭūsī-couple, put forth by the famous thirteenth-century astronomer and polymath Naṣīr al-Dīn al-Ṭūsī (1201-1274). Although it has been known for some time that Ṭūsī used the device in his lunar and planetary models found in his *al-Tadhkira fī ʿilm al-hayʾa* (Memoir on the science of astronomy), there has been a divergence of opinion about when Ṭūsī first proposed his new device and models. In this paper, I present new evidence that sheds light on the first appearance of the Ṭūsī-couple.

In an earlier paper,[3] I argued that Naṣīr al-Dīn al-Ṭūsī first *announced* his famous astronomical device, which we now refer to as the Ṭūsī-couple, in a Persian astronomical work entitled the *Risālah-i Muʿīniyya* (The Muʿīniyya treatise, named for one of Ṭūsī's patrons), which was completed in 632/1235.[4] He first *presented* it in the appendix to this work, which is called, among other things, the *Ḥall-i mushkilāt-i Muʿīniyya* and *Dhayl-i Muʿīniyya* (the resolution of difficulties in the *Muʿīniyya;* appendix to the *Muʿīniyya*). I maintained that there were compelling reasons for believing that the *Ḥall* predated a second version of the couple briefly presented in Ṭūsī's *Taḥrīr al-Majisṭī* (Recension of the *Almagest*), which was completed in 644/1247; however, there was still some question since no manuscript had yet been found that gave a date for the *Ḥall*. But thanks to an examination of a manuscript in Tashkent, which was brought to my attention by Sergei Tourkin, we now have a date for the *Ḥall* and therefore for the first publication of the Ṭūsī-couple. This new dating confirms my original chronology, but it also raises some new questions and puzzles, which I discuss in what follows.

Before presenting this new evidence, let me briefly summarize the information we have on the Ṭūsī-couple. The final and most complete presentation of Ṭūsī's models occurs in *al-Tadhkira fī ʿilm al-hay'a*, written in Arabic, which first appeared in 659/1261 when Ṭūsī was the director of the Marāgha observatory that had been established under Mongol patronage in Azerbaijan. Ṭūsī presents them in the context of criticisms of the models that had been developed by Claudius Ptolemy in the 2nd century CE in Alexandria, Egypt, and brought forth in the latter's *Almagest*

1 Evans 1984.

2 For a review of several theories on the origin of the equant, see Duke 2005.

3 Ragep 2000.

4 When separated by a slash, the first date is lunar hijrī; the second is common era. Otherwise the date is common era.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 229–237. Chapter DOI: 10.5281/zenodo.3975745.

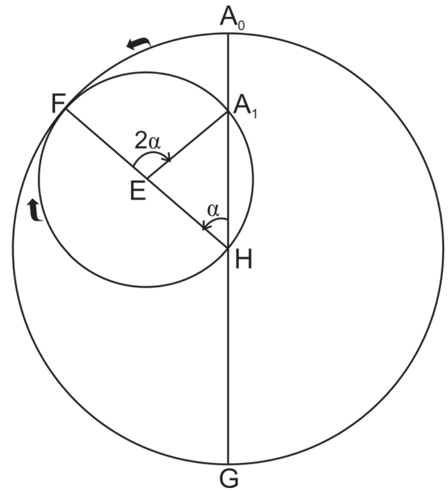

Figure 1. The Rectilinear Version of the Ṭūsī-couple.

and *Planetary Hypotheses*. Following a line of criticism that can be traced at least as far back as Ibn al-Haytham in the 11th century CE, Ṭūsī identifies 16 difficulties, or *ishkālāt,* that taint the Ptolemaic models. Rather than go through these individually, we can instead point to the general problem they highlight, namely that these models did not adhere to the recognized physics that required that all motion in the heavens be uniform and circular, and such that one uniformly rotating motion be brought about by a single spherical body called an orb [*falak*]. The two versions of the Ṭūsī-couple seek to resolve these problems by using a combination of uniformly rotating orbs that can, alternatively, produce either a straight-line oscillation in a plane [Rectilinear Version], or a curvilinear oscillation along a great circle arc [Curvilinear Version]. The Rectilinear Version was used by Ṭūsī to resolve irregular planetary motions in longitude by ingeniously decomposing Ptolemy's deferent (longitudinal) motions into two parts: one based on variable speed with respect to the observer and the other based on distance from the observer, this latter being brought about by the couple. The Curvilinear Version, which first appears in the *Tadhkira*, was used, among other things, to produce latitudinal (north-south) motion by having the couple create curvilinear oscillations by means of physical orbs. These latitudinal motions had been brought about in the *Almagest* by circles, but without an underlying physical explanation. Ṭūsī also notes that Ptolemy's latitude circles cause motions in all directions, whereas what is needed for the latitude models is an oscillation along a great circle arc.[5]

In the *Muʿīniyya*, when noting the irregular motion associated with the lunar epicycle center on its deferent, Ṭusī mentions "an elegant way" (*wajh-i laṭīf*) he has discovered to resolve the issue (Book II, Chap. 5). He refers to this solution at least twice more, when discussing the upper planets and Venus (Book II, Chap. 6) and when setting forth Mercury's configuration (Book II, Chap. 7). As for the models for latitude, Ṭūsī points out that Ibn al-Haytham had dealt with this in a treatise and gives a brief sketch of his theory (Book II, Chap. 8). But he finds this solution lacking and criticizes it without going into details, since "this [work, i.e. the *Muʿīniyya*] is not the place to discuss it." Despite this criticism, Ṭūsī does not claim to have a solution to the problem of latitude, unlike the case with the longitudinal motions of the moon and planets.[6]

5 Extended discussions of the Ṭūsī-couple occur in: Ragep 1987; Ragep 1993, 1.46-53 and 2.427-457; Ragep and Hashemipour 2006; and Ragep 2017.

6 The relevant passages from Book II, Chaps. 5, 6 and 8 of the *Muʿīniyya*, with English translation, can be found in Ragep 2000, 123-125.

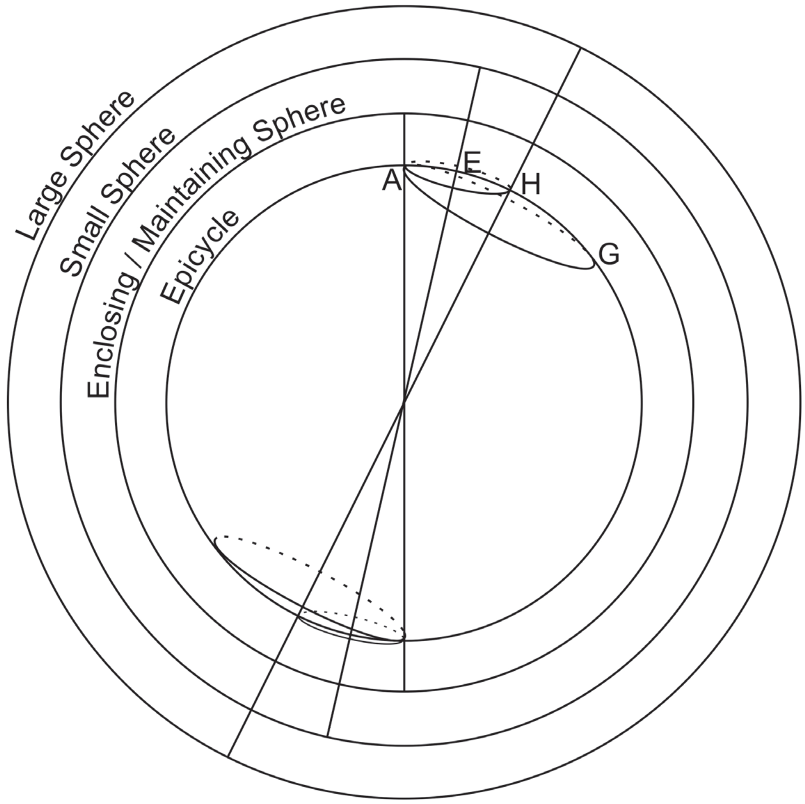

Figure 2. The Curvilinear Version of the Ṭūsī-couple.

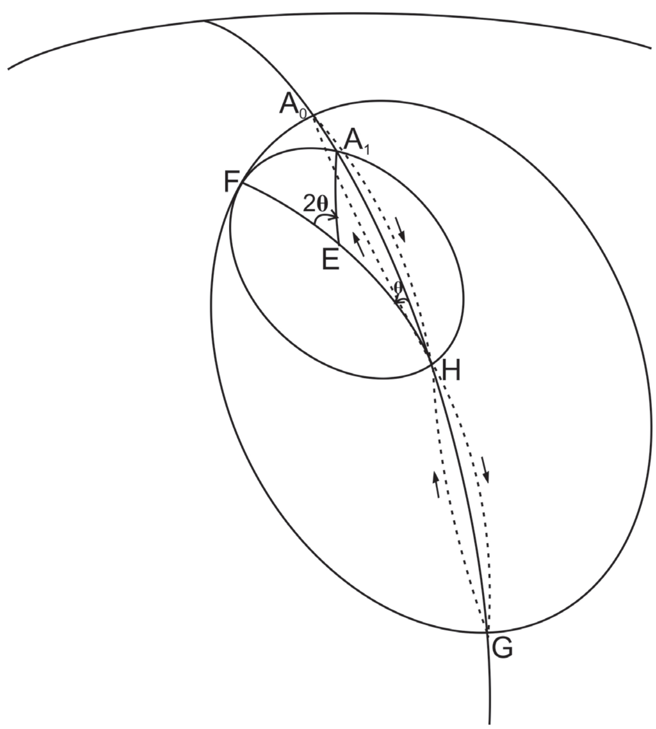

Figure 3. Polar View of the Curvilinear Ṭūsī-couple (dotted line represents actual path of pole A).

Ṭūsī promises to put his solution in a separate work if the "Prince of Iran…would be so pleased to pursue this problem," a reference to Muʿīn al-Dīn Abū al-Shams, the son of his patron Nāṣir al-Dīn Muḥtasham. And indeed, a solution is presented in the *Ḥall-i mushkilāt-i Muʿīniyya*. The *Ḥall* consists of 9 chapters:

Chapter 1: On the possibility of a fixed star whose colatitude is greater than the difference between the local latitude and the total obliquity, after having been either permanently visible or permanently invisible, becoming invisible or visible	فصل ١: در آنکه چون تمام عرض کوکبی از ثوابت زیادت از فضل عرض بلد بر میل کلّی بود ممکن باشد که بعد از آنکه ابدی الظهور یا ابدی الخفا بوده باشد اورا خفائی یا ظهوری حادث شود
Chapter 2: On why the eccentric orb was chosen for the sun over the epicycle	فصل ٢: در آنکه فلك خارج مرکز جهت آفتاب چرا بر تدویر اختیار کرده اند
Chapter 3: On the solution of the difficulty occurring with regard to the motion of the center of the lunar epicycle on the circumference of the deferent, and the uniformity of that motion about the center of the World	فصل ٣: در حلّ شکی که بر حرکت مرکز تدویر ماه بر محیط حامل و تشابه آن حرکت بر حوالی مرکز عالم واردست
Chapter 4: On the explanation of the circuit of the moon's epicycle center and the manner in which the circuit of the center of the lunar epicycle orb comes about	فصل ٤: در شرح مدار مرکز تدویر قمر و چگونگی حدوث مدار مرکز فلك تدویر ماه
Chapter 5: On the configuration of the planets' epicycle orbs according to the doctrine of Abū ʿAlī ibn al-Haytham	فصل ٥: در هیأت افلاك تداویر سیارگان بر مذهب ابو علی بن الهیثم
Chapter 6: On the explanation for finding the stationary positions of the planets on the epicycle orb	فصل ٦: در شرح معرفت مواضع اقامت کواکب از فلك تدویر
Chapter 7: On clarifying the different circumstances of lunar and solar eclipses from the point of view of difference in latitude and other matters	فصل ٧: در بیان تفاوت احوال خسوف وکسوف از جهت تفاوت عرض وغیر آن

Chapter 8: On conceptualizing the equation of time [lit.: equation of days with their nights]	فصل ۸: در تصویر تعدیل الایام بلیالیها
Chapter 9: On depicting the Indian Circle, the direction of a locale and other matters	فصل ۹: در صورت دایرۀ هندی و سمت بلاد وغیر آن

What is striking about the *Ḥall* is the variety of the contents (one might call it a hodgepodge) and the fact that the most innovative part of it, i.e. that devoted to the rectilinear version of the Ṭūsī-couple and its use to resolve the irregular motion of the moon's epicycle on its deferent, is relegated to Chapter 3. Furthermore, the curvilinear version, which is for resolving irregular motion resulting from Ptolemy's latitude theory, is not presented in any way in the *Ḥall*; rather, for the problem of latitude, for which Ṭūsī would later use his curvilinear version in the *Tadhkira*, he simply presents in Chapter 5 the solution that had been proposed by Ibn al-Haytham.[7]

Since it is sometimes referred to as an "Appendix" (*dhayl*), one might assume that the *Ḥall* must have been written soon after the *Muʿīniyya*, especially since there is nothing in it that is particularly new or that had not been promised in the *Muʿīniyya*. Thus it comes as something of a surprise that the *Ḥall* was completed over ten years after the *Muʿīniyya*. The evidence for this comes from a manuscript witness of the *Ḥall* currently housed at the al-Bīrūnī Institute of Oriental Studies in Tashkent, Uzbekistan [MS 8990, f. 46a (original foliation)]:[8]

تمت الرّساله والحمد لله

اتفق فراغ المصنف رفع الله مراتبه فی معارج القدس من تالیفه اوایل جمادی الآخرة سنه ۶۴۳ هجریه بمقام بلدة تون بالبستان المعروف بباغ برکه

> The treatise is completed, praise be to God. The author, may God elevate his stature on the ascents to the Divine, completed its composition during the first part of Jamādā II, 643 of the Hijra, within the town of Tūn in the garden known as Bāgh Barakah. [=late October 1245]

We should note here that Ṭūsī at this time was in the employ of the Ismāʿīlī rulers of Qūhistān in southern Khurāsān. As stated by Farhad Daftary: "The supreme Nezārī [Ismāʿīlī] leader, whether *dāʿī* or imam, selected the local chief *dāʿī*s to serve in the main Nezāri territories: Kūhestān (Qohestān) in southern Khorasan and Syria. The chief *dāʿī* (often called *moḥtašem* [as is the case here]) of the Kūhestān Nezārīs usually lived in Tūn, [in] Qāʾen, or [in] the fortress of Moʾmenābād, near Bīrjand."[9] Tūn, today called Firdaws, lay some 80 km/50 miles west-northwest of the main town of the region, Qāʾin.

7 For an edition, translation and discussion of this part of the *Ḥall*, see Ragep 2004.

8 I thank the Bīrūnī Institute for providing images of this valuable manuscript. On the side of the last page, the text is said to have been collated with a copy that had been collated with a copy in the hand of the author (i.e. Ṭūsī) on 4 Ramaḍān 825/late August 1422 (f. 46a). The page with the colophon and copy date is reproduced in the Appendix below.

9 Daftary 1993, 6.592 (col. 1). I have added a few clarifying remarks between square brackets.

As mentioned, the *Taḥrīr al-Majisṭī* (recension of Ptolemy's *Almagest*), written in Arabic, was completed on 5 Shawwāl 644/ 13 February 1247 and thus after the *Ḥall-i mushkilāt-i Muʿīniyya*. I have argued elsewhere that it is likely that Ṭūsī, for some reason, perhaps related to a falling out with his patrons in Qūhistān, relocated (or was relocated) to the Ismāʿīlī fortress of Alamūt in north-central Iran sometime before Ṣafar 644/June-July 1246. This was the date of the *Ḥall mushkilāt "al-Ishārāt"*, his commentary on Ibn Sīnā's philosophical treatise *al-Ishārāt wa-al-tanbīhāt*. Ṭūsī's work was dedicated to Shihāb al-Dīn Muḥtasham, who was most likely in Alamūt, thus providing us a probable location for Ṭūsī's residence at the time. Now that we know the date of the *Ḥall-i mushkilāt-i Muʿīniyya*, we can say with some degree of certainty that Ṭūsī's move to Alamūt occurred between Jamādā II 643 and Shawwāl 644, since the *Taḥrīr al-Majisṭī*, a major work of considerable consequence, is not dedicated to any of the Ismāʿīlī rulers.[10] The date of the move is further confirmed by the fact that Ṭūsī, after completing the *Ḥall-i mushkilāt-i Muʿīniyya,* no longer dedicated his works to anyone at the court in Qūhistān.[11]

There is another interesting aspect to Ṭūsī's writings after the move to Alamūt. The vast majority of Ṭūsī's works (but not all) appear now in Arabic. And we can perhaps better understand the context of his writing the *Taḥrīr al-Majisṭī*. It was the first of Ṭūsī's recensions; these would eventually include the Middle Books (*Mutawassiṭāt*, to be studied between the *Elements* and the *Almagest*), which were completed in 663/1265, as well as the recension of Euclid's *Elements*, completed in 646/1248. We can only speculate about Ṭūsī's motives for this monumental project, but it most likely involved both retrospective and prospective aspects: retrospective because of the desire to preserve the great mathematical and astronomical works of Hellenistic and early Islamic science, especially in the wake of the Mongol invasions; prospective because of the pedagogical importance of these works. Given the tumultuous times in which Ṭūsī lived, and the real danger that the great achievements of Islamic science might be lost, the recension projects can be understood as making available a body of textbooks, with commentary, that could provide both a record and a pedagogical tool even if the institutions of Islamic science were destroyed.

Now that the chronology between the *Muʿīniyya*, its *Ḥall*, the *Taḥrīr al-Majisṭī,* and *al-Tadhkira fī ʿilm al-hayʾa* has been firmly established, we can make the following observations:

1) Ṭūsī's claim to having discovered an "elegant way" (*wajh-i laṭīf*) in the *Muʿīniyya* for resolving some of the problems of Ptolemaic planetary theory would seem to have been somewhat premature. That he waited over ten years to present this new model, and because none of the other material in the *Ḥall* is particularly new or creative, leads one to conclude that he had not finalized his model when he made his claim in the *Muʿīniyya*. Another bit of supporting evidence is that in the *Muʿīniyya* (II.7), Ṭūsī claimed that the solution for Mercury "is as for the other planets," something that he later contradicted in the *Tadhkira* (II.11[11]), where he admits to not having a solution for Mercury's complex model.

2) Another surprising point is that despite the many years between the *Muʿīniyya* and the *Ḥall,* the lunar model based on the Ṭūsī-couple has a mistake in it. In listing the orbs (*aflāk*) of the moon and their motions, Ṭūsī gave the wrong daily motion for the second (inclined) orb (13°11' instead of 13°14'). At some point he must have realized the error and corrected it in the *Tadhkira*, while at the same time dividing up the inclined orb of the *Ḥall* into an inclined and a deferent orb.[12]

10 The simple dedication is to a certain Ḥusām al-Dīn Ḥasan b. Muḥammad al-Sīwāsī.

11 For an elaboration of the points in this paragraph, see Ragep 1993, 1.9-13.

12 In the *Tadhkira,* the sum of the lunar inclined and deferent orbs comes to 13°14' (24°23'/day – 11°9'/day); cf. the *Ḥall*, where the equivalent motion of the inclined orb is given as the mean motion of the moon (*wasaṭ-i qamar*),

3) The criticism of Ibn al-Haytham's latitude model that Ṭūsī gave in the *Muʿīniyya* is not repeated in the *Ḥall.* Instead he presents Ibn al-Haytham's model without commentary. This seems another indication that in writing the *Ḥall* he still had not come up with the second, curvilinear version of his device.

4) The model for latitude that Ṭūsī describes in the *Taḥrīr al-Majisṭī* is schematic at best. In fact, it is a rather simplistic adaptation of the rectilinear Ṭūsī-couple and very different from the curvilinear version given in the *Tadhkira*, which Ṭūsī presented as an adaptation of Ibn al-Haytham's model.[13]

From this we can conclude that the Ṭūsī-couple, and its applications to various planetary models, emerged in stages and rather slowly. After coming up with the idea, apparently when writing the *Muʿīniyya*, it took many years before he felt comfortable enough to present it in the *Ḥall.* And at the time of writing the *Ḥall,* he still had not come up with the curvilinear version. A year later he tentatively put forth a kind of adaptation of the rectilinear version for a latitude model, but it was completely unsatisfactory since it produced straight-line motion, not the needed curvilinear oscillation along a great circle arc. Fifteen years later, he would bring forth both versions in their final form in his Arabic adaptation of the Persian *Muʿīniyya,* namely *al-Tadhkira fī ʿilm al-hayʾa*.

i.e. 13°11' (Naṣīr al-Dīn al-Ṭūsī 1335 H. Sh./1956-7 CE, f. 11). It is of great historical interest that it is the *Ḥall* version of Ṭūsī's lunar model that makes it into the Byzantine Greek work of Gregory Chioniades (d. ca. 1320) entitled the *Schemata of the Stars,* which would be available in Italy by the fifteenth century at the latest; see Ragep 2014, 242. For a listing of the parameters for the lunar model in the *Tadhkira*, see Ragep 1993, 2.457; a comparison of parameters between the *Tadhkira* and *Ḥall* can be found in Ragep 2017, 167.

13 Naṣīr al-Dīn al-Ṭūsī, *Taḥrīr al-Majisṭī*, Istanbul, Feyzullah MS 1360, ff. 199b-202a. This assessment of the model in the *Taḥrīr al-Majisṭī,* as well as the chronology of the development of the two versions of the Ṭūsī-couple, would tend to undermine the conclusions reached by G. Saliba 1987. A translation, edition, and analysis of the relevant parts of the *Taḥrīr* can be found in Ragep 2017, 168-171 and endnote 15. The *Taḥrīr* version appears in various European contexts, including Copernicus's *De revolutionibus*, for which see Ragep 2017, 182-184.

Appendix

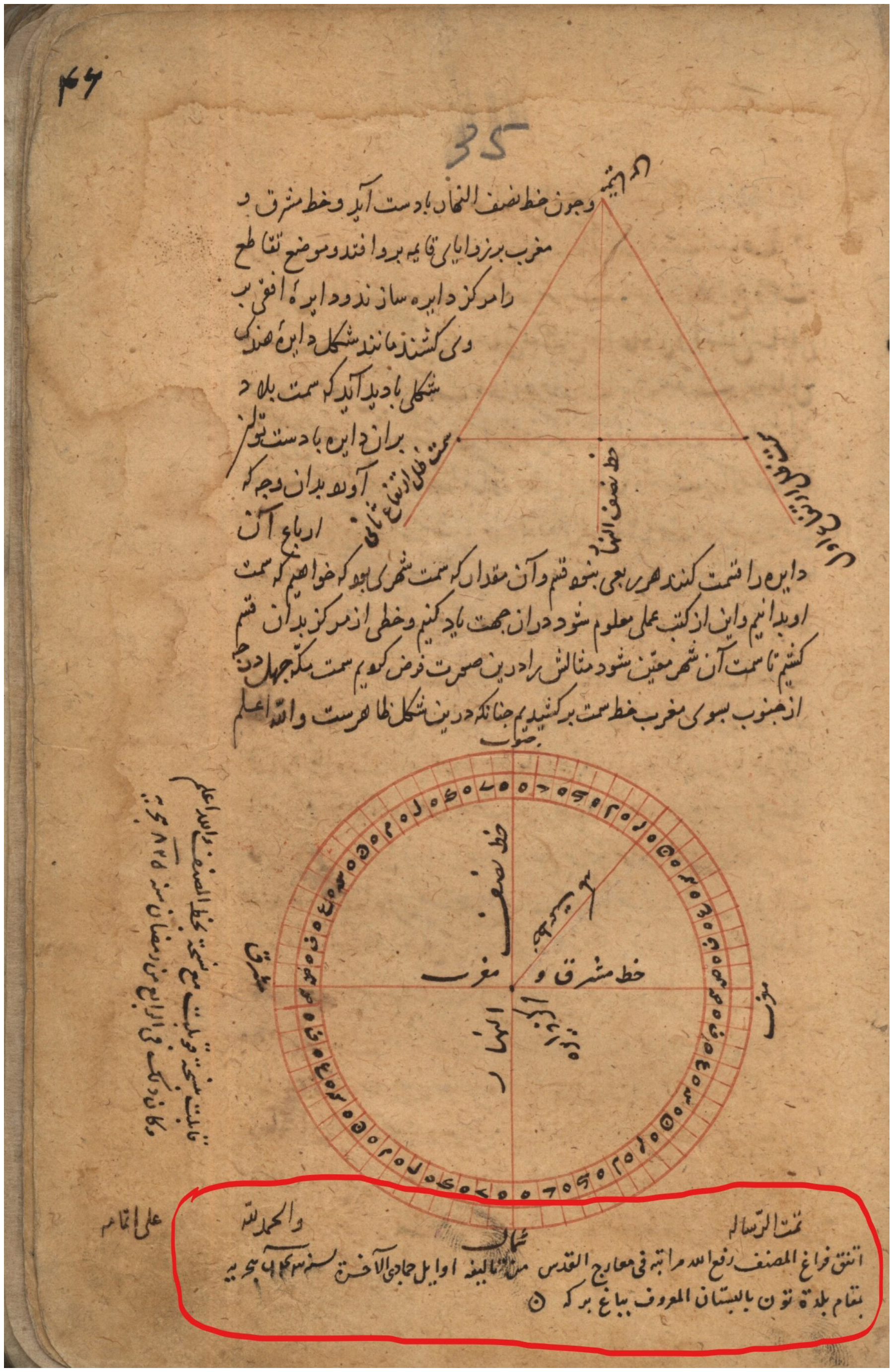

Figure 4. Colophon (boxed in red by current author) of *Ḥall-i mushkilāt-i Muʿīniyya,* Tashkent, al-Bīrūnī Institute of Oriental Studies, MS 8990, f. 46a (original foliation). Courtesy of the Institute.

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What every young astronomer needs to know about spherical astronomy: Jābir ibn Aflaḥ's "Preliminaries" to his *Improvement of the Almagest*

J. L. Berggren[1]

Abū Muḥammad Jābir b. Aflaḥ is believed to have worked in Seville in the first half of the 12th century during the reign of the Almoravids. His best-known work is his astronomical treatise, the "Improvement of the *Almagest*" (*Iṣlaḥ al-majisṭī*). It was the first serious, technical improvement of Ptolemy's work to be written in the Islamic west and it reveals Jābir as an accomplished theoretical astronomer, one devoted to logical exposition of the topic.

This paper focuses on the First Discourse of Jābir's work, which establishes the mathematical preliminaries needed for the study of astronomy based on the geometric models of heavenly bodies (Sun, Moon, planets and stars) revolving around a spherical Earth.

Early in his *Improvement*, Jābir states that one of his goals in writing the book is to write an astronomical work that would—apart from reliance on Euclid's *Elements*—stand on its own as regards the necessary mathematics. He specifically mentions that he has obviated the need for the works of the ancient writers, Theodosius and Menelaus. In the case of Theodosius, Jābir meant that the reader would not have to refer to the Greek author's *Spherics* since Jābir has selected from Books I and II of that work statements and proofs of theorems necessary for an astronomer. However, he did assume that his reader would have some basic knowledge of spherics[2] since such basic results as *Sph.* I, 12. ("Great circles on a sphere bisect each other.") were taken for granted. He also ignored propositions in *Spherics* that deal with geometrical constructions (e.g. to construct the diameter of a given sphere or to construct a circle through two given points on a sphere), which would not be useful to an astronomer.

In the case of Menelaus, the result in his *Spherics* that Arabic writers called the Sector Theorem (also known as the Transversal Theorem) was another result known to all who studied astronomy; Jābir, however, does not mention the work and replaced it with the spherical trigonometry developed in Iraq and Iran in the last half of the 10th century.[3]

One possible source for Jābir's knowledge of the results which had been discovered in Irāq and Irān in the latter half of the tenth century and which included the Sine Theorem for spherical triangles was the work of an older contemporary of Jābir, the *qāḍī* (religious judge) Ibn ʻAbdullah Muḥammad ibn Muʻādh of the Spanish province of Jaen. In fact the latter's *Book of Unknowns of*

1 The author thanks Professors José Bellver for sending him scans of the First Discourse of the *Improvement* as found in Escorial 910. He also thanks Professors Bellver, J. Hogendijk, R. Lorch and J. Samsó for aid in understanding a number of passages in the Arabic text. The author especially acknowledges his debt to the valuable study of the work in R. Lorch, "Jābir b. Aflāḥ and the Establishment of Trigonometry in the West." Items VI–VIII in Lorch 1995.

2 This was not an unreasonable assumption. Jābir specifically mentions Euclid as an author he assumes his readers know and Book 11 of the Elements is devoted to solid geometry. Indeed, since anyone in Jābir's time who was reading a treatise dealing with the *Almagest* would almost certainly have gone through a collection of books known as the Middle Books which included works on spherics by Euclid and Autolycus and possibly Menelaus.

3 For an account of this see Van Brummelen 2009, pp. 179–192.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 239–259. Chapter DOI: 10.5281/zenodo.3975747.

Arcs of the Sphere was the first work to treat spherical astronomy in a purely mathematical fashion with no mention of astronomy, rather as Theodosius had done for spherical geometry. Lorch 1995 (Item VIII), however, points out that Jābir's treatise has "surprisingly little in common" with that of the *qāḍī* (and, in any case, Jābir never mentions Ibn Muʿaādh) .

By the 13th century Jābir's *Improvement* was circulating in both the western and eastern parts of the Islamic world. It was translated into both Hebrew and Latin and had a considerable impact on the development of spherical trigonometry and astronomy in Western Europe.[4]

In this paper we shall survey the contents of the First Discourse of the *Improvement* and provide translations of some of its parts, a number of which have not, to our knowledge, been translated elsewhere.[5] Our translation, which is indented, is from folios 4a – 16a of the Arabic text of the treatise contained in Escorial MS 910, and references of the form /X,Y/ refer to line Y on folio X of that work and, when the context is clear, /Y/, alone, refers to line Y on the last folio previously mentioned. Brackets < > enclose our explanatory material. References to the "Arabic version" refer to the Arabic version of Theodosius's *Spherics* (Kunitzsch and Lorch 2010). That important work also points out the major differences between the Arabic version and the Greek versions as published in Cziczenheim 2000.

> [4a, 12] First Discourse – Preliminaries
>
> Let us begin with an explanation of the terms used. First: The **pole** of the circle drawn on the surface (*ẓahr*[6]) of the sphere is the point of the surface (*basīṭ*) of the sphere such that all lines[7] produced from it to the circumference of the circle are equal to one other.[8] And the **great circle** /15/drawn on the sphere is the circle whose center is the center of the sphere, and it divides the sphere into two halves. And, among the angles bounded by the arcs of great circles,[9] a **right angle** is one that, if we make its vertex a pole and make a circle with any distance [about that vertex] then the arc of the circle that is cut off between the two sides [of the angle] is a quarter of that circle and this arc is called the arc of the angle if it is part of a great circle.[10] And if that arc is greater than a quarter circle then that angle is obtuse, and if it is less then it is acute /20/ and this arc is called the arc of the angle if it is from a great circle.[11]

4 The Latin translation was done by Gerhard of Cremona, who referred to the author as 'Geber.'

5 Prof. Lorch, in his study mentioned above, gives a valuable, brief account of some of the important contents of this part of the *Islaḥ*. And see (Berggren 2006, pp. 539–544) for a survey of the mathematics and translations of some of its key results.

6 The Arabic edition of the *Spherics* uses *saṭḥ* for the surface of a sphere.

7 The Arabic here is *khuṭūṭ*, which could refer to straight lines or arcs of circles. In the *Spherics* "straight lines" are specified.

8 Prior to this point in his text Theodosius has defined "sphere," "center of the sphere" and "axis" of rotation of the sphere. Jābir assume his reader does not need to have these defined.

9 Theodosius gives a careful explanation of the general notion of angles between great circles on a sphere based on the idea of inclination of planes, which is also found in El. XI, Def. 6 and 7. (Throughout this paper the abbreviation "*El.*" refers to Euclid's *Elements*.)

10 A marginal note adds "And the perpendiculars produced from [a point in] their common section in the planes of each of them contain a right angle." (This is, in fact, a consequence of *El.* XI, Definitions 3 and 4.)

11 Jābir made the same remark for the special case of a right angle a few lines earlier.

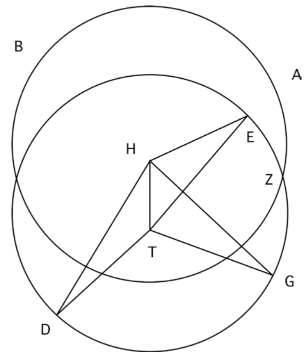

Figure 1. Proposition 1.

And the **Sine** of an arc[12] is half the chord of its double, and it is also the perpendicular falling from an extremity [or the arc] onto the diameter produced from the second extremity.[13]

And the **complement** of an arc is the difference between it and a quarter circle,[14] and similarly the complement of an angle is the difference between it and a right angle, whether the angle is less than a right angle or greater. And of two angles, which, together, are equal to two right angles, each is called **supplementary**.[15] And they are those the sum of whose arcs is equal to a semicircle, and the Sines of their two arcs are one and the same line.[16] Similarly, of two arcs that, together, are a semicircle, each of them is called supplementary. [4b, 1]

Jābir now states and proves eleven theorems from Books I and II of Theodosius's *Spherics*. Since it is clear from the introduction of his *Almagest* that Ptolemy assumes that his reader knows the basics of this subject Jābir's inclusion of theorems from Theodosius's work is very much in order. The fact that he gives not only the theorems he needs but their proofs seems to indicate that the astronomer should know not only what is true but why it is.

We have translated the theorems but the proofs only in those cases where Jābir's proof is significantly different from that in the Arabic version. Footnotes note minor differences.

Theorems from Spherics, Book I

[4b.1][Proposition 1. (= *Sph.* I, 1)] If a plane [*saṭḥ*] cuts a sphere the section common to that plane and the surface [*basīṭ*] of the sphere is the circumference of a circle.[17]

Jābir's proof (Fig. 1) differs from the Arabic version in a number of respects: The diagram in the Arabic has only one circle, representing the plane section of the sphere, but Jābir adds a second

12 The material from here to the beginning of the theorems from Theodosius is not in the *Spherics*.

13 We have capitalized "sine" to remind the reader that here, as in all medieval literature, the sine of an arc is a geometrical object, a line segment, not a ratio. For this reason it is usually capitalized in modern translations.

14 A marginal note adds, "If the arc is smaller or larger than a quarter of a circle."

15 The Arabic word here means 'joined.'

16 The medieval Sine of an angle was a line segment (half the chord of double the angle in a reference circle). The supplementary arcs play an important role in Jābir's discussion of spherical triangles.

17 Unlike its modern definition as "a plane curve" the circle in the ancient and medieval world was, as Euclid put it, "a plane figure contained by one line...."

circle intersecting that and representing the sphere itself; The Arabic proof begins with the easy case, when the section of the sphere contains the center of the sphere, a case Jābir ignores; Although Jābir and the Arabic version then both drop a perpendicular from the center of the sphere onto the plane section, the Arabic then requires that one join the center of the sphere to two points chosen at random on the periphery of the section. Jābir requires that for three points. Both versions then require that one join the foot of the perpendicular to the points chosen on the periphery.

These differences in the diagram continue in the body of the proof itself. The Arabic version uses *El.* I, 47 (not cited) to conclude that the squares on the lines joining the foot of the perpendicular to the two points chosen on the circumference are equal. Hence the two lines are equal. Jābir neither mentions squares nor that his three right triangles have not only equal hypotenuses but a common side as well. Presumably he thought it was obvious from this that the triangles are congruent and hence the third sides must be equal as well.

A consequence of the proof of this proposition is that the perpendicular from the center of the sphere to the plane of a small circle passes through the center of that circle.

The following, Proposition 2, asserts the equivalence of three conditions relative to a small circle in a sphere and a diameter of the sphere: (1) A line joining the center of a circle in a sphere to the center of the sphere is perpendicular to the circle, (2), Extended in both directions that line passes through the poles of the circle and conversely, and (3) a line passing through the poles of a circle in the sphere passes through its center and that of the sphere.

These statements are parts of the contents of *Sph.* I, 7 – 11 Jābir proves them with reference to Fig. 2, where E is the center of the circle, Z the center of the sphere, and points T and H are the poles of circle ABGD.

> [4b, 13] [Proposition 2. (= *Sph.* I, 7 & 8)] <i> If there is on a sphere a circle that is not a great circle[18] and we join its center with the center of the sphere by a line then it <the line> is perpendicular to the plane of the circle. <ii> And if it is extended in both directions it will pass through the two poles.
>
> And conversely, <iii> if a line is produced from the center of the sphere perpendicular to the plane of a circle then it will pass through its center and, if it is produced in both directions, it passes through its poles.
>
> And <iv> if a line passes through its two poles then it passes through its center and the center of the sphere.

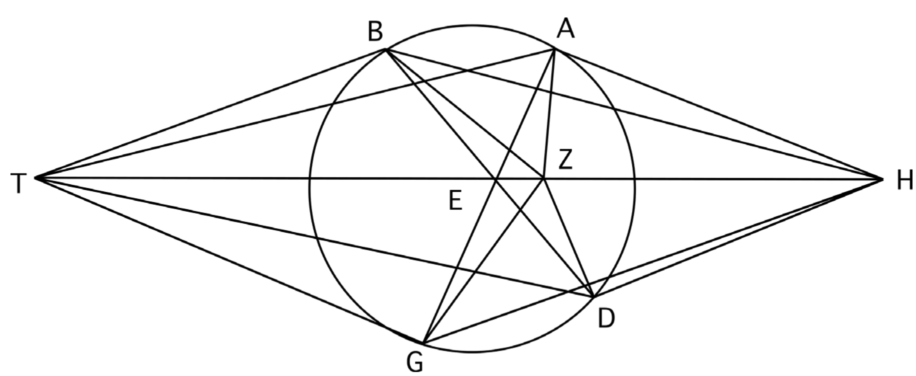

Figure 2. Proposition 2.

18 The condition "that is not a great circle" is missing in the Greek and Arabic versions. Its inclusion here is testimony to Jābir's pedagogical intentions.

Jābir's proof of i: With reference to Figure 2 Jābir marks two arbitrary points A, B on the circumference of the given circle and joins them to its center, E. He then extends these radii to meet the circle again at G and D. And, since E is the center, the radii EA, EB, etc are all equal. He than joins A, B, G and D to Z, the center of the sphere. Since Z is the center of the sphere the lines AZ, BZ, etc are also equal. And since the line joining the centers, EZ, is common <to all four triangles> angles AEZ and GEZ, are equal (*El.* I, 8). Hence (*El.* 1, Def. 10) they are right angles right angles. Similarly angles BEZ and DEZ are right. Jābir then concludes that EZ is perpendicular to the plane of circle ABGD (*El.* XI, 4).In the Arabic version lines AZ, BZ, GZ and DZ neither appear nor are needed.

For whatever reason, Jābir took A and B as arbitrary points and then showed that all four angles formed with EZ by the two diameters going through A and B are right. (Perhaps he felt that the student might not immediately grasp the importance of the fact that BD was an arbitrary diameter of the circle.)

Jābir's proof of ii: He extends line EZ in both directions tot meet the sphere at H and T and join both of these with points A, B, G and D with lines HA, HB,... and TA, TB.... Because E is the center of circle ABGD the four lines AE, BE,... are equal. Since EZ is perpendicular to the plane of the circle ABGD the four angles AEH, ... DEH are right. And AH is common to all four triangles AEH, ... , BEH so the lines AH, ... , DH are equal, and therefore H is a pole of circle ABGD. A similar argument shows T is a pole.

Apart from Z being labeled D, H labeled Z, T labeled H and D being labeled T the proof in the Arabic version is the same.

<iii> and <iv) The last part of <iii> is, of course, immediate from the first part, whose proof, Jābir says, may be obtained from the last part of <ii>. As for <iv> Jābir does not prove it, although <iv> (together with the statement that the line joining the poles is perpendicular to the circle) is carefully proved as Theorem 11 of the Arabic version.

> [5a, 7] [Proposition 3 (= *Sph.* I, 15/Arabic I,16[19])] If a great circle passes through the pole of a circle on a sphere[20] then it divides it into two halves and it stands on the surface [of the circle] at right angles.[21] (Fig. 3)

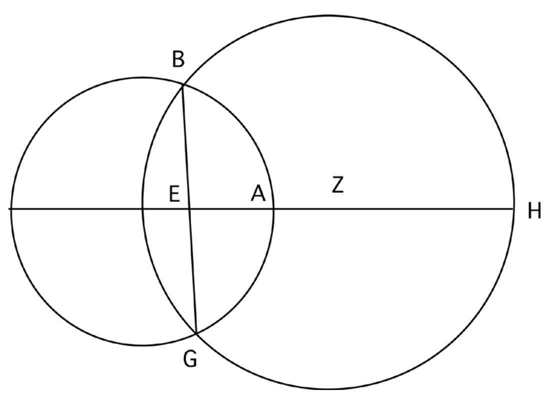

Figure 3. Proposition 3.

19 Theorem I, 9 of the Arabic text is not found in the Greek, so from I, 10 onward the Arabic numbers are one greater than the Greek.

20 The Arabic version changes this, unnecessarily to 'If a great circle in a sphere cuts one of the circles in the sphere and passes through its poles...." Jābir agrees with the Greek version.

21 The Arabic version has 'and cuts it at right angles,' although it often uses Jābir's phrase as well.

The main difference between Jābir's proof (which identifies the circle as ABG and that in the Arabic version, which identifies it as ABGD) is that whereas the latter begins by joining the poles with a straight line, Jābir's proof produces that line by joining the center of the sphere to the center of the circle, which line – by previous results – when extended in both directions passes through the poles. Both make implicit reference to *El.* XI, 18 when they argue that the great circle is perpendicular to the given circle because it contains a perpendicular to that circle. But, the Arabic version then obtains the result that the great circle bisects the given circle by Arabic I, 14, which says that when a great circle on the sphere is perpendicular to a given circle it bisects it.[22] Jābir, by introducing the line connecting the poles as a line going through the centers of the sphere and the circle knows immediately that the great circle passes through the center of the circle and, so, being a diameter, must bisect it.[23]

> [5a, 20] [Proposition 4 (= *Sph.* I, 13/Arabic I,14)] And, analogously, we will show the converse, that the surface of any great circle perpendicular to the surface of circle ABG passes through its poles.

In the Arabic version the conclusion reads "divides it into halves and passes through its poles."

> The proof is that if we produce from the center of the sphere a perpendicular to the common section of the great circle and the circle ABG, i.e. to the line BG, and it is the line ZE, it will be perpendicular to the plane of circle ABG,[24] and so it will pass through its center. And if it is extended in both directions, so that it meets the surface of the sphere, then it passes through its two poles, as we proved. And this line, EZ, is in the plane of the great circle.

Propositions from Spherics, II

In the Arabic version of the *Spherics* the following proposition is stated without reference to the converse or the statement dealing with the case when it is known only that a great circle passes through the poles of one of the circles.

> [5b, 2][Proposition 5 (=*Sph.* II, 9)] If two circles intersect each other on a sphere and a great circle passes through their poles then it divides the intersecting arcs of those two circles into halves. And the converse is also true. And, like that, if it [the great circle] passes through the pole of one of the two and it cuts the second into two halves then it also passes through its pole.

Jābir's proof uses the same basic idea as that in the Arabic version but has some variations. It was certainly not copied from the Arabic, so we reproduce it here although it is not so clear as the Arabic.

---

22 As proof of this latter statement Proposition 14 offers no more than the fact that the figure in question is a semicircle.

23 That a diameter of a circle bisects it is stated in the definition of "diameter" in *El.* I, Def. 17, although as Heath 1956, Vol. I, p. 186 points out (citing Simson) it may be proved from propositions in *El.* III.

24 The text in the Arabic version, for justification, argues that since the great circle and the circle are at right angles to each other and from a point in the great circle a line is constructed in its plane perpendicular to the common section it follows that that line is perpendicular to the circle. This is no doubt true, but the editors in the mathematical notes refer to *El.* XI, 4, which deals with a different situation.

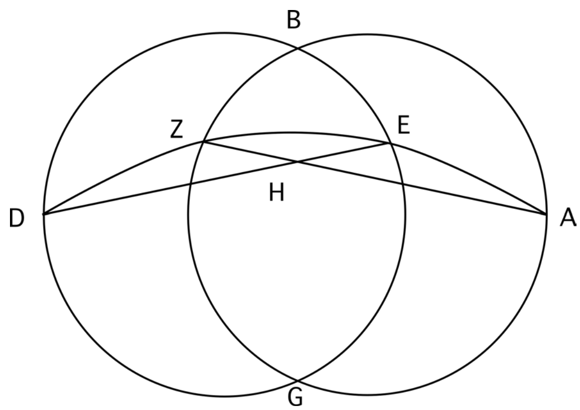

Figure 4. Proposition 5.

So, let the two circles AGB and /5/ GDB intersect each other on a sphere, and let the great circle AEZD pass through their poles. I say it divides arcs BAG, BZG, BEG[25] and BDG into two halves.

Its Proof (Fig. 4): We make line BG the common section between the two circles ABG, BDG and line AZ the common section between the great circle and circle ABG and the common section between it and circle BDG and line ED.

And so, because the great circle AEZD passes through the two poles of the two circles it will pass through their centers and the center of the sphere. And it will divide /10/ the common section between the two circles, the line BG, at a point H in the surface of the great circle,

And because the point H is on the line BG, which is the common section to the two circles ABG, GBD, it is in each of their two surfaces. And so the point H is on the intersection of the two common sections between the two of them and the great circle.

And because of that the intersection of the two lines AZ, ED is necessarily at point H of the line BG.

Also, because the great circle passes through the poles of the two circles, each of the two of them will stand on it at a right angle, and it cuts /15/ each of them into halves [*Sph.* I, 6].

And [so] their common section, line BG, is perpendicular to the great circle, [*El.* XI, 19]. And so it is perpendicular to each of the two lines AZ, ED, which are diameters of the two circles.

And so, for that reason, arc AB is equal to arc AG and, similarly, arc GD is like arc BA and arc GE is like arc BE and arc GZ is like arc ZB,[26] which is what we wanted /20/ to prove.

And the converse of that is proved by reversing the argument and by an easy demonstration (*bi-bayīnat qarīb*). And like that it will also be clear that if the great circle passes through the pole of one of the two [circles] and divides the arc of the other circle into halves then it passes through its two poles also. The proof is finished.[27]

The following theorem is the first reference to parallelism of circles. Its first statement appears as Theorem II, 2 in the Arabic, and its second as Theorem II, 1. However, Jābir begins with the converse and states both parts before beginning the proof of either.

25 Text has "H"

26 One notes Jābir's use of two quite different words in this argument for the same concept: *musāwī* (equal) and *mithl* (like).

27 This statement of the converse is missing in the Arabic version.

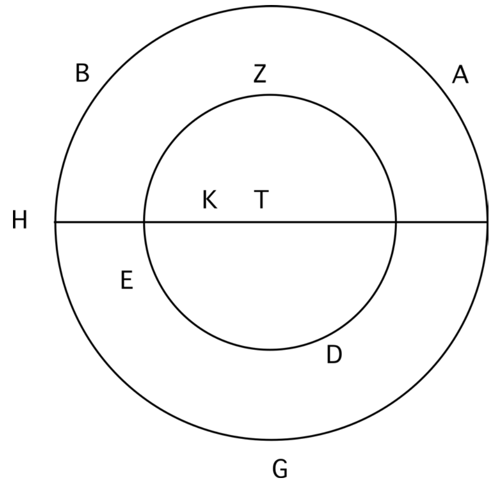

Figure 5. Proposition 6.

[5b, 24] [Proposition 6 (=*Sph.* II, 2)] Circles drawn around a single pole are parallel to each other. And if circles are parallel they are drawn around a single pole.

Proof (Fig. 5): We join the center of the sphere, point T, with the pole of the two circles, point H, with the line HT, and it will pass through the centers of the two circles and be perpendicular to their surfaces. And if a single line is perpendicular to two [plane] surfaces they are mutually parallel [*El.* XI, 14]. And so the surfaces of the two circles are parallel. End of proof.

And [*Sph.* II, 1 and 2] if the two circles are parallel then their poles are one and the same.

Proof: We join the center of the sphere, point T, with point K, the center of circle ABG,[28] and we extend it to the surface of the second circle and to the surface [*basīṭ*] of the sphere,[29] point H. And line TK is perpendicular to both surfaces.[30] And so it is perpendicular to the surface of circle DEZ, so it will pass through its center.

And since a line passes through the center of the sphere and the center of a circle drawn on the sphere it will also pass through its poles. And so line TK passes through the pole of the circle DEZ. But it also passes through the pole of circle ABG, and so its pole is one point, and that is point H.

End of proof.

Jābir, having now shown that circles being parallel is equivalent to their having the same poles, now has what he needs to prove the next theorem.

[6a, 12] [Proposition 7 (=*Sph.* II, 10[31])] Great circles passing through the two poles of parallel circles cut off similar arcs from them in the surface between them.

28 The proof in the *Spherics* begins with a line passing through the poles of circle ABG, and the diagram there does not indicate the center of the sphere.

29 This extension to the sphere is clearly indicated in the Greek and Arabic diagrams, but in Jābir the extension stops at the larger circle. And, indeed, Jābir calls H, the point of intersection of the line joining the center of the sphere and the pole with the larger circle, “the pole.”

30 Any line from the center of the sphere to the center of a circle on that sphere is perpendicular to the circle. So TK is perpendicular to circle ABG. And because the two circles are parallel TK is also perpendicular to DEZ And so it passes through its center,

31 II, 10, in both the Greek and Arabic, also adds that the arcs of the great circles between the two parallels are equal.

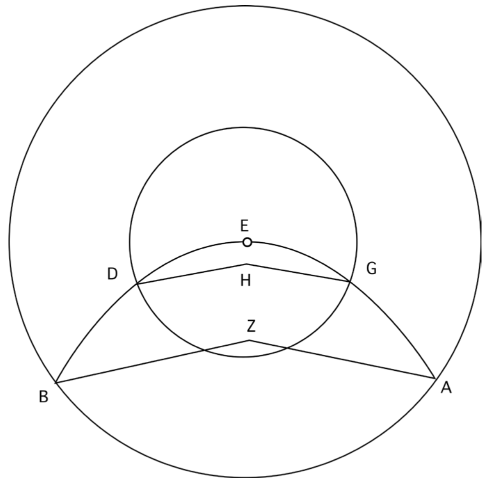

Figure 6. Proposition 7.

Jābir's proof (Fig. 6) has some gaps, which the longer version in the *Spherics* supplies with a step-by-step construction of the centers of the parallel circles.

Let there be on a sphere two parallel circles, AB and GD, and through their two poles, the point E, let there pass two great circles, AGE and BDE. Then I say that the arcs AB, GD of the two parallel circles are similar.

Proof: We make the center of [circle] AB the point Z and the center of circle GD the point H, and we join point Z with the two points A, B by the two lines AZ, BZ. And we also join point H with the two points G, D with the lines GH, DH. And because the two circles AB, GD are parallel, and the two circles AGE and BDE cut them, the common sections are parallel [*El.* XI,16[32]] and so line AZ is parallel to line GH and likewise, BZ is parallel to DH. So [*El.* XI, 10] angle AZB is equal to angle GHD, and, so, arc AB is similar to arc GD, which is what we wanted to prove.

[6a, 24] [Proposition 8 (= part of *Sph.* II, 19, Arabic II, 18)[33]] Let there be on a sphere two parallel and equal circles ABGD, EZHT and let a great circle AQSE that does not pass through their poles cut them. (Fig. 7)

Let the two common sections with the two circles ABGD and EZHT be the two lines AG, EH. /27/ Then I say that it [the great circle] cuts each one of them [the parallel circles] into unequal sections and that their alternate segments /6b, 1/ are equal. I mean that segment ABG is equal to segment ETH and the segment ADG is equal /2/ to segment EZH. And the great circle parallel to the two circles ABGD, EZHT in what is between them cuts arcs AE and GH from circle AQSE into halves.

Proposition 8 is proved very differently in *Sph.* II, 19 by use of *Sph.* II, 18, which says that if a great circle cuts equal and parallel circles, then the arcs of that great circle between the great circle parallel to the two parallel circles and the two parallel circles are equal.

32 Because El. XI, 16 requires that the lines GH and EZ lie in the same plane, but for that Jābir needs to show – as the *Spherics* does – that H lies in the plane of the great circle EA. Similarly for DH and BZ.

33 The Greek and Arabic version, however, state the theorem for an arbitrary number of parallel circles and state that, of their segments cut off in one of the hemispheres formed by the greatest of the parallels, those between the greatest of the parallels and the visible pole are larger than semicircles and the remaining [between the greatest of the parallels and the hidden pole] are smaller.

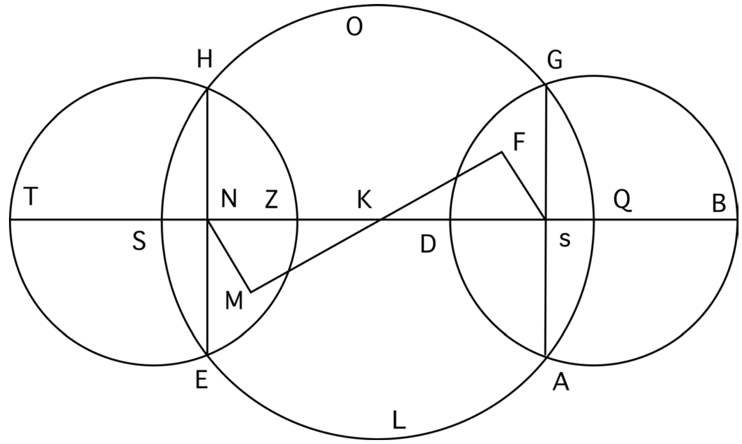

Figure 7. Proposition 8.

The following is Jābir's proof of the result.

Proof: /4/ We make the common section between the great circle AQSE and the great circle that passes over its /5/ two poles and the two poles of the parallel circles the line QsN and the common section between it [AQSE] and the greatest of the parallel circles the line LKO. Let K be the center of the sphere and the centers of /7/ the two parallel circles the points F and M. And let us join them with the center of the sphere by the two lines FK, MK. And we join the two lines, Fs and MN.

Because the centers of the two parallel circles ABG, EZH are joined with the center of the sphere by the two lines KF and KM, each of them is perpendicular to the surface of the two circles. And because the two circles are parallel, the two lines FK and KM form a continuous straight line. And because the two circles are equal to each other the two lines FK and MK that join their centers and the center of the sphere are equal. And each of the two angles, F and M, is a right angle. And the two [vertical] angles at K are equal, so [*El.* VI, 4] the two triangles FKs[34] and KNM are similar. And, because their two sides FK, KM /14/ are equal the two sides Ks and KN are equal. But they are perpendicular to /15/ the two lines AG, EH. And for that reason the [two] chord[s], AG and EH, are equal [*El.* III, 14]. And they are in two equal circles, so [*El.* 28 & 24] segment ABG is equal to the alternate segment ETH. And, likewise, segment ADG is equal to the alternate segment EZH. And because the two lines AG, EH are equal and are parallel to the diameter LO, the arcs AL, LE, GO, [O]H are equal to each other. And this is what we wanted.

[6b, 24] [Proposition 9 (=*Sph.* II, 11 & 12)[35]] (Fig. 8) If a segment of a circle is set up at right angles on a diameter of a circle ABG, say segment ADG, and on its circumference an arbitrary[36] point, D, is marked, and from the circumference of circle ABG in both directions from the point G, two equal arcs are cut off, BG, GE, and two lines, DE and DB, are joined then I say that

34 "F" is missing in the text but is supplied in the margin.

35 *Spherics* II, 11 & 12 state Proposition 9 and its converse for the case of segments erected on the diameters of two equal circles. Proposition 9 is, of course, an immediate consequence of II, 11 & 12.

36 Instead of "arbitrary" (*kayfa mā waqaʿa*) the Arabic version requires that the points marked on the two equal segments divide the arcs so that the part nearer the end of the diameter are less than the other part. Jābir makes the same requirement in his statement of the converse.

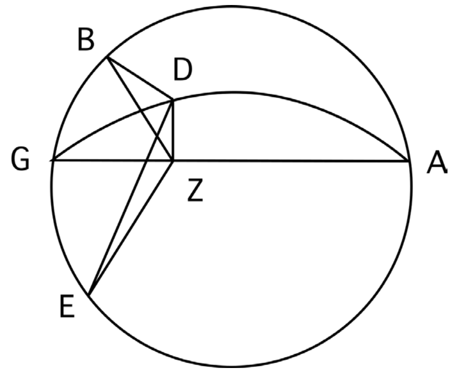

Figure 8. Proposition 9.

they are equal, and (conversely) if they [the lines] are equal and the point D divides the arc ADB into two unequal parts then the arcs GB, GE are equal.

And, conversely, the proof will be evident that if BD and DE are equal and point D divides arc AGD into unequal parts, then the two arcs BG and GE will be equal. And for the very same reason it will be necessary when there are set up on the diameters of equal circles segments of equal circles. And that is what we sought to prove.

And, now that that has been established, let us establish a necessary premise, namely:

[The Rule of Four Quantities][37]

[7a, 14] If there are two great circles on a sphere, [neither passing through the pole of the other] and if two points are marked on the circumference of one of them, or one point on the circumference of each of them, however it may fall, and from each of the two points is produced an arc of a great circle which contains with the arc of the second circle a right angle then the ratio of the Sine of the arc that is between one of the two points and one of the two points of intersection[38] to the Sine of the arc produced from that point to the second circle is as the ratio of the Sine of the arc that is between the second and between the other of the two points of the intersection to the Sine of the arc produced from that point to the second circle.

So, let the two circles AGDB and AEZB be great circles on the sphere and first of all let there be marked on the circumference of circle ABGD two points G, D and let there be produced from them to the circumference of circle AEZ at two right angles [arcs GE and DZ]. Then I say that the ratio of the Sine of arc AG to the Sine of arc GE is as the ratio of the Sine of arc AD to the Sine of arc DZ.[39]

37 Jābir has no special name for this theorem. That Ibn Muʿādh skips it, and goes directly from the Sector Theorem (which Jābir skips) to the Sine Theorem is one more indication of Jābir's independence from Ibn Muʿādh's work.

38 This refers to point A in the diagram.

39 Kūshyār in his *Zīj al-jāmiʿ* states what is a special case of this theorem when arc AD in Figure 9 is a quadrant. He then states, as a corollary, the general case that Jābir and Abū al-Wafāʾ in his *Zīj al-majisṭī* state.

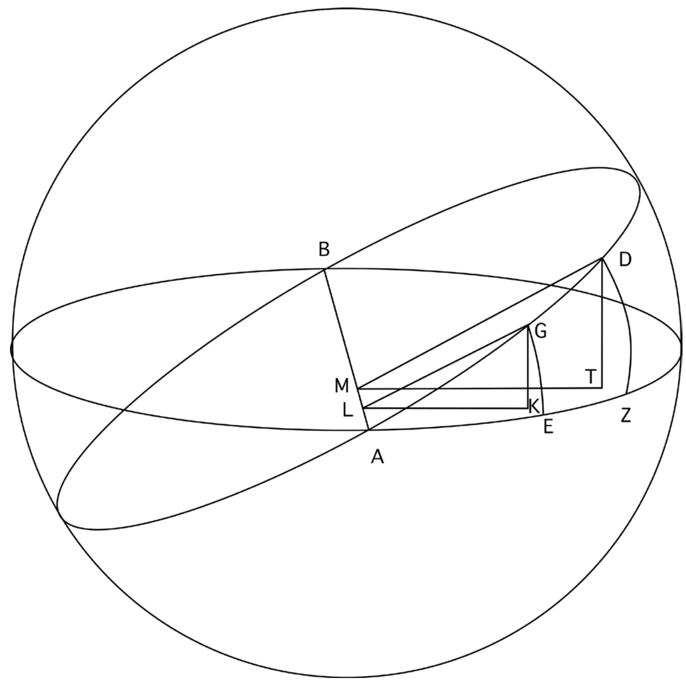

Figure 9. The Rule of Four Quantities.

The idea is[40] that the perpendiculars, DT and GK, from D and G onto the surface of circle AEZ, lie in the great circles containing arcs DZ and GE respectively.[41] Then, if we draw the perpendiculars, DM and GL, from D and G onto the diameter AB, and draw lines TM and KL we have created two right triangles, DMT and GLK, with MT parallel to LK and KG parallel to DT. Hence the two angles KGL and TDM are equal.[42] So the two triangles are similar. Thus GL:GK = DM:DT. But GL = Sin(arcGA) and GK = Sin(arc GE). Also, line DM = Sin(arc AD) and DT = Sin(arc DZ). The conclusion of the first part of the theorem follows.

Jābir now reduces the proof of the second part, when the two points are on opposite sides of A, to the first part.[43] (Fig. 10)

> [7b, 6] Let the point N be marked on the circumference of the circle AEZ and let there be produced from it an arc of a great circle that contains with the arc of circle AGB a right angle, namely arc *Ns*, and let angle *s* be right. Then I say that the ratio of Sine of arc AG to Sine of arc GE equals the ratio of the Sine of arc BN to [Sine] of arc Ns.

40 What follows is a summary of the author's translation in Berggren 2016, pp. 540-43, which also contains Jābir's proof of the Sine Theorem. Jābir's use of these theorems to prove two rules about spherical triangles with only one right angle may be found on pp. 543-44 . (The figure in Berggren 2016 is inexact since it gives the impression that the perpendiculars DT and GK do not lie in radii to Z and E.)

41 Both Abū al-Wafā' (Berggren 2007, 621-3) and Kūshyār (Debarnot 1985, 142-4) describe the perpendiculars DT and GK as perpendiculars from D and G onto the radii from the center of the sphere to the points Z and E respectively. Mathematically, of course, Jābir's approach comes to the same thing.

42 Kūshyār, too, argues that these two angles are equal, but Abū al-Wafā' argues that the other pair of acute angles in these two triangles are equal.

43 Kūshyār proves this, as a corollary to his 'Rule of Four Quantities' by regarding each of the two triangles AGE and AsN in Fig. 10 as being contained in a larger right triangle whose hypotenuse is a quadrant.

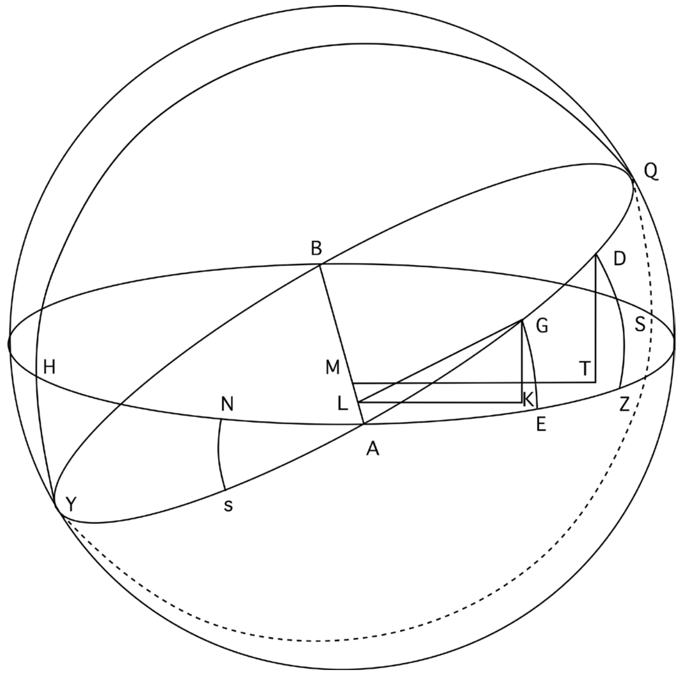

Figure 10. Second part of the Rule of Four Quantities.

Jābir then specifies that with A as a pole one makes arc AQ a quadrant and draws the great circle YHQ through Q intersecting AGB at Y and AEB at H.

The argument then runs as follows:[44]

Sin(AN):Sin(Ns) = Sin(AH):Sin(HY)

by the first part of the Rule applied to triangles ANs and AHY. By the same Rule applied to triangles AGE and AQS it follows that

Sin(AG):Sin(GE) = Sin(AQ):Sin(QS)

But AH = AQ because both arcs are quadrants. And if we remove HQ, the common part, from the two equal semicircles YHQ and HQS we obtain HY = QS, since. So HY = QS and AQ = AH, and therefore

Sin(AQ):Sin(QS) = Sin(AH):Sin(HY).

Hence

Sin(AG):Sin(GE) = Sin(AH):Sin(HY) = Sin(AN):Sin(Ns), which was to be proved.

Jābir now states the Sine Law for spherical triangles as follows:

> [8b, 23] Let there be a triangle ABG of arcs of great circles. Then I say that the ratio of the Sine of each of its sides to the Sine of the arc of the angle subtending it is one and the same ratio.

With reference to Fig. 11, Jābir first takes the case where the triangle has at least one right angle, say B. He extends arc GB to a quadrant, GE, and then considers the great circle through E and

44 Since all pairs XY in this proof denote arcs we shall abbreviate "arc XY" as "XY" and Sine as "Sin."

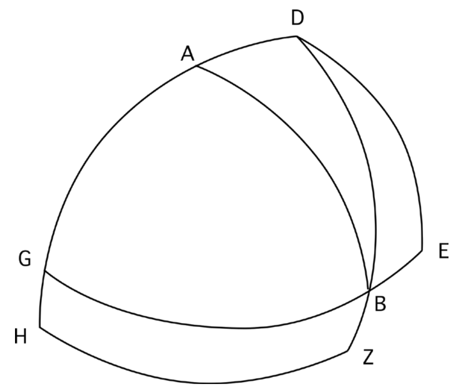

Figure 11. Sine law for spherical triangles.

the pole of GE. He then extends GA to meet the aforesaid great circle at D and then applies the Rule of Four to the two spherical right triangles, GBA and GED, with right angles at B and E and a common angle at G. After straightforward manipulations of proportions he arrives at the result that the ratio of the Sine of side AG to the Sine of the arc of the angle subtending it, B, is as the ratio of the Sine of side AB to the Sine of the arc of the angle subtending it, G.

With a similar construction involving constructing the quadrant AZ and the great circle arc HZ he is able to show that the ratio of the Sine of the side /13/ AG to the Sine of the arc of angle B, which it subtends, is as the ratio of the Sine of side BG to the Sine of the arc of angle A which subtends it.

He next deals with the case in which the triangle has no right angles by taking two cases, the first in which the perpendicular, AD, from A onto arc BG falls between B and G and the other case in which it falls on an extension of the arc BG, i.e. one of angles B or G is obtuse. With this case he concludes his proof of the Sine Law.[45]

Jābir then uses the Rule of Four Quantities to demonstrate two more rules for spherical trigonometry pertaining to a spherical triangle with a single right angle:

> 3. The ratio of the Sine of the side subtending the right angle to the Sine of one of the sides containing it is as the ratio of the Sine of the complement of the other containing side to the Sine of complement of the arc of the angle subtending it.[46]
>
> 4. Further, the Sine of the complement of the side subtending the right (angle) to the Sine of the complement of one of the two containing it is as the ratio of the Sine of the complement of the third side to the Sine of a quadrant.[47]

Although one can use Jābir's four rules of spherical trigonometry, the last of which is known as "Geber's Theorem" (after the Latinized version of Jābir's name), to find the Sine of a particular arc or angle one could not deduce immediately from that value whether the arc or angle was greater or less than 90°. This is because, as Jābir pointed out in the definitions at the beginning

45 In the Latin edition of *Improvement* published by Peter Apian in 1533 the proof of the Sine Theorem begins with the special cases of spherical triangles with three or two right angles (Lorch 1995, Item VIII, p. 6).

46 The "Sine of the complement" corresponds to the modern cosine.

47 More details on the proof than we have included in our overview may be found in the paper of Lorch, pp. 6 – 8, cited earlier. In particular, the manuscript that Lorch took as the basis for his account of Jābir's trigonometry includes, in the proof of the Sine Law, separate proofs for the case in which the spherical triangle has three or two right angles.

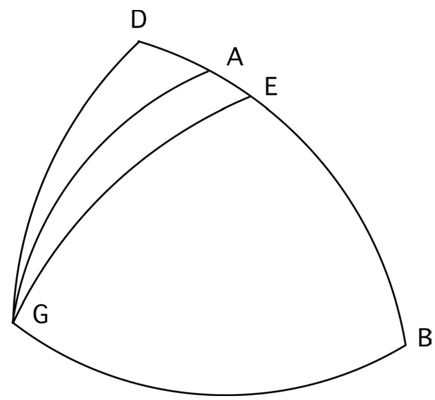

Figure 12. Proposition 1.

of this book, for any given acute angle, Θ, Sin(Θ) = Sin (180°- Θ). Thus, an astronomer would have to have some way of knowing whether a given angle should be the acute or its supplement, and whether a given arc should be less than 90° or the supplement of that arc.

To this end, Jābir followed the above theorems with a section "Some remaining properties of right triangles." The first propositions in this section deal with the two sides, AB and BG, containing the right angle, B, of a spherical triangle ABG. We have numbered the propositions 1 – 5 to aid in referring to them later. We refer to the angles as A, B, G and the sides opposite them as *a, b g*, the side opposite the right angle being *b*. The first theorem states a simple relation between the sides containing the right angle and the angles opposite them, and is tacitly referred to in each of the following three.

> [9a, 1] *Proposition 1.* Either of its two sides containing the right angle, together with the angle subtending it, follow one another. That is, if the side is equal to a quadrant the angle subtending it is right, if it is greater than a quadrant it [the angle] is greater than a right angle, and if it is less it [the angle] is [also] less. And, similarly, the side also follows the angle.
>
> So let the triangle be triangle ABG and let its angle B be right. Then I say that side AB and angle G /5/ subtending it follow one another that is if side AB is equal to a quadrant then angle G subtending it is right, and if it is greater than a quadrant it [the angle] is greater than a right, and, if is less then it, it is less than a right. And likewise, the side also follows the angle. And similarly for side BG with the angle A subtending it.
>
> Proof: If side AB is equal to a quadrant then the point A is a pole of arc BG and so angle G is [also] right. And if it [AB] is greater than a quadrant we cut off from it a quadrant, arc BE, [and] the point E is a pole of arc BG. And so the arc of the great circle passing over the two points, E and G, contains, with arc BG. a right angle. So angle EGB is right and so angle AGB is greater than right. And like that it is proved that if arc AB is less than a quadrant then angle AGB is less than a right angle. And in the same way it is also proved that the very same occurs for side BG and angle A. And that is what we wanted to prove.

The remaining theorems deal with how various conditions on *A, G, a, g* affect *b*, the side opposite the right angle. The following is a summary in modern notation. The statements are a bit involved because Jābir felt it necessary to remind the reader in each one of them that conditions on *a* or *g* could be replaced by conditions on A or G.

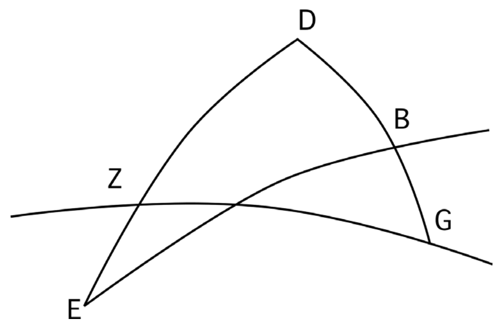

Figure 13. Proposition 3.

Proposition 2

If *a/g* is a quadrant or if A/G = 90° then *b* is a quadrant.

Proposition 3

If *g* and *a* are both > or < quadrants or A and G are both greater than or less than 90° then *b* is a quadrant

Proposition 4

If *a* > a quadrant and *g* < a quadrant and one of A or B > 90° and the other is less

Then *b* > 90° and conversely

Proposition 5

If *g* is a quadrant or G = 90° then A is a pole of *a* and *b* is a quadrant.[48]

As one more example of the proofs in this section we give Jābir's statement and proof of the part of Proposition 3 dealing with the case when sides *a* and *g* are both less than quadrants. (We have supplied the necessary figure (Fig. 13) which is lacking in the manuscript.

> I also say that... if each of the two sides containing it [the right angle] is less than or greater than a quadrant, or if each of the two remaining angles is less than or greater than a right [angle] the [side] subtending the right [angle] is less than a quadrant.
>
> If each of the two sides, AB and BG, is less than a quadrant and we make each of GD, BE a quadrant, then the arc of the great circle passing through the two points E, D, namely arc EZD, will cut circle AG beyond point A and its section is, say, at point Z. And because angle B is right and arc BE is a quadrant the point E is a pole of arc DG. So angle D is right. And because

48 Given Propositions 1 (that angles follow sides) and 2 the only thing this proposition adds is that A is a pole.

arc GD is a quadrant point G is a pole of arc ED and so GZ is a quadrant and so arc AG is less than a quadrant.

The following three propositions deal with how the two sides of a spherical triangle ABG containing the right angle, B, depend on side *b*. The first of them is the converse of the second proposition above, the second is the converse of the third proposition above, and the last is the converse of the last.

1. [9b, 23] [Proposition] Let us make the side AG subtending the right [angle] a quadrant. Then one of the two sides, AB and BG, is a quadrant.

Proof: If neither of the two sides, AB, BG is a quadrant then each of the two of them will either be less than a quadrant or greater than a quadrant and the other of the two will be greater than a quadrant or less. So, it follows from what we proved in the preceding, that AG is either greater than a quadrant or less. And that is a contradiction.

2. If side AG [10a, 1] subtending the right angle is smaller than a quadrant then the two sides, AB and BG, are either both larger or both smaller than a quadrant. Its proof is that if the two are not like that then one of them is greater and the second smaller or one of them is a quadrant and the second is smaller. And so it is necessary from what we have proved [earlier] that side AG is [then] greater than a quadrant of a circle. But it was assumed smaller, which is a contradiction, not possible.

And likewise, also, if one of the two is a quadrant it is necessary that side AG is a quadrant according to what we proved /5/ in the preceding, and so it is a contradiction that one of the two is greater and the second is less, or that one of the two of them is a quadrant. And so they will follow each other, i.e. that if one of the two is less than or greater than a quadrant then the second will be like it.

3. And, if the side subtending the right is greater than a quadrant then the two containing the right differ from each other, i.e. one of them is larger than a quadrant and the other is smaller.

Proof: If it is not so, then let the two of them follow one another, so both of them are either greater or less than a quadrant, as it is for when that side AG is less than a quadrant.[49] But it was postulated to be larger. This contradiction is impossible, so the two of them differ one from the other.

/10/ And like that it is also necessary that neither of them is a quadrant because if one of the two of them were a quadrant it is necessary that the one subtending the right is a quadrant according to what has been proved. And this contradiction is impossible.

/12/ So for that reason it is necessary that one of the two of them is larger than a quadrant and the second is less than a quadrant.

And the statement of the two angles following [the sides they subtend] is the same as the statement of the two sides subtending them. And let it be necessary that the [side] subtending the right is a quadrant or that one of the two remaining angles is right and that it [the side] is less than a quadrant. [Then] each one of them [the angles] is either less than or

49 By the third result above.

greater than a right. And if it is larger than a quadrant [then] one of them is larger than a right and the second is less than a right, and that is what we wanted to prove.

Jābir now leaves spherics and turns to a proof of the latter part of a remark that Ptolemy makes near the end of *Alm*agest I, 3 that "the circle is greater than [all other] surfaces and the sphere greater than [all other] solids." He writes:

/10a, 21/ Among the necessary premises we will prove that the measure of its [a sphere's] body is greater than the measure of every solid of equal surface whose surface is equal to the surface of that sphere.[50]

And it will be proved approximately (*min qurb*) if we prove that the measure of the body of a sphere is equal to[51] the product of half of its diameter by one-third of its surface.[52]

We omit Jābir's proof of the result and go to his conclusion of this section.

/10b, 27/ And now that we have proved this it will be clear that the volume of each sphere [11a,1] is greater than any polyhedral solid whose surface is equal to the surface of the sphere.

The proof of this statement ends at [11a, 13] and we resume our translation at that point where Jābir returns to the topic of spherics (Fig. 14).

[11a, 14] And among the necessary premises also are two cases: Let there be arcs AB, AG of two great circles which contain an angle, A, less than a right, and an arc DBG of a great circle passes through their poles. And let point D be the pole of arc AG. We produce a mean proportional in the ratio of the Sine of arc GBD, which is a quadrant of a circle, to the Sine of the arc DB[53] /20/ and it is the Sine of the arc DE, and we extend it to point L on arc AG. Then point E is the point at which there is the greatest difference between each of the two arcs cut off from arcs AB, AG, which correspond to arcs AE, AL.

So, let us mark two points, Z and H, on the two sides of point E. And let there pass over them and the pole D the two arcs DZT and DHK. Then I say that the difference between the two arcs AE, AL is greater than the differences between the two arcs AZ, AT and between AH, AK.

Proof. [We paraphrase Jābir's proof but use modern symbolism.[54]]

50 Ptolemy states this proposition without proof (and somewhat vaguely) near the end of *Alm*agest I, 4 (Toomer, p. 40.). It is one of a group of theorems on isoperimetry proved by the mathematician Zenodorus early in the second century B.C. (On Zenodorus see Toomer 1972)

51 The MS is somewhat blurred at this point, but instead of "equal to" it seems to say "greater than," which is both untrue and inconsistent with the rest of the text.

52 If *V, S* and *d* are, respectively, the volume, surface area and diameter of a sphere, then Jābir's statement, which we may express as $V = (d/2)(S/3)$ is equivalent to Archimedes' theorem in *Sphere and Cylinder* I, 34 that any sphere is equal to four times the cone which has as its base the greatest circle in the sphere and whose height is equal to the radius of the sphere. The equivalence of Jābir's statement and Archimedes' result is easily seen in light of Archimedes' Proposition *SC* I, 33 that *S* is equal to four times the area of the greatest circle in the sphere. (Archimedes' *Sphere and Cylinder* was known throughout the medieval Islamic world.)

53 The text repeats here "and it is the Sine of the arc DB."

54 When Jābir says the ratio of A to B is as the ratio of C to D we have rendered this as A/B = C/D, and "Sin" abbreviates the medieval Sine. Finally, as remarked earlier, since all magnitudes written XY denote arcs of great circles we omit the usual arc sign over them.

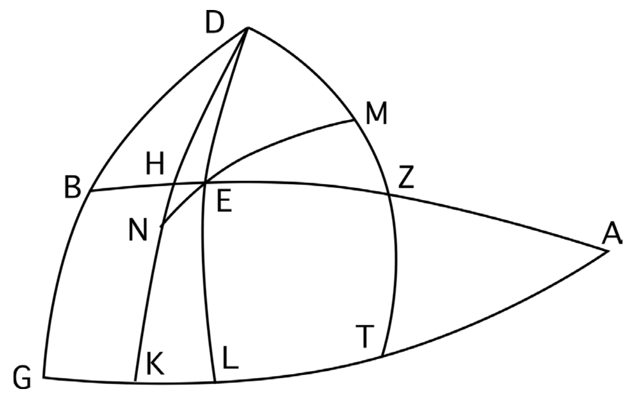

Figure 14.

By definition of E, and since DG and DL are equal, both being quadrants,

Sin(DL)/Sin(DE) = Sin(DE)/Sin(DB).

By the Rule of Four applied to triangles DEM and DLT, we also have

Sin(DL)/Sin(DE) = Sin(TL)/Sin(TM)[55]

So, *ex aequali*,

Sin(TL)/Sin(EM) = Sin(ED)/Sin(DB).

By the Rule of Four applied to triangles BDZ and MEZ, we also have

Sin(BD)/Sin(DZ) = Sin(EM)/Sin(EZ).

Hence, *ex aequali*,

Sin(TL)/Sin(EZ) = Sin(ED)/Sin(DZ).

But, since the arcs are all less than a quadrant, and ED < DZ it follows that TL < EZ. From this it follows that AE – EL > AZ – AT.

Jābir proves the other inequality by the same method, using the triangle EHN in Figure 14, which has a right angle at N and an angle at H equal to the vertex angle at H in triangle DHB.

We omit the proof of this proposition, which concludes on 12b/3, where he explains what this is all about:

> As has been shown clearly from the previous theorem before this one, we need to know the ecliptic degree for which the difference, between the degrees of the ecliptic and their

55 Here one needs the condition that EM passes through the pole of DT. Theodosius shows how to construct such a great circle, but Jābir takes the existence of such things for granted.

> ascensions in the corresponding[56] sphere, attains a maximum value, for this is something needed to calculate the difference between day and night. He [Ptolemy] mentioned this directly without any proof. We have considered it adequate to give a demonstration. It is also clear from the already previously mentioned second theorem that the variation (*tafāḍul*) in the declination of the degrees of the ecliptic in respect to the equator reaches its maximum at the two points of intersection [between the ecliptic and the equator] and its minimum at the two solstices. This was also mentioned by him in the second book [of the *Almagest*] in a straightforward way, without proof, and we have also decided to demonstrate it with the purpose that nothing in his book remains without demonstration. God (may He be exalted) willing. He is our help, etc.
>
> And this is all we need to premise in order to be free of needing the sector theorem,[57] the book of Menelaus and the book of Theodosius and what was with it that he mentioned, sending it without a proof. So the book will (God, the Exalted willing) stand by itself, without reference to any other, except the book of Euclid.

Jābir concludes his Discourse with, first, an explanation of how to find the lengths of chords in a circle of given radius, beginning with an explanation of how to calculate an approximate value of Crd(1/2°). He follows very closely Ptolemy's presentation of this topic in *Almagest* I, 10.

The final section of the First Discourse in his *Improvement* contains rules showing how to use the table of chords to solve plane triangles,[58] The final case is :

> And if the three angles are known then all three sides are known, on the condition that the diameter of the circle containing the triangle is known. And that follows because each one of its angles is known and so the arcs of that circle that those sides subtend are known[59] and for that [reason] the sides are known, i.e. the ratio of each one of them to the diameter of the circle is known. And for that reason it is necessary that the ratio of each one of them to each of the other two is known. And that is what we wanted to prove.
>
> The First Discourse, On the Premises, has finished with praise to God....

And our survey of the First Discourse of Jābir's work has finished with best wishes to Jim Evans for many more years of health and energy to inform his colleagues with his scholarship and delight his friends with his company.

56 *Muttaṣil* the "connected" sphere, which one understands to be the *Sphera recta* and the *Sphera obliqua*. If this interpretation is correct, he refers to the computation of "half the equation of daylight" (*e*), in order to obtain the longest day or night of the year, applying $e = \alpha_{\varphi}(\lambda_{\odot}) - \alpha_{0}(\lambda_{\odot})$. See *Alm.* III, 9. Also Pedersen/Jones 2011.

57 This was the usual name for it in medieval Islam and the Latin west. Today this theorem, upon which Ptolemy built his spherical trigonometry, is usually (and somewhat inaccurately) called Menelaus's Theorem.

58 Details my be found in Lorch Item VIII, pp. 31-4.

59 According to *El.* III, 20 the angle at the center of the circle is twice the angle at the circumference when they subtend the same arc.

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Traduttore-traditore: Thomas Digges as translator and interpreter of Copernicus' cosmology in *De revolutionibus*[1]

Michel-Pierre Lerner

In his opuscule published at London in 1576 under the title *A Perfit Description of the Cælestiall Orbes according to the most aunciente doctrine of the Pythagoreans, latelye revived by Copernicus and by Geometricall Demonstrations approved* ,[2] Thomas Digges (*ca* 1546-1595) confirms his allegiance to the Copernican doctrine of *De revolutionibus*, already proclaimed in his *Alae seu scalae mathematicae* (London, 1573).[3] But here he makes a noteworthy step beyond, since he now declares that the heliocentric thesis is *philosophically* founded.[4] According to Thomas Digges, the doctrine that Copernicus proposes in the *De revolutionibus* evades the infinite absurdities pertaining to the Ptolemaic system resting on the authority of Aristotle and accepted in the universities. By setting the Earth in motion upon itself and around the Sun, which is situated immobile at the middle of the universe, he lays a solid and true foundation from which consequences follow that are themselves true too, whereas the traditional theory, which rests on a false principle—namely having the Earth immobile at the center of the universe—cannot produce anything but errors and absurdities.[5]

1 The present essay is an enlarged version of a chapter written for the vol. I (Introduction) of Nicolas Copernic, *De revolutionibus orbium cœlestium*/*Des révolutions des orbes célestes*, Édition critique, traduction et notes par M.-P. Lerner, A.-Ph. Segonds et J.-P. Verdet, avec la collaboration de C. Luna, I. Pantin, D. Savoie et M. Toulmonde, 3 vol., Paris, 2015. I thank Alexander Jones for the English version of my text. For the cited passages of Copernicus' *De revolutionibus*, references are given to the folios in the Nuremberg edition (1543) and to pages and lines in the vol. II (Texte et Traduction) of the French edition.

2 *A Perfit Description* by Thomas Digges was printed in the form of an *Addition* following the meteorological and astrological writing of Leonard Digges (first edition in 1555) entitled *A Prognostication everlastinge of righte good effecte* [...]. *Lately corrected and augmented by Thomas Digges his sonne*, London, 1576. The text of Digges father and son had seven editions between 1576 and 1605 (see J. L. Russell, "The Copernican System in Great Britain," in *Études sur l'audience de la théorie héliocentrique* [Studia Copernicana, vol. V], Wroclaw, 1972, 189-239, esp. 194). A critical edition of *A Perfit Description* was published by F. R. Johnson and S. V. Larkey, "Thomas Digges, the Copernican System and the Idea of the Infinity of the Universe in 1576," *The Huntington Library Bulletin*, 5 (1934), 69-117, esp. 78-95. We refer to the 1576 London edition (reprinted 1987) and to the Johnson-Larkey edition.

3 *Alae seu scalae Mathematicae, quibus visibilium remotissima Cœlorum Theatra conscendi, et planetarum omnia itinera novis et inauditis methodis explorari* [...] *Deique stupendum ostentum, Terricolis expositum, cognosci liquidissime possit*: see f. A4^{v}, 2A3^{r}, L2^{v}, L3^{r}. See S. Pumfrey, "'Your astronomers and ours differ exceedingly': the controversy over the 'new star' of 1572 in the light of a newly discovered text by Thomas Digges," *The British Journal for the History of Science*, 44 (2011), 29-60.

4 To the works of Johnson-Larkey, A. Koyré and M. A. Granada on Digges's Copernicanism cited *infra*, 000, n. 26 and 35, one may add L. Holt, "Rational Magic : Thomas Digges' Sixteenth-Century Defense of Copernicanism," *The Modern Schoolman*, 79 (2001), 23-40 ; R. S. Westman, *The Copernican Question. Prognostication, Skepticism and Celestial Order*, Berkeley-Los Angeles-London, 2011, 264-268.

5 *A Perfit Description*, f. M1^{V} ; 80 Johnson-Larkey. Digges directly echoes the passage of the *Praefatio authoris* (*De rev*., f. iij^{V}; vol. II, 7.23-28), where Copernicus evokes the fallacious character of the hypotheses on which his predecessors relied and which can only result in erroneous consequences. Here there is a direct critique of the *regula falsi*

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> There is no doubte but of a true grounde truer effects may be produced then of principles that are false, and of true principles falshod or absurdities cannot be inferred. If therefore the Earth be situate immoueable in the Center of the worlde, why finde we not Theorickes uppon that grounde to produce effects as true and certaine as these of Copernicus?

One has to know, in this subject, how to spurn the testimony of the deceiving senses, to follow Copernicus, guided by the pure light of reason that God has given man to lead him towards the truth in the midst of the forest of errors.[6] And, making a clear allusion to the thesis upheld in the *Ad lectorem*, Digges writes that one should not trust a certain author who, in his desire to protect Copernicus, pretends that he conceived the hypothesis of the Earth's mobility "onely as Mathematicall principles, fayned & not as Philosophicall truly auerred."[7]

In order that philosophers and mathematicians of the English universities can treat of so noble a question, not with "childish Inuectiues," but "with graue reasons Philosophicall and irreproueable Demonstrations Mathematicall,"[8] Digges chooses to set before their eyes the very text of Copernicus, without however specifying that he is giving chapters 10, 7, and 8 (and a short fragment of chapter 9) of the first book of *De Revolutionibus*, nor even citing the title of the book that he is presenting to the reader,[9] contenting himself with alluding to it by the name "new Theorick or model of the World."

One should note at the outset a peculiarity of the Diggesian presentation of Copernican heliocentrism. Whereas in *De Revolutionibus* Copernicus begins by establishing the probability of the diurnal motion of the Earth (while presenting, and then refuting, the arguments put forward by the philosophers against this thesis), and then he deals with the annual revolution, Digges chooses the inverse order of presentation for the arguments, which is the same order that Copernicus adopted in the *Commentariolus*. He begins with the description of the heliocentric order of the celestial orbs (the topic of chapter 10), and presents in second place the arguments of the philosophers against the possibility of the Earth's rotation (chapter 7) and their refutation by Copernicus (chapter 8).[10]

To give offhand an idea of the way in which Digges transposes the text of Copernicus while embellishing it with personal additions, a good example is offered by the English version of the

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that philosophers and astronomers often privileged, and that was critiqued by Kepler with a special demonstrative vigor in his *Contra Ursum* : see N. Jardine & A.-Ph. Segonds, *La guerre des astronomes. La querelle au sujet de l'origine du système géo-héliocentrique à la fin du XVIe siècle*, 2 vol. in 3 parts, Paris, 2008: vol. II/2, 265-267 and 418-419 (note).

6 *A Perfit Description*, f. M1^{v} ; 80 Johnson-Larkey.

7 *A Perfit Description*, f. M1^{r} ; 79 Johnson-Larkey.

8 F. Yates, "Giordano Bruno's Conflict with Oxford," *Journal of the Warburg Institute*, II, 1938-1939, (reprinted in *Lull & Bruno. Collected Essays*, vol. I, London, Boston and Henley, 1982, 134-150, esp. 142-144), qualifies this attack by Digges upon the academic circles for being closed to Copernican cosmological innovation as characteristic of what she calls a "Freelance Elisabethan philosopher." Falling in the same category, according to F. Yates, were such pro-Copernican authors as John Dee and Thomas Harriot.

9 The reader finds this title, in abbreviated form, in a passage where Digges writes that the heliocentric order of the planets will be established demonstratively "in the residue of *Copernicus Reuolutions*" (f. N2^{r} ; 85 Johnson-Larkey). In his copy of the *De rev*. (ed. 1566) preserved today in the Bibliothèque Publique et Universitaire de Genève (see O. Gingerich, *An Annotated Census of Copernicus'* De revolutionibus *(Nuremberg, 1543 and Basel, 1566)*, Leiden-Boston-Köln, 2002, 215 (Geneva 1), Digges has not underlined or annotated any of the passages that he translated in *A Perfit Description*.

10 This inversion of order does not imply that Digges knew the *Commentariolus* (a hypothesis that nothing allows one to justify from a documentary basis in any event).

first sentence of chapter 10. Where Copernicus soberly writes, "Altissimum uisibilium omnium, cælum fixarum stellarum esse, neminem uideo dubitare," Digges introduces this idea and comments on it in the following way:[11]

> Althoughe in this most excellent and dyfficile parte of Philosophye in all times haue bin sondry opinions touching the situation and mouing of the bodies Celestiall, yet in certaine principles all Philosophers of any accompte, of al ages haue agreed and consented. First that the Orbe of the fixed starres is of al other the moste high, the fardest distante, and comprehendeth all the other spheres of wandrringe starres.

On the subject of the periods of the planets, where Copernicus writes:

> Errantium uero seriem penes reuolutionum suarum magnitudinem accipere uoluisse priscos Philosophos uidemus, assumpta ratione, quod æquali celeritate delatorum quæ longius distant, tardius ferri uidentur, ut apud Euclidem in Opticis demonstratur,

Digges is more succinct in his translation, omitting the important reference to Euclid:[12]

> And of these strayinge bodyes called *Planets* the old philosophers thought it a good grounde in reason that the nighest to the center shoulde swyftlyest mooue, because the circle was least and thereby the sooner ouerpassed and the farther distant the more slowelye.

In the remainder of chapter 10, which he translates on the whole quite faithfully, Digges departs from his source on certain minor points, but in a more significant way in three more specific passages.

As regards the minor points, one notes that Digges (a) gives in rounded form the values of the Ptolemaic planetary distances that Copernicus took from Proclus's *Hypotyposis of astronomical hypotheses*; (b) omits the name *Machometus Arecensis* [= Albategnius], perhaps because he was unable to identify him; and (c) adds, in the description of the position of the Sun at the center of the universe, two verses with slight variants from the *Urania sive de stellis* of Giovanni Pontano (I 240-241).[13] The more noteworthy divergences from the text of Copernicus are the following.

The first concerns the passage regarding the order of the orbs, which Copernicus describes beginning with the highest of all ("Prima & suprema omnium, est stellarum fixarum sphæra [...] in deductione motus terrestris assignabimus causam"). In his transposition, Digges seeks to clarify the Copernican thought while stressing the economy of supernumerary spheres that it makes possible:[14]

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11 *De rev.* I 10, f. 7v ; vol. II, 33.5-6; *A Perfit Description*, f. N1r ; 83 Johnson-Larkey.

12 *De rev.* I 10, f. 7v ; vol. II, 33.6-10; *A Perfit Description*, f. N1r ; 83 Johnson-Larkey.

13 *A Perfit Description*, f. N3r ; 87 Johnson-Larkey; see Ioannis Ioviani Pontani, *Urania (siue De stellis),* Florence, 1514, f. 7r: "Ad cuius numeros et Di moueantur, et orbis / Accipiat [orbes — iant *Digges*] leges, præscriptaque fœdera seruet [ent — *Digges*]" [According to its rhythm [*scil.* the Sun's] the gods move, and the orb of the universe receives from it its laws, and obeys the prescribed rules]. These verses are already cited by Rheticus with reference to the description of the order of the Copernican orbs; see Georgii Joachimi Rhetici *Narratio prima*, Gedani, 1540 ; critical edition, French translation, and commentary by H. Hugonnard-Roche and J.-P. Verdet with the collaboration of M.-P. Lerner and A. Segonds (Studia Copernicana XX), Wrocław-Warszawa, etc., 1982, 59. It cannot be excluded that Digges was here indebted to Rheticus.

14 *De rev*. I 10, f. 9r; vol. II, 37.13-18; *A Perfit Description*, f. N3r ; 86-87 Johnson-Larkey. The mention of Copernicus›s name in this passage is a "confession" of Digges›s intervention with respect to the text that he is translating.

> The first and highest of all is the immoueable sphere of fixed starres conteininge it self and all the Rest, and therefore fyxed: as the place uniuersal of Rest, whereunto the motions and positions of all inferiour spheres are to be compared. For albeit sundry Astrologians findinge alteration in the declination and Longitude of starres, haue thought that the same also shoulde haue his motion peculiare : Yet *Copernicus* by the motions of the earth salueth al, and vtterly cutteth of the ninth and tenth spheres, whyche contrarye to all sence the maynteyners of the earthes stability haue bin compelled to imagine.

The second divergence consists of adding something concerning time that is missing from the text of Copernicus. After the passage where Copernicus evokes the “admirandam mundi symmetriam, ac certum harmoniæ nexum motus & magnitudinis orbium” (words that he translates faithfully), Digges adds on his own initiative the following consideration respecting time:[15]

> The times whereby we the Inhabitauntes of the earth are directed, are constituted by the reuolutions of the earth, y[e] circulation of her Centre causeth the yeare, the conuersion of her circumference maketh the naturall day, and the reuolution of the Moon produceth the monethe.

But it is chiefly the developments of the end of this chapter that introduce significant distortions in the presentation of Copernicus’s thought, a reflection of Digges’s wholly personal conception regarding the status of the Earth inhabited by mankind —which he characterizes systematically as *mortall world* or *Globe of Mortalitie, because it is the peculiare Empire of Death*[16] — and the nature of the supreme heavens, which are to contain both the stars and the angelic creatures. Thus, after having noted the tininess of the great orb relative to the immensity of the universe which man cannot ever sufficiently admire—an echo of the conclusion of Copernicus’s chapter 10 (“Tanta nimirum est diuina hæc Optimi Maximi fabrica”) —, Digges adds in his own right the following long elaboration:[17]

> we may easily consider what litle portion of gods frame, our Elementare corruptible worlde is, but neuer sufficiently be able to admire the immensity of the Rest. Especially of that fixed Orbe garnished with lightes innumerable and reachinge vp in *Sphæricall altitude* without ende. Of whiche lightes Celestiall it is to bee thoughte that we onely behoulde sutch as are in the inferioure partes of the same Orbe, and as they are hygher, so seeme they of lesse and lesser quantity, euen tyll our sighte beinge not able farder to reache or conceyue, the greatest part rest by reason of their wonderfull distance inuisible unto vs. And this may wel be thought of vs to be the gloriouse court of y[e] great god, whose vnsercheable worcks iuuisible we may partly by these his visible coniecture, to whose infinit power and maiesty such an infinit place surmountinge all other both in quantity and quality only is conueniente.

Where does the thesis come from according to which the orb of the fixed stars extends infinitely “in *Sphæricall altitude*”?[18] It did not escape Digges that, in chapter 8 of the first book, of which he gives a partial translation further down, Copernicus advances the hypothesis that the heavens

15 *De rev*. I 10, f. 10[r]; vol. II, 39.1-2; *A Perfit Description*, N3[v] ; 87 Johnson-Larkey.

16 *A Perfit Description*, f. M1[r], M2[r], N3[r] ; 79, 80, 87 Johnson-Larkey.

17 *De rev.* I 10, f. 10[r] ; vol. II, 39.23; *A Perfit Description*, f. N4[r] ; 88-89 Johnson-Larkey.

18 This assertion is explicitly formulated in the caption, cited below, that accompanies the heliocentric diagram placed at the front of *A Perfit Description* (f. 43).

could be infinite and limited only by its interior concavity.[19] But whereas Copernicus holds back from making a formal settlement of the sense in which the universe is finite or infinite, saying that he is leaving this debate to the philosophers, Digges takes a stand in favor of infinity as a philosopher, but also as a theologian, as the caption of the diagram that portrays the higher region of the universe suggests:[20]

> This orbe of starres fixed infinitely up extendeth hit self in altitude sphericallye, and therefore immovable. The pallace of foelicitye garnished with perpetuall shininge glorious lightes innumerable farr excellinge our sonne both in quantitye and qualitye,

and he declares that this same heavens in which are scattered stars larger and more luminous than the Sun that lights the Earth is also:[21]

> The very court of coelestiall angelles devoyd of greefe and replenished with perfite endlesse joye. The habitacle for the elect.[22]

It follows from this curious description of the "content" of the orb of fixed stars that the latter is, for Thomas Digges, at once the astronomical heavens of the fixed stars and the empyrean heaven (though the term is not used) endowed by the theologians to the angels and the elect. This conception came close to that of Marcellus Palingenius (*ca* 1500-*ca* 1543), author of the famous *Zodiacus vitae*[23] whom Digges cites in the *Perfit Description* by the name of "Stellifyed Poet," and also "Christian Poet."[24] But Digges's conception is more complex than that of Palingenius. For the latter, only the empyrean heaven, being of "spiritual" nature, is infinite in extent, thus expressing the all powerfulness of God who could not limit himself to producing a finite universe.[25] For Digges, by way of contrast, the infinity is recognizable in the heavens of the fixed stars, the astronomical heavens, though without belonging uniquely to it as such, inasmuch as this infinity is also, inseparably, an attribute of the "spiritual" heaven where the saints and angels of God also dwell. In this sense, though for Digges the sphere of fixed stars vanishes in its classical configuration—whereas it is preserved in Palingenius—the infinite "orb" in which it loses itself remains fundamentally heterogeneous in relation to the universe where the planets have their

19 *De rev.* I 8, f. 5^{v}; vol. II, 28.24-25: "si cælum fuerit infinitum, & interiori tantummodo finitum concauitate [...]." Digges translates this as follows: "if [...] the Haeuen were indeede infinite vpwarde, and only fynyte downewarde in respecte of his spericall concauitye" (*A Perfit Description*, f. O1^{v} ; 91 Johnson-Larkey).

20 *A Perfit Description*, f. 43.

21 *Ibid.*

22 In the *Prognostication everlastingue*, Leonard Digges (*supra*, n. 1) proposes in two passages of his text (f. 4^{v} et 16^{r}) the geocentric *systema mundi* involving ten astronomical heavens, surrounded in the classical way by the empyrean heaven, labeled thus: "Here the learned do appoyncte the abitacle of God and all the electe." According to Digges *père*, the elect do not share their realm with the stars, which remain confined to the *Sterrie firmament* that revolves below the starless crystalline heaven.

23 See the critical edition of the *Zodiacus vitae*, edited, translated, and annotated by J. Chomarat, Genève, 1996. On Marcellus Palingenius, see the notice "Manzoli, Pier Angelo," in *Dizionario Biografico degli Italiani*, Roma, 1960 — , vol. 69 (2007), 293-297 [M. Palumbo].

24 *A Perfit Description*, f. M1^{v}-M2^{r} ; 80 Johnson-Larkey.

25 See J. Chomarat, "La création du monde selon le poète Palingène," *Bulletin de l'Association Guillaume Budé*, 4 (1988), 352-363; see also F. Bacchelli, "Palingenio e la crisi dell'aristotelismo," in *Sciences et religions. De Copernic à Galilée (1540-1610)* [Actes du Colloque international organisé par l'École française de Rome, 12-14 décembre 1996], Rome, 1999, 357-374.

motions, and still more in relation to the "globe of mortality" that is our Earth.[26] As one can see, one is truly very far from the spirit of Copernicus—though his disciple Rheticus, in the writing (unpublished during his lifetime) in which he defends heliocentrism *more theologico*, could "justify" the immobility of the heavens of the fixed stars (but not its infinity) by the fact that Scripture and certain theologians placed there the abode of God:[27]

> [...] since Scripture [*Isaiah*, 66, 1] calls the heaven God's seat, and the earth His footstool, we may conceive the heaven as being immobile and subject to no disquiet, as the noblest part of created nature.

With the English adaptation of chapters 7 and 8 of Book 1, devoted to defending the possibility of the proper rotation of the terrestrial globe, Digges returns to a greater faithfulness to the text of Copernicus. His declared purpose is to give his reader:[28]

> a taste of the reasons philosophicall alleged for the earthes stabilitye, and their solutions, that sutch as are not able with *Geometricall* eyes to beehoulde the secrete perfection of *Copernicus Theoricke*, maye yet by these familiar, naturall reasons be induced to serche farther, and not rashly to condempne for phantasticall, so auncient doctrine reuiued, and by *Copernicus* so demonstratiuely approued.

The title of chapter 7 develops somewhat Copernicus's briefer title ("Cur antiqui arbitrati sint terram in medio mundi quiescere tanquam centrum") in the following way: "What reasons moued Aristotle and others that folluued him to thincke the earth to rest immoueable as a Centre to the whole world."[29] If, as has been said, the English author is here closely following the Latin text, nevertheless a point of translation deserves to be brought out: it concerns the passage where Copernicus borrows from Ptolemy the critique according to which the extreme speed of the Earth's proper rotation would have the effect of breaking it up and making it fall out of the heavens ("& iamdudum, inquit [*scil.* Ptolemaeus], dissipata terra cælum ipsum (quod admodum ridiculum est) excidisset"). The verb *excidisset* has manifestly posed a problem of understanding for Digges—others after him have met with the same difficulty—who has translated the passage in question thus: "And longe ere this the Earthe beinge dissolued in peeces should haue been scattered through y^e heauens, which were a mockery to thincke of."[30] Here, the idea of "departure" from the heavens that is present in Ptolemy—which should result in a rectilinear fall towards the bottom of the terrestrial globe, and not, as Copernicus makes him say, a violent

26 On the conceptions of Palingenius and of Digges, see A. Koyré, *Du monde clos à l'univers infini*, cit., 23-29 and 39-43, and M. A. Granada, "Bruno, Digges, Palingenio: omogeneità ed eterogeneità nella concezione dell'universo infinito," *Rivista di storia della filosofia*, 47 (1992), 47-73.

27 See *Cujusdam anonymi Epistola de terrae motu*, Ultrajecti, 1651. This anonymous writing, published in 1651 by Johannes van Waesberge, printer at Utrecht, was attributed to Rheticus by R. Hooykaas in 1984 and published with an English translation in *Georg Joachim Rheticus' treatise on Holy Scripture and the motion of the earth, with translation, annotations, commentary and additional chapters on Ramus-Rheticus and the development of the problem before 1650*, Amsterdam, 1984; for the cited passage, see 17: "Cum igitur Scriptura vocet coelum, sedem DEI et terram scabellum pedum suorum [*Is*. 66, 1], poterimus coelum intelligere immobile, et nulli inquietudini subjectum, tanquam nobilissimam naturae conditae partem" (English transl., *ibid*., 73).

28 *A Perfit Description*, f. N4^r ; 89 Johnson-Larkey.

29 *De rev.* I 7, f. 5^r; vol. II, 26.15-16; *A Perfit Description*, f. N4^r ; 89 Johnson-Larkey.

30 *De rev.* I 7, f. 5^r-v; vol. II, 27.18-19; *A Perfit Description*, f. N4^v ; 90 Johnson-Larkey.

circular motion—is missing.[31]

The translation of chapter 8 too is faithful in general, starting with its title: "The Solution of these Reasons with their insufficiencye."[32] Digges does not abstain, nevertheless, where he judges it necessary, from clarifying Copernicus's thought, or from outright adding an argument to reinforce the latter's reasoning. We will cite two significant instances. The important passage where Copernicus declares: "Siue igitur finitus sit mundus, siue infinitus, disputationi physiologorum dimittamus: hoc certum habentes, quod terra uerticibus conclusa superficie globosa terminatur," is elucidated in the following manner by Digges:[33]

> But whether the worlde haue his boundes or bee in deede infinite and without boundes, let vs leaue that to be discussed of Philosophers, sure we are y^t the Earthe is not infinite but hath a circumference lymitted, seinge therefore all Philosophers consent that lymitted bodyes maye haue Motion, and infinyte cannot haue anye.

Digges's other notable intervention consists in the addition to Copernicus's text of an "experience" designed to reinforce his argument, according to which, down here, there is no pure rectilinear movement, but one is always dealing with a mixture of rectilinear and circular. Thus the sentence: "Cadentium uero & ascendentium duplicem esse motum fateamur oportet mundi comparatione, & omnino compositum ex recto & circulari," faithfully translated as: "And for thinges ascendinge and descendinge in respect of the worlde we must confesse them to haue a mixt motion of right & circulare," is accompanied by a brief commentary concerning the experience of the senses which deceive us on this point ("albeit it seeme to vs right & streight"), and followed by an example intended to confirm, by reasoning, the truth of the thesis:[34]

> No otherwise then if in a shippe vnder sayle a man should softly let a plummet downe from the toppe alonge by the maste euen to the decke: This plummet passing alwayses by the streight maste, seemeth also too fall in a righte line, but beinge by discours of reason wayed his Motion is found mixt of right and circulare.

This addition of Digges's deserves to be highlighted in two respects. First, because it extends in a certain manner the Virgilian comparison of the boat leaving the port that Copernicus cites,[35] adding to it the experience of a mass of lead falling in a straight line from the top of the mast to its foot on a ship in motion. Second, because this experience (here presented as imaginary: "if ... a man should let") will recur in 1584 in the *Cena de le Ceneri* of Giordano Bruno, whose direct source very likely was Digges, as Johnson and Larkey first suggested, and as A. Koyré also thinks.[36]

A final significant intervention by Digges before the end of chapter 8 should be mentioned. Where Copernicus soberly shows that, since the planets approach and recede from the Earth,

31 For analysis of this passage of the *De rev*. I 7, f. 5^r; vol. II, 27.12-21, see Nicolas Copernic, *De revolutionibus orbium cœlestium* (cit n. 1), vol. III, 98-101.

32 *De rev*. I 8, f. 5^v; vol. II, 27.25; *A Perfit Description*, f. O1^r ; 89 Johnson-Larkey.

33 *De rev*. I 8, f. 6^r; vol. II, 29.3-5; *A Perfit Description*, f. O1^v ; 91 Johnson-Larkey.

34 *De rev*. I 8, f. 6^r; vol. II, 30.5-6; *A Perfit Description*, f. O2^r ; 92-93 Johnson-Larkey.

35 *De rev*. I 8, f. 6^r; vol. II, 29.10-11 : "Prouehimur portu, terræque urbesque recedunt" (see Virgil, *Aeneid*, III 72).

36 G. Bruno, *La Cena de le Ceneri* (s. l., 1584), third dialogue, ed. G. Aquilecchia, French translation Y. Hersant, Paris 1994 (OC II), 182-184 and n. 73 ; *The Ash Wednesday Supper: La cena de le ceneri*, transl. and ed. E. A. Gosselin-L. Lerner, New York, 1977, 162-164. See F. R. Johnson-S. V. Larkey, "Thomas Digges, the Copernican System," cit., 99 ; A. Koyré, *Études d'histoire de la pensée scientifique*, Paris, 1973, 327-328.

they cannot have the latter as the unique center of their movements, Digges introduces considerations concerning gravity, which he borrows from the beginning of chapter 9, before resuming the final lines of the preceding chapter:[37]

> Seinge therefore that these Orbes haue seuerall Centres, it may be doughted whether the Centre of this earthly Grauity be also the Centre of the worlde. For Grauity is nothinge els but a certaine procliuitye or naturall couetinge of partes to be coupled with the whole, whiche by diuine prouidence of the Creator of al is giuen & impressed into the parts, y[t] they should restore themselues onto their vnity and integritie concurringe in sphericall fourme, which kinde of propriety or affection it is likelye also that the Moone and other glorious bodyes wante not to knit & combine their partes together, and to mainteyne them in their round shape, which bodies notwithstandinge are by sundrye motions, sundrye wayes conueighed.

After having faithfully repeated Copernicus's conclusion at the end of chapter 8 on the "probable" character of the Earth's mobility, Digges again adds on his own initiative the following commentary:[38]

> So if it bee Mathematically considered and wyth Geometricall Mensurations euery part of euery *Theoricke* examined : the discreet Student shall fynde that *Copernicus* not without greate reason did propone this grounde of the Earthes Mobility.

After his exposition of the Copernican ideas relating to the mobility of the Earth, Digges maintains that, for anyone who engages in a "technical" comparative examination of the models of the geocentric and heliocentric universes concurrently, the choice can only fall upon Copernicus, whose superiority he believes he has demonstrated from the point of view of natural philosophy. However, he seems to have had the goal of strengthening his pro-Copernican argumentation in a work specially consecrated to *De Revolutionibus*, which would have had the title: *Commentaries upon the* Reuolutions of Copernicus, *by euidente Demonstrations grounded upon late* Obseruations, *to ratifye and confirme hys* Theorikes *and* Hypothesis, *wherein also Demonstratiuelie shall be discussed, whether it bee possible upon the vulgare* Thesis *of the Earthes* Stabilitie, *to delyuer any true* Theoricke *voyde of such irregular Motions, and other absurdities, as repugne the whole* Principles of Philosophie *naturall, and apparant grounds of common* Reason.[39] But this work would never see the light of day.

Conclusion

Consisting essentially of an English paraphrase of certain passages of Book 1 of Copernicus, *A Perfit Description of the Cælestiall Orbes* has the interest of presenting for the first time in public in a vernacular language the major outlines of the new cosmology.[40] As we have seen, Digges in

37 *A Perfit Description*, f. O3[r] ; 94 Johnson-Larkey. For the passage concerning gravity in Copernicus see *De rev*. I 9, f. 7[r]; vol. II, 32.10-18, and commentary, vol. III, 117-119.

38 *De rev*. I 8, f. 7[r]; vol. II, 31.26-32.1 : "Vides ergo quod ex his omnibus probabilior sit mobilitas terræ, quam eius quies"; *A Perfit Description*, f. O3[r] ; 94-95 Johnson-Larkey.

39 The work was announced in *An Arithmeticall Militare Treatise, named Stratioticos* [...], London, 1579, f. aii[i]j[r], in a list of books "to be published."

40 Nicolas Raimer Ursus (1551-1600) was responsible for the first quasi-complete German translation of the *De revolutionibus*. Completed in 1586-1587, this version in "frühhochneudeutsch" of Copernicus's book remained unpublished until the beginning of the 21st century: see *Die erste deutsche Überstezung in der Grazer Handschrift*. Kritische Edition, bearbeitet von A. Kühne und J. Hamel, unter Mitarbeit von U. Lück, Berlin, 2007 [= Nicolaus

places translates Copernicus's Latin with fidelity, but he often indulges in paraphrase, adding not only words but also ideas of his own invention, without making it clear that a personal intervention is involved, so that his reader has at his disposal a Copernican doctrine that is in part faithful, and in part "interpreted" in a sense that is sometimes quite special.

Was this *sui generis* version of Copernicanism capable of contributing to the making of favorable conditions for the adoption of heliocentrism by the mathematicians and philosophers of the English universities? Contrary to what F. Yates believes,[41] the significant number of reeditions of the *Perfit Description* between 1576 and 1605 cannot stand as a cogent index of the success of the new astronomy presented by Thomas Digges, inasmuch as his opuscule and the *Prognostication everlasting* of his father Leonard contain diverse elements of practical mathematics that could have held the interest of a public more diverse than that of the astronomers and philosophers of nature. In fact, we do not seem to find a contemporary reaction from the English academic circles to the Diggesian version of the Copernican cosmology.[42] But this silence is perhaps itself a response to the question that we are asking. For if the advantages of Copernicus's system as they are presented in *A Perfit Description* could at a pinch be discerned, even if not accepted, by a specialist in the science of the stars, it is clearly more doubtful whether a philosopher[43]—and *a fortiori* a theologian, whether Protestant or Catholic[44]—would have accepted the "heterodox" vision presented by Thomas Digges of a stellar universe of infinite extent where stars and angels cohabit in a kingdom washed in light and beatitude.[45]

Copernicus Gesamtausgabe im Auftrag der Kommission für die Copernicus-Gesamtausgabe herausgegeben von H. M. Nobis und M. Folkerts, vol. III/3]. For an analysis of some particularities of Ursus' translation in German, see Nicolas Copernic, *De revolutionibus orbium cœlestium* (cit. n. 1), vol. I, 618-637.

41 "Giordano Bruno's Conflict with Oxford" (cit. n. 7), 143.

42 M. Feingold, *The Mathematicians' Apprenticeship. Science, universities and society in England, 1560-1640*, Cambridge, 1984, is silent on this point.

43 Some historians have asked whether the conception of an infinite universe defended by Nicholas Hill in the *Philosophia Epicurea, Democritiana, Theophrastica proposita simpliciter, non edocta*, Paris, 1601 (2[d] ed. Geneva, 1619) could have been influenced by the Diggesian conception, but the prevailing view is that Giordano Bruno and/or William Gilbert would be the best candidates: see S. Ricci, *La fortuna del pensiero di Giordano Bruno 1600-1750*, Florence, 1980, 56-63.

44 One among the ideas that earned the "Stellifyed Poet" Marcellus Palingenius condamnation by the Roman Inquisition as a heretic after his death was certainly the conception of an infinite universe: see C. Moreschini, "La *perfidia* di Marcello Palingenio Stellato," *Bruniana & Campanelliana*, XIX (2013), 103-118.

45 K. A. Tredwell's attempt to connect Thomas Digges' defence of the Copernican heliocentrism with his approval of Melanchthon's providential view of the heavens, and not to his supposed Calvinist tendencies, is unconvincing (see "The Melanchthon Circle's English Epicycle," *Centaurus*, 48 (2006), 23-31, part. 27-29). Furthermore, nothing is more adverse to the rejection of the Empyrean by the majority of the Protestants than Digges' conception of an infinite stellar heaven being also "the habitacle for the elect": see W. G. L. Randles, *The Unmaking of the Medieval Christian Cosmos, 1500-1760*, Ashgate, 1999, 34-39, 133-136.

Gallucci's pseudo Copernican equatorium, Venice, 1593

Owen Gingerich

When a large package arrived from Germany a few years ago, I was filled with indignation. Why on earth had the auctioneers packaged Giovanni Paolo Gallucci's quarto volume in such an oversized carton?

But when I unwrapped the apparently oversized parcel, the joke was on me! I thought I had bid on Gallucci's relatively common celestial map book known for its volvelles, *Theatrum mundi et temporis* (1588), unaware that the Venetian author had in 1593 published a much larger-format volvelle book. What I had won at the auction was the very different folio, *Speculum uranicum*, a rarer production. It isn't a very thick volume, but it stands 42 cm high. It has [4] + 43 folios, and 16 of its pages have large circular woodcuts with moving parts.

Thus began a quest to understand how Gallucci's volvelles are supposed to work, and in fact how some of the moving parts were to be correctly assembled. So far I have examined a dozen copies, and with the exception of one hand-colored exemplar, apparently the dedication copy now in a private American collection, every copy has some fault: at least one loose, missing, or miss-positioned piece. The book was clearly intended to be used for computing the positions of the planets, undoubtedly for astrological purposes. My strong impression is that the book was more to be admired than actually used for computations.

But then again, the same could be said for Peter Apian's spectacular *Astronomicum Caesareum* of 1540. Apianus's masterpiece was surely the greatest work of astronomical printing in the 16th century. All the copies—probably around 150—were marvelously produced, hand-colored, and assembled in his Ingolstadt workshop. Twenty of the 146 pages contain volvelles, and the mechanism for the longitude of Mercury contains six rotating disks.

One reason that the *Astronomicum Caesareum* is a comparatively well-known book is that Edition Leipzig in 1970 issued a magnificent colored facsimile based on the copy in the Gotha (Germany) library, in an edition of 750 copies.[1] The printing was a tour de force, complete with a trompe l'oeil wrinkle on one of the final pages. But the assembly of the moving parts was a major disaster. For example, where Apianus had used multiple axes on some of the more complicated mechanisms, the facsimile has simply forced a single axis through the stack. And many of the disks are attached to the wrong pages. Having corrected more than a dozen copies of the facsimile, I feel familiar with the way it is supposed to work. When the paper discs are correctly positioned, it is possible to match the planetary longitudes calculated by the *Alfonsine Tables* to within a degree.

In order to appreciate what Gallucci has and has not accomplished with his *Speculum uranicum*, it is useful to examine first how planetary positions are established with a true equatorium. By an equatorium I mean a device that models the planetary motions, generally a paper instrument. To compare and contrast Gallucci's presentation with Apianus's *Astronomicum Caesareum*, I will show in detail with each volume how to compute the position of Mars for 1593 August 10 (Julian). This date is specially chosen because it was then that Tycho Brahe noticed that the

1 Owen Gingerich, Apianus's *Astronomicum Caesareum* and its Leipzig Facsimile, *Journal for the History of Astronomy*, 2 (1971), 168-177.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 271–280. Chapter DOI: 10.5281/zenodo.3975753.

Ptolemaic (Alfonsine) tables and the Copernican tables predicted woefully inaccurate positions for Mars.[2] It is only a coincidence that this was noticed in 1593, the very year in which Gallucci's *Speculum uranicum* was printed. As Kepler was later to discover, this Martian catastrophe took place for a few weeks every 32 years (and had apparently gone unnoticed for centuries!). The catastrophe was of course for the tables, not for the ruddy planet. As we will see, Apianus's work is based on the *Alfonsine Tables*, whereas Gallucci generally uses the Copernican *Prutenicae Tabulae*, and both sets of tables were in trouble.

From Tycho's Observation Log for Mars, 1593 August 10 (Julian)

Copernicus	Psc 12° 0'	(calculated from *Prutenicae tabulae*)
Tycho	Psc 16° 7½'	(observed)
Alphonso	Psc 21° 26'	(calculated from the *Alfonsine Tables)*

In the technical details that follow, it will become apparent why I have chosen to refer to Gallucci's device as a "pseudo Copernican equatorium," for it is neither an equatorium nor is it heliocentric (but its numbers are Copernican!).

First, a brief orientation to the geocentric model for the superior planets (Mars, Jupiter, and Saturn). A large basic circle, called the deferent or carrying circle, is offset from the earth by an amount called the eccentricity and in a direction toward the farthest point called the apogee or aux. What the deferent or eccentric circle carries is the epicycle. Even if the epicycle is carried at a constant speed around the deferent, it will not appear to move at a constant angular speed as seen from the earth. Hence, in calculating the longitude of a planet, the first step must be to locate the direction of the apogee and to make an adjustment for the varying apparent speed of the epicycle. Then the apparent back-and-forth position of planet as it moves uniformly in its epicycle is the next step. Finally, a correction depending on the distance of the epicycle from the earth is required.

In Apianus's graphical device all of these corrections occur automatically in what is essentially an analog computer, where all the steps are visual. Gallucci's system, in contrast, is a less intuitive digital method, where it requires strict attention to know, for example, if a quantity is to be added or subtracted.

1593 August 10 (Julian): Mars in the Astronomicum Caesareum

In Ptolemy's epicyclic theory each planet's motion depends on three time-dependent motions: the moving position of the planet in its epicycle, the position of the epicycle on the deferent, and the position of the apogee. At first glance it might be assumed that the apogee is a fixed direction in space, but if it is locked with the starry frame, then there is a slow precessional motion as the entire sidereal frame moves with respect to the coordinate frame indexed to the sun's sidereal position at the time of the equinox.

Precession is such a sufficiently slow effect that Ptolemy was obliged to compare his current star positions with those made over two centuries earlier to derive the rate of precessional mo-

2 J. L. E. Dreyer, ed., *Tychonis Brahi Dani Opera Omnia,* vol. 12, (Copenhagen, 1925), 285.

tion, and in this he made one of the two major stumbles of his entire system (the other being the variation in the apparent size of the moon). In any event, he chose the convenient precessional rate of a degree per century, while actually it is closer to a degree every 71 years. Thus it didn't take too many centuries for repair work to be required on the rate of precession. However, in order to preserve the apparently slow rate deduced by Ptolemy between his time and that of Hipparchus, a variable supplementary rate was proposed, called trepidation. By the time of Copernicus and Apianus, the combined rate was failing, but Apianus stuck with the traditional precession-trepidation combination. He began his planetary calculations with precession and trepidation.

Because Apianus assumed that the precession-trepidation combination affected all the planets identically, he chose to deal with this by a single volvelle, a particularly beautiful rotating planisphere. the first moving disk in his book. (It will of course be desirable to have an *Astronomicum Caesareum* or its facsimile in hand in order to understand easily the following instructions.) To determine the precession for our example, open the book to its first volvelle, set the index tab M from the precession scale hidden under the right edge of Apianus's star map. Extend the thread from the middle of the planisphere across *1600 post Christ* on the trepidation oval and rotate the planisphere until the index tab labeled *Y AUX Communis* lies under the thread. The index tab marked with the symbol for Mars should then be at Leo 16°, which will be used with the set of disks for Mars itself on f. D III.[3] Note that this procedure has simultaneously set the starting point for each planet.

(Mean motion) We must next deal with the second and eventually the third time-dependent parameters; the starting points for these are given on the two tall tables on the page facing the Mars disks on f. D III of the *Astronomicum Caesareum*. Take the starting value for 1500 years after Christ for the mean motion of Mars, 8^{S} 5° 6', (where the superscript *s* refers to the zodiacal sign, which is labeled and numbered on the border of the disks on the facing pages). Take also the mean argument (which will refer to the epicycle), 1^{S} 14° 13'. Set the pointer tab (labeled M) on the lowermost disk to Sco 5° 6' (where Sco is labeled as the 8^{th} sign).

The next step is a little tricky, because we will make the correction for the year by using the *completed* year, 1592, rather than the year in question, 1593. Find 92 on the border of the lower disk (hint: it should be near Libra 6°). Stretch the thread from *A mũdi* (= "mundi" or "world") over and past 92, and rotate the lower disk until the edge of the M index tab is directly under the thread. Finally, find August 10, which is concealed by the disk above it in the stack (the so-called AUX disk). Using the thread as before, rotate the M tab, which should now be at Aquarius 3°. This establishes the mean motion of Mars.

(Eccentricity) We must now take into account the eccentricity of Mars' orbit. Set the AUX disk at Leo 16°, the number found in the initial step from the planisphere. The next step is to transfer the mean motion indicated by the position of the M index pointer to the equant system which has its own axis, and which by definition carries uniform motion around the equant. In Apianus's ingenious arrangement, the set of diagonal lines on the AUX disk facilitates the transfer. The diagonal line from Aqr 3º leads to 5^{S} 17° on the AUX disk. Stretch the thread from *E Equant* to 5^{S} 17° and turn the Deferens Martis disk until the center of the epicycle coincides with the thread.

3 Readers using an uncorrected Leipzig facsimile will not find the index stub for Mars, which was carelessly trimmed off in the facsimile. Consult with the author of this essay concerning a repair kit.

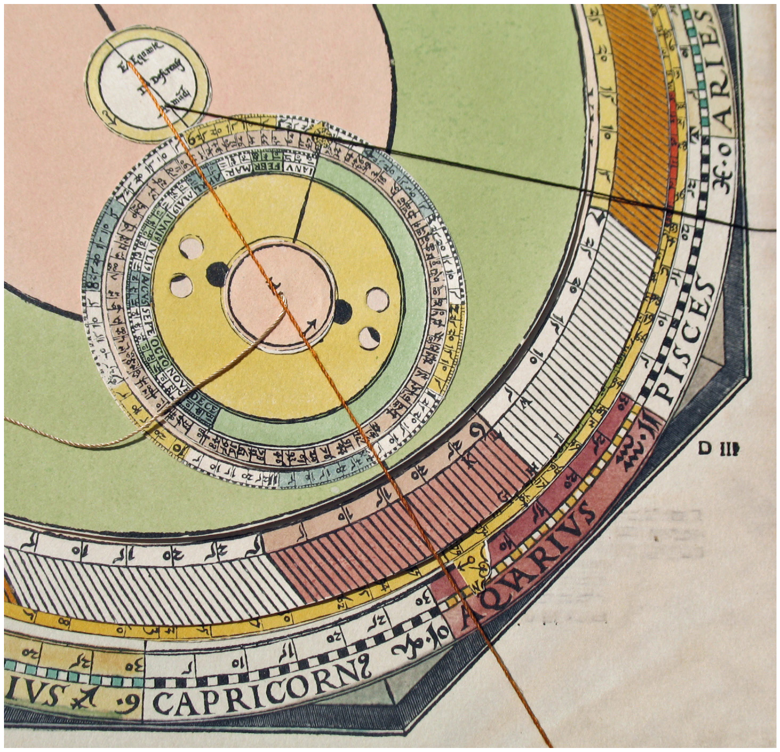


Figure 1. Detail of the Mars longitude calculation from the volvelles in Apianus's Astronomicum Caesareum. The setting is for 1593 August 10 (Julian), and the resultant longitude is Pisces 21°.

(Epicycle) Then stretch the thread from A through F (the center of the epicycle); rotate the lower epicycle disk so that index *+AUX* is also on the straight line.[4] Next, rotate the upper epicycle disk so that tab Y with the small rosette sits at 1^S 14° (for 1500, from the table of mean arguments on the facing page). Then add the increment for 92 years (again using the *complete* year), and finally, add a further increment for August 10. Stretch the thread from A through the center of the rosette on the inner epicycle disk, and this should end up at Psc 21°, matching the calculation given by Tycho Brahe.

Examination of Leovitius' *Ephemeridum novum* (1557) (a huge compilation based on the *Alfonsine Tables*) shows that Mars was almost stationary at Psc 22° during July and the early part of August, and that by August 10 it had gone into retrograde motion and had dropped back to Psc 21°. Inspection of Figure 1, a detail of the Mars longitude page from the *Astronomicum Caesareum*, shows that as the Martian epicycle itself moves counterclockwise during this time, Mars in its epicycle is effectively cancelling this eastward motion as it approaches the earth. The derived position both from the *Alfonsine Tables* and Apianus's equatorium is 5° too large. It is difficult to see how some simple parameters could be changed to correct this without causing difficulties at another time. This was indeed the case, and it required a major geometrical rearrangement on Kepler's part to fix it (which was not done merely by introducing elliptical orbits).[5]

4 In this particular configuration the positioning of *+AUX* seems trivial, but it can be a significant correction elsewhere.

5 See Owen Gingerich, "The Great Martian Catastrophe and How Kepler Fixed It," *Physics Today*, vol. 64, no. 9 (September, 2011), pp. 50–54.

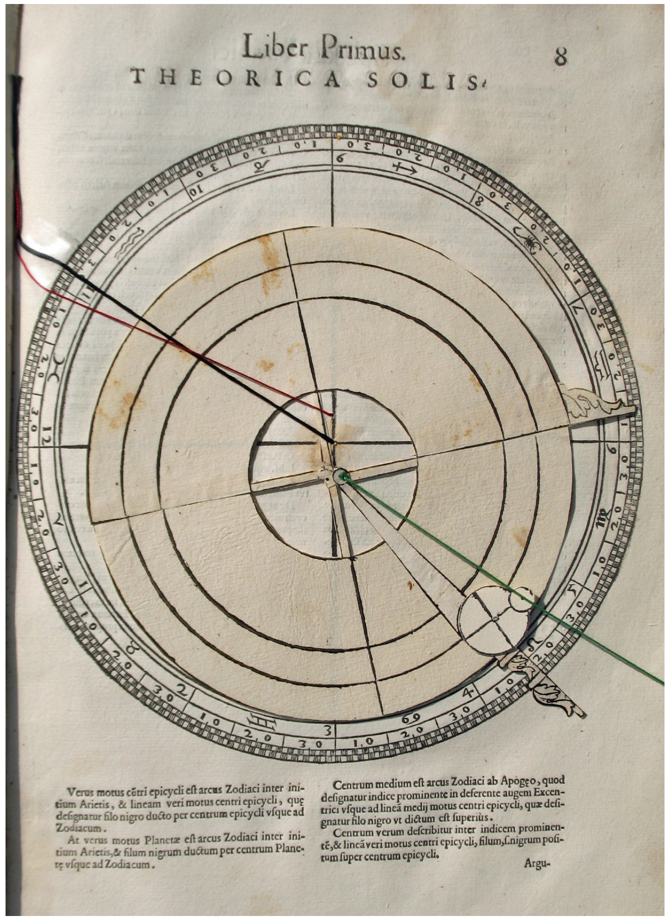


Figure 2. Volvelles used to teach the geocentric vocabulary for planetary theory in G. P. Gallucci’s Speculum uranicum (Venice, 1593).

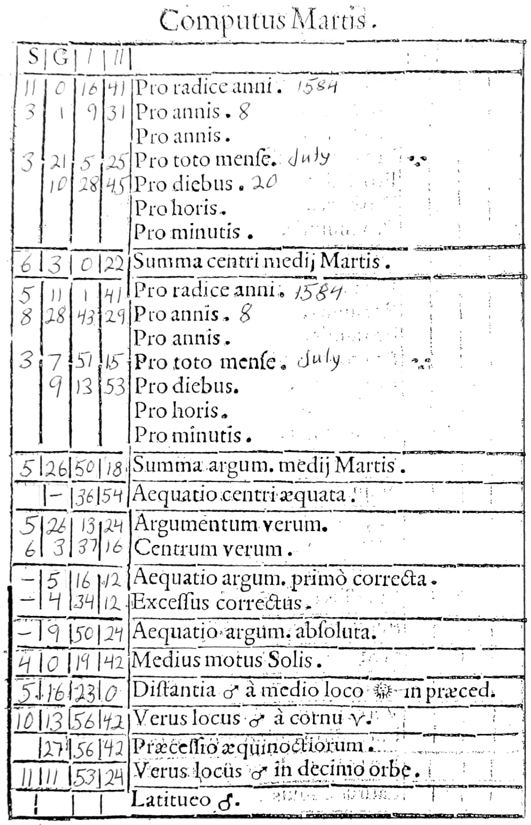
1593 August 20 (Gregorian):[6] Mars in Gallucci’s Speculum Uranicum

Because the *Speculum Uranicum* is not only full of volvelles covered with numbers, and because the geocentric cosmology is represented with a movable epicycle near the beginning of the book, one might readily suspect that this book is some sort of equatorium that has essentially fallen below the radar of the relevant experts. But such is not the case. The page in question (Figure 2) is simply a device for establishing the vocabulary for geocentric planetary astronomy, and the subsequent disks are an ingenious and not entirely successful way of presenting the requisite tables.

For Apianus, the great majority of additions (or subtractions) required in the calculation of a planetary position are carried out by sequential rotations of the disks. For Gallucci, these are pencil-and-paper calculations, and he provided a set of small printed tally sheets to guide the user (see Figure 3 for a worked example).

This section will explicate in detail how to use Gallucci’s volume to calculate the position of Mars for the same day as before. Apianus’s equatorium was based on the Ptolemaic/Alfonsine

6 In 1582 the Gregorian calendar reform took place for the Catholic countries, so Gallucci’s computational scheme is entirely based on the newer calendar.



Computus Martis.

S	G	′	″	
11	0	16	41	Pro radice anni. 1584
3	1	9	31	Pro annis. 8
				Pro annis.
3	21	5	25	Pro toto mense. July
	10	28	45	Pro diebus. 20
				Pro horis.
				Pro minutis.
6	3	0	22	Summa centri medij Martis.
5	11	1	41	Pro radice anni. 1584
8	28	43	29	Pro annis. 8
				Pro annis.
3	7	51	15	Pro toto mense. July
	9	13	53	Pro diebus.
				Pro horis.
				Pro minutis.
5	26	50	18	Summa argum. medij Martis.
	–	36	54	Aequatio centri æquata.
5	26	13	24	Argumentum verum.
6	3	37	16	Centrum verum.
–	5	16	12	Aequatio argum. primò correcta.
–	4	34	12	Excessus correctus.
–	9	50	24	Aequatio argum. absoluta.
4	0	19	42	Medius motus Solis.
5	16	23	0	Distantia ♂ à medio loco ☉ in præced.
10	13	56	42	Verus locus ♂ à cornu ♈.
	27	56	42	Præcessio æquinoctiorum.
11	11	53	24	Verus locus ♂ in decimo orbe.
				Latitueo ♂.

Figure 3. Gallucci's tally sheet, filled in by the author for the longitude of Mars on 1593 August 20 (Gregorian). Here the resultant longitude is Pisces 12°. The actual observed longitude was just over Pisces 16°.

numbers, whereas Gallucci's numerical scheme turns out to based on the Copernican numbers from Erasmus Reinhold's *Prutenicae tabulae*.

In general the Copernican system employs the same number of parameters as the Ptolemaic system, which means that the Copernican numbers will be very similar to those used for the *Alfonsine Tables*. Copernicus used the Ptolemaic observations to anchor the time-dependent parameters and followed up where possible with a set of modern observations to check if there had been any changes. In this pursuit he found that the apsidal lines of the superior planets did not simply follow the precessional changes. This meant that Copernicus, Reinhold, and Gallucci had to add another time-dependent table to handle the slow independent motions of the apsidal lines. Gallucci chose to tabulate the mean motion of the planet with respect to the moving solar poisition, and to include the moving precessional position of the apsidal line later in the calculation.

At the same time, Gallucci had to cope with the change in the calendar, which broke the rhythmic flow of years and months. Workers using the *Prutenic Tables* generally used the date of the Incarnation as the starting point (i.e., AD 0 as the initial complete year), and simply added the tabulated incremental motions to bring the setting to the desired date. For instance, the mean longitude of Mars for AD 0 was, according to the *Prutenic Tables*, 0^{Sex} 34° 7' 46''. To find the "complete" position for 1593 August 20, the user of the tables had to add the relevant tabulated increments as follows:

Radix for AD 0	0^{Sex}	34°	7'	46''	
1500 yrs		4	4	12	41
80 yrs	3	12	13	29	
12 yrs	2	16	50	1	
July	1	51	5	41	
19 days		0	9	57	24
	0	8	27	2	

(As a quick reality check here, we know that the sidereal period of Mars is about two years, and therefore Mars must move about half a degree per day; hence 10 degrees in 19 days seems about right.) But there is a calendar problem! The *Prutenic Tables* were compiled long before the Gregorian calendar reform. A user working with the Gregorian calendar must remember to subtract 10 days to account for the short year, 1582. However, Gallucci has made it easy: he includes radices for 1584 in his tabulations, which nicely skips over the short year.

The procedure for Mars requires four of these calendar-dependent calculations, two for Mars itself, another for the mean sun, and a final one for “the eighth sphere” (which is tantamount to the precession). We turn now to the small tally sheet for Mars, filling it with numbers from the page facing the Mars volvelle on f. 15. (Note that Gallucci is using the zodiacal sign-degree system rather than the sexagesimal system of the *Prutenic Tables*, and the superscript S designates a zodiacal sign.) The calculations are for *complete* years, which means that for a date in August, the “complete” month of July applies.

Figure 3 shows the tally sheet, where the upper half of the sheet gives the sequential parts for the first two time-dependent quantities, the mean center for Mars and for the mean argument of Mars. The first moves with the sidereal rate of Mars, and the second with the synodic rate, just as in the *Prutenic Tables*, except Gallucci’s radices for the first quantity are fixed to the mean sun (rather than the beginning of Aries), and without precession.

(Eccentricity) Then follow the corrections for the eccentricity and for the effect of the epicycle. (In the Apianus equatorium these effects are automatically taken care of by the varying distance of the actual epicycle.) These corrections are found on Gallucci’s Mars disk on f. 15. Precisely why the disk is a rotatable volvelle is not at all clear except that the user can bring the appropriate part of the inscribed table to a convenient place where the requisite numbers are not upside down. There are two moving indices (see Figure 4); the first, labeled *CENT* and *S PRO* needs to be set on the mean center of Mars, 6^S 3° 0' 22'' which is just past the midpoint of the circumference, 183°. We will set this using the “aequationes” in the center of the volvelle, which is the principal clever thing about Gallucci’s process. The central part of the disk has an outer ring with the signs 1 through 6 running counterclockwise and an inner ring with signs 7 through 12 running clockwise. As the cautionary caption below the disk indicates, numbers obtained from the inner signs (7–12) are positive, and the outer signs (1–6) negative. Inspection of the numbers in the double *CENT* band show they go from 0^S to 11^S 6° and back down to 0^S (taking

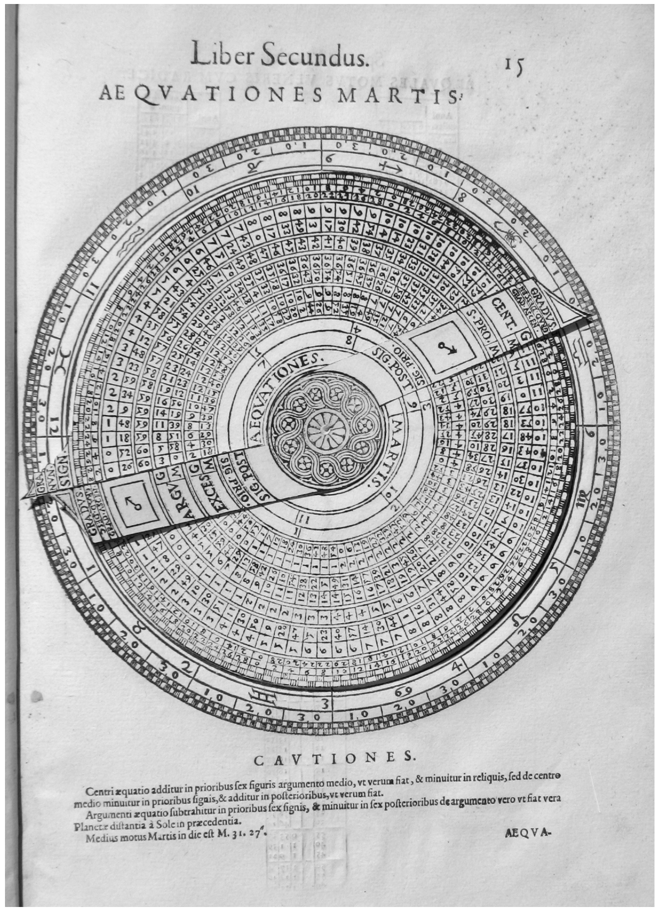


Figure 4. Detail from the disc for the longitude of Mars with the first index set for the example shown in the tally sheet (Figure 3). The correct setting for the second index would fall on top of the first, so it is simply randomly placed here.

care of the first six signs) and then in a symmetrical counterclockwise sense to –11^{S} 6° and back to zero. This exactly matches the prosthaphaeresis columns in the *Prutenic Tables*.

Because in our particular case the epicycle is near the perigee of its orbit, the eccentricity effect is very small. We find this by setting the first pointer to 5^{S} 26°, where for *CENT* we read 0° 26'. Of course we really want the result for 5^{S} 26° 50' 18'' and Gallucci is not much help with the interpolation over the two-degree step. We can say that the answer is roughly –36' 54'', which we enter on the tally sheet under *Aequatio centri æquata*. We subtract this from the argumentum and add it to the centrum, entering them into the next two lines of the tally sheet.

But wait! The first index points to a second quantity, labeled as *S Pro* or "scrupula proportio," but with no place to enter it on the tally sheet. We can see on Figure 4 that the entry yields 60', which in the sexagesimal system equals unity, and as the process unfolds, a correction will have maximum effect with no intricate multiplication.

(Epicycle) What follows next both in the *Almagest*'s instructions and in the *Prutenicae tabulae* is one of Ptolemy's cleverest moves. Imagine that the planetary epicycle is at the apogee. Ptolemy could have tabulated the visual angle from the earth for every degree that the planet might have as it goes around in the epicycle, say 180 positions since symmetry reduces the total number of 360. As the epicycle moves away from the distant apogee point, the angles will become gradually larger as the epicycle comes nearer. So another 180 tables each with 180 entries would be required to take into account the varying distance of the epicycle itself. This would make a truly huge size for a book of tables since such a 180 × 180 set would be required for each planet. What Ptolemy did was to reduce the two dimensional table to a product of pair of linear functions, one related to the synodic rate of the epicycle and the other to the sidereal rate of the deferent circle.

The functions related to the epicycle come with second index on Gallucci's Mars disk. These are abbreviated *Argu* and *Exces* for Argumentum and Excess, and the argument for the setting is the entry on the tally sheet labeled *Centrum verum*, 6^S 3° 37' 16''. At 6^S 2°, the respective numbers are 5° 58' and 4° 30'. Once again a fairly tedious numerical interpolation is required, here between 6^S 2° and 6^S 4°, leading to 5° 16' 12'' and 4° 34' 12''. As Gallucci explains in his text, the second number must be multiplied by the *S Pro* found with the first index, which in this case is unity and the number is unchanged. The label for the line on the tally sheet, *Excessus correctus,* gives the hint that a corrected number is required. (Gallucci included a foldout base-60 multiplication table in case that was needed.) The sum of these two corrections go into the tally sheet line *Aequatio argum. absoluta.* This quantity is then used to correct the *Argumentum verum*; for numbers determined from the second index, the sign convention is reversed with respect to the first index. Thus in our case the two corrections are negative, and hence they are subtractive. Note that their sum is marked with a minus sign when entered in the line *Aequatio argum. absoluta*, which is therefore subtracted from the *Argumentum verum* to obtain the *Distantia* etc.

(Mean motion and Precession) Two other time dependent quantities are needed to complete the calculation, the mean position of the sun, and the precession. Each of these have full openings with disks in Gallucci's scheme, although in the solar case, because only the mean sun's position is required, the disk is not used, and for the precession only a rudimentary use of the disk is required. The *Distantia* line is subtracted from the *Media motus Solis* to get the true location of Mars from the horn of Aries, and finally the precession is added to obtain 11^S 11° 53' 24'', that is 341° 53' 24'', the "true position of Mars in the tenth sphere." This position is in satisfactory agreement with Tycho's Copernican calculation, which misses his observed position by over 4° and in the opposite sense from Apianus's even more erroneous Alfonsine calculation.

General Evaluation of the Speculum Uranicum

When Gallucci's *Speculum Uranicum* was published both Galileo and Kepler were alive. Both would become enthusiastic converts to the Copernican system, and the two most important figures in persuading a skeptical public that the heliocentric cosmology was a physically real description of the world. But in 1593 neither had yet published anything hinting of the earth as a spinning planet. In the closing decades of the 16^{th} century the overwhelming majority of astronomers accepted *De revolutionibus* as a recipe book for computing the positions of the planets, but not as an actual physical cosmology. As the blurb at the center of Copernicus' title page put it, "You have in this recently created and edited work the motions of the stars, both fixed and planets, re-established from ancient observations and recent ones as well, and moreover embellished with new and admirable devices. You also have here the most expeditious tables from which you can very easily calculate for any time. Therefore buy, read, profit."

The expanded version of Copernicus' "most expeditious tables" was Erasmus Reinhold's *Prutenicae tabulae* (Tübingen, 1551), and these were tastefully independent of cosmology. Paul Wittich's well-annotated copy of *De revolutionibus*, now in the Vatican Library, has supernumerary pages with geocentric equivalences to Copernicus' heliocentric arrangement.[7] In this category was J. A. Magini's *Novae coelestium orbium theoricae congruentes cum observationibus N. Copernici* (Venice, 1589), a thoroughgoing geocentric text. These works provided the intellectual framework into which Gallucci's supposedly "handy version" of the *Prutenic Tables* fit.

My own encounter with these "handy tables" was exceedingly tedious because of the host of computational errors that beset my attempt to understand how it worked. It took some time to appreciate that this was not a volvelle book somehow distantly related to Apianus' masterpiece, and that it was in fact a distinct version of the *Prutenic Tables*. There was a great moment of triumph and relief when I discovered that the result from Gallucci's book agrees reasonably closely with the Psc 12° 8' in Magini's *Ephemeris* (Venice, 1582), a result which was more or less accidental because the time of day was not considered explicitly. It would have been much easier to use the *Prutenicae tabulae* directly, and the whole process was in fact facilitated by having Reinhold's book immediately at hand.

While the circular disks show clearly how the corrections function, now additive, now subtractive, they hardly have enough significant figures for accurate interpolation, and even Gallucci remarked that it would have been better to have engraved plates rather than woodcut disks. In fairness to Gallucci, I must remark that besides the disks for planetary longitudes, there are similar disks for calculating the latitudes of the planets, and the volume concludes with a substantial amount of astrological instructions and star tables.

Nevertheless, the fact that only one tally sheet for each planet was provided suggests the set of small tally sheets was intended for one-time use as the owner personalized his copy by working out the planetary positions for his own horoscope. The volume could then take its place as a vanity press or trophy book. Today it remains as a trophy specimen for that transitional period when Copernicus was appreciated for his numbers but scarcely for his cosmology.

7 See Owen Gingerich and Robert S. Westman, *The Wittich Connection: Conflict and Priority in Late Sixteenth-Century Cosmology, Transactions of the American Philosophical Society*, vol. 78, part 7, 1988.

The Georgian Star

Michael Hoskin

In June 1767, three months after the death of his father, Jacob Herschel of Hanover took leave from his prestigious post in the Court Orchestra and went to visit his brother William in the fashionable English resort of Bath, where there were rich pickings for enterprising musicians. Jacob, the eldest son, was the most gifted of a talented family, and their sister Caroline tells us that

> his stay must have been prolonged on account of waiting till he had had the honour of playing before their Majiesties; for which (in consequence of having composed and dedicated a Set of Sonatas to the Queen) he was informed he would receive a summons.

Jacob and George III, King of Great Britain and Elector of Hanover, both understood the conventions of patronage: if the patron accepted the dedication, he was under obligation to reward the dedicator. Jacob's salary was increased by 100 thalers, and instructions given that this was not to exclude him from the increases that occurred whenever a musician quit the Court Orchestra.[1]

King George was an intelligent and courteous man, and he had been impressed by this first encounter with a Herschel. A decade later George Griesbach, eldest son of Jacob's eldest sister Sophia, arrived in London as a recruit to Queen Charlotte's band, and one evening he played a concerto by Jacob. The King asked who was the composer.

> "My uncle." "Who is your uncle?" "Herschel, at Hanover." Here the King left me and went to tell the Queen whose nephew I was.[2]

Three years later, in March 1781, William Herschel was struggling to reconcile his professional work as a Bath musician with his amateur passion for astronomy. He was currently engaged in a systematic examination of all the naked-eye stars (and many more besides), partly to familiarize himself with the starry heavens, partly in the search for 'double stars'. To establish the distances of stars was one of the current challenges to astronomers, and the solution lay in measuring how much a star appears to move as we on Earth orbit the Sun: the more a star seems to alter its position in the sky, the nearer it is to the observer. The problem was that the stars were so very distant that this 'parallax' was minute, and almost impossible to measure with instruments that must warp over time with changes in temperature and humidity (to say nothing of the complications resulting from alterations in atmospheric refraction and so forth). Galileo had popularised a suggestion[3] that if two stars lay in almost the same direction from Earth, and if one star was near and the other distant, then the movement of the nearer measured relative to the distant would circumvent these problems; and Herschel was assembling a collection of double stars that astronomers might use in this way.[4]

1 *Caroline Herschel's autobiographies*, ed. by Michael Hoskin (Cambridge, 2003; hereafter: *CHA*), 44, 109.

2 Michael Hoskin, *The Herschel partnership* (Cambridge, 2003; hereafter: *Partnership*), 50.

3 Harald Siebert, "The early search for stellar parallax: Galileo, Castelli, and Ramponi", *Journal for the history of astronomy*, xxxvi (2005), 251–71.

4 See Michael Hoskin, *The construction of the heavens: William Herschel's cosmology* (Cambridge, 2012), Part I, chap.

Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans, ed. Alexander Jones and Christián Carman, 2020, DOI: 10.5281/zenodo.3928498, pp. 281–289. Chapter DOI: 10.5281/zenodo.3975755.

The instrument he was using was a 7ft Newtonian reflector of his own design and construction. He had ground and polished the mirror with his own hands, and by his dedicated practice at this art he had at last achieved a mirror of wholly exceptional quality.[5] As a result, during the evening of 13 March he was able to see immediately that the object he was then examining was no ordinary star, but "a curious either nebulous star or perhaps a comet". His interest aroused, he returned to the object four days later, and found that it had already moved position relative to stars in the same field of view: it was therefore not a true star, which would have been so distant as to seem motionless, but a nearby member of the solar system.[6]

By this time, the musician-astronomer had come to the notice of several men influential in the scientific world. As long ago as 1774 he had met Thomas Hornsby, professor of astronomy at Oxford. More recently the Astronomer Royal, Nevil Maskelyne, had visited him in Bath.[7] His most loyal ally was Dr William Watson Jr, a local physician of distinction and Fellow of the Royal Society, who gladly communicated to the Society Herschel's announcement of his observation of what mathematicians eventually showed to have the orbit of a planet, the first to be discovered in historic times. But the man with the greatest influence in British scientific life—and who had the ear of the King himself—was Sir Joseph Banks, the President of the Royal Society, who met Herschel when he attended a meeting of the Society in the spring of 1781.[8]

It was a sign of the extraordinary quality of Herschel's telescope, that neither Maskelyne at Greenwich, nor Hornsby at Oxford, could with their professionally-made instruments distinguish the planet from an ordinary star; and it was some time before either could be certain as to which of the objects in that particular region of sky was deemed special. Within weeks of the discovery Maskelyne was inclined to the view that it was indeed a planet in a near-circular orbit; as much as a year later Hornsby still preferred to think of it as a comet.[9]

Whatever the truth, the discovery was significant enough for Herschel to be honoured in November 1781 with the Copley Medal of the Royal Society, and to be elected a Fellow soon afterwards. In January his first catalogue of no fewer than 269 double stars was read to the Society;[10] many of them defied resolution in the best telescopes of other observers. It was evident that an astronomical talent of quite exceptional quality had arrived on the scene. The discovery of the planet/comet was an isolated *tour de force* never to be repeated; the unprecedented list of double stars signalled an observer using exceptional instrumentation with long-term commitment.

Yet Herschel was an amateur, forced to spend most of his waking hours in his profession of music. Only eight days after first coming across the planet/comet, he had had to turn his at-

2; and *idem*, "Herschel and Galileo", *Actes du XIe Congrès International d'Histoire des Sciences*, iii (1968), 41–44.

5 On Herschel's telescopes, see J. A. Bennett, "'On the power of penetrating into space': The telescopes of William Herschel", *Journal for the history of astronomy*, vii (1976), 75–108.

6 The many accounts of this discovery include J. A. Bennett, "Herschel's scientific apprenticeship and the discovery of Uranus", in *Uranus and the outer planets*, ed. by Garry Hunt (Cambridge, 1982), 35–53.

7 Michael Hoskin, *Discoverers of the universe: William and Caroline Herschel* (Princeton, 2011; hereafter: *Discoverers*), 44.

8 William Herschel, "Memorandums from which an historical account of my life may be drawn", RAS MS Herschel W.7/8.

9 Hoskin, *Discoverers*, 50; Constance A. Lubbock, *The Herschel chronicle* (Cambridge, 1933; hereafter *Chronicle*), 79–81.

10 William Herschel, "Catalogue of double stars", *Philosophical transactions*, lxxii (1782), 112–62.

tention to a performance of a Handel oratorio.[11] Could anything be done to secure his financial future and enable him to dedicate himself wholly to astronomy?

Hornsby and Maskelyne were in their prime, and in any case Herschel was neither an academic nor a mathematician. But in 1768 the King had embarked on the construction of a private observatory at Kew, near London, specifically in order to view the transit of Venus across the face of the Sun that was to occur the following year. He equipped it with instruments by leading makers — Adams, Shelton and Sisson — and appointed as Superintendent his former tutor, Stephen Charles Triboudet Demainbray. Born in 1710, Demainbray was one of the more successful of the itinerant lecturers in 'natural philosophy' of the mid-century, and after teaching in France for some time he had settled in London in 1754, where the following year he was paid no less than £210 for giving a course of natural and experimental philosophy to the future King and his brother Prince Edward.[12] The viewing of the transit was a success, the King being attended by Demainbray, Jeremiah Sisson the instrument maker, George Wollaston and others (although the results of their observations were never published).[13] Thereafter, however, Demainbray looked on his post as a sinecure, and limited himself to recording the weather,[14] checking the clocks that provided the time at Parliament and other public buildings in London,[15] and perhaps giving a few lectures. By early 1782 he was in his seventies and frail in health. Here was a post ideally suited to Herschel, in effect that of astronomer to the Court with freedom to pursue his research except when the Royal Family wished to profit from his knowledge. But how best to direct the King's mind in this direction? A precedent for the discovery of Herschel's 'star' was Galileo's discovery of the moons of Jupiter, and he had named them for the Medici family and been handsomely rewarded. Banks saw here an example to be followed: Herschel should name his 'star' for the King, and his allies would suggest to the King that the post at Kew — when it fell vacant — would be a suitable reward.

This well-laid scheme was overtaken by events: Demainbray died before the naming of the 'star' could be arranged. As Banks wrote to Watson on 23 February 1782:

> I wished the new star, so remarkable a phenomenon, to have been sacrificed somehow to the King. I thought how snug a place his Majesty's astronomer at Richmond [i.e., Kew] is and have frequently talked to the King of Mr Herschel's extraordinary abilities. I knew Demainbray was old but as the Devil will have it he died last night. I was at the [royal] Levy this morning but did not receive any hopes. I fear [the time] has passed by which a well timed

11 Hoskin, *Discoverers*, 60–1.

12 On Demainbray, see Alan Q. Norton and Jane A. Wess, *Public & private science: The King George III Collection* (Oxford, 1993), chap. IV: "The career of S. C. T. Demainbray (1710–82)". Demainbray was paid £210 on 14 May 1755 for a "Course of Natural & Experimental Philosophy for their Royal Highnesses" the Prince of Wales and Prince Edward (p. 106). The same year he was granted a pension of £100 from King George II, but had difficulty in securing payment, and so he was forced to return to giving courses of public lectures in London (pp. 106, 111). He petitioned, apparently without success, to be librarian to the Prince of Wales (pp. 106, 109).

An older account, based on family papers, is Gibbes Rigaud, "Dr. Demainbray and the King's Observatory at Kew", *The observatory*, v (1882), 279–85. According to this account, "When His Majesty George III came to the age of eighteen [1756], and governors and teachers were dismissed, Dr. Demainbray was pleased to have the sole trust of teaching him sciences till he came to the throne; Queen Charlotte, after her marriage with the King, also became his pupil and listened to his lectures in philosophy..." (p. 281).

13 The archives of Kew Observatory are held at King's College London. The transit observations are in K/MUS 1/1.

14 K/MUS 1/7 contains daily temperature, barometric and rainfall readings taken at Kew between 1773 and 1783.

15 Rigaud, *op. cit.* (ref. 12), 282.

> compliment might have helped if the old gentleman had chose to live long enough to have allowed us to have paid it.[16]

The following day, Herschel wrote to Banks thanking him for his advocacy on his behalf, and declaring his ambition that the new star should be connected to the King in whatever manner Banks should think best.[17]

The King's unwillingness to appoint Herschel to Kew was, it later transpired, because he had already promised the post to Demainbray's son.[18] An honourable man, he would not go back on his word; yet the commitment put him in a quandary as to just how else he could make it possible for Herschel to give up music for astronomy. King George had not needed Banks to tell him of Herschel's remarkable discovery: he had discussed it with Demainbray months before.[19] Not only that, but "Col^l [John] Walsh in particular informed my Brother that from a conversation he had had with his Majesty it appeared that in the spring he [William] was to come with his 7 feet Telescope to the King".[20]

The King was more than happy to have the object named after him, but how was he to respond? The longer he pondered the problem, the more he looked forward to meeting the man of whom he had heard so much, and to seeing for himself how the heavens appeared when viewed through Herschel's telescope. As Watson told Banks on 27 March 1782,

> It gives me likewise great pleasure to be able to inform you that since Dr Demainbray's death, the King has again twice spoken to Mr Griesbach in relation to Mr Herschel, & told him that Mr Herschel was to come to him as soon as the Concerts at Bath were over. These are very encouraging circumstances, & make me still hope that the King has some notion of making him Demainbray's successor....[21]

The next day, Caroline Herschel records, Griesbach "arrived to pay us a visit and brought the confirmation that his Oncle was expected with his Instrument in Town".[22] In his Memoranda for April 1782, Herschel notes that he "was informed by several that the King awaited" him.[23] Watson told Banks early in the month:

> The King the first time he saw him [Griesbach] at Windsor asked him after his *uncles* at Bath, & how Mr Herschel's telescope went. To which Mr Griesbach answered that his Uncle was preparing them for the inspection of his Majesty. The King, you see, has very often made enquiries after Mr H since Dr Demainbray's Death, & indeed, I find since, oftner than I have

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16 Lubbock, *Chronicle*, 112.

17 Herschel to Banks, 24 February 1782, copies of Banks letters in the British Museum (Natural History), made for Dawson Turner in 1833–34, D.T.C. 2, 94–95.

18 Lubbock, *Chronicle*, 113. There is little doubt that what Lubbock says is correct, although she cites no source, but just when the King made his promise publicly known is unclear. Certainly Watson continued to cherish hopes of Kew for many weeks to come, as we shall see.

19 RAS MS Herschel W.1/13.D.14, letter of Demainbray to Herschel, 12 August 1781. *Cf.* Hoskin, *Discoverers*, 59.

20 *CHA*, 64.

21 Letter in the British Museum — Natural History, Dawson Turner Collection, ii, 108, cited by Norton and Wess, *op. cit.* (ref. 12), 35.

22 *CHA*, 64.

23 Herschel, "Memorandums" (ref. 8).

> mentioned to you, which makes me hope he has him in his eye yet....[24]

On 10 May Colonel Walsh wrote to Herschel:

> In a conversation I had the Honour to hold with His Majesty the 30th ult° concerning You and Your memorable Discovery of a new Planet, I took occasion to mention that You had a twofold claim, as a native of Hanover and a Resident of Great Britain, where the Discovery was made, to be permitted to name the Planet from his Majesty. His Majesty has since been pleased to ask me when You would be in Town....[25]

In preparation for his London visit Herschel had devised a portable stand for the 7ft,[26] and an ambitious list of double stars to show the King;[27] and on the Monday 20 May he took the coach to London where he was to stay with Dr William Watson Sr in Lincoln's Inn Fields.[28]

How he spent the days immediately following we do not know, but on the Saturday he dined with Colonel Walsh in the company of Maskelyne and Alexander Aubert.[29] Aubert was a respected amateur astronomer, who had recently succeeded in confirming Herschel's claim that the Pole Star was a double.[30] He was to become another of Herschel's loyal allies, and in 1786 made him the exceptional gift of a timepiece by John Shelton, which keeps time to this day.[31]

On Sunday the 26th Herschel had an audience with the King and Queen, to whom he presented a drawing of the solar system in which the new planet doubtless featured prominently. "My telescope in three weeks time is to go to Richmond, and meanwhile to be put up at Greenwich."[32] At Greenwich it would be professionally tested against the instruments of the Royal Observatory; at Kew the King himself could compare it with the ones he had bought for his own observatory.

At this stage it seems that the King had said nothing about Demainbray's successor, and Herschel — and certainly Watson — were under the impression that the Superintendency was still undecided.[33] But first the Greenwich trial lay ahead, in which Herschel's home-made reflector would seek endorsement from the friendly but demanding Astronomer Royal. Accordingly, later on the Sunday Herschel took his reflector to Greenwich. To Caroline he wrote: "Tell Alexander [their brother] that everything looks very likely as if I were to stay here.... My having seen the

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24 British Museum – Natural History, Dawson Turner Collection, ii, 118–19, cited by Norton and Wess, *op. cit.* (ref. 12), 35.

25 Walsh to Herschel, 10 May 1782, RAS MS Herschel W.1/13.W.5.

26 *CHA*, 65: "A new 7 ft Stand and Steps were made to go in a moderate sized box for to be screwed together on the spot where wanted."

27 Namely γ Vir, γ Leo, π Boo, 54 Leo, Castor, α Her, β Cyg, γ And and γ Vir, RAS MS Herschel W.2/1.4, f. 12. Rightly or wrongly, Herschel assumed the King would be a serious observer and interested to see examples of Herschel's more challenging discoveries.

28 Caroline gives his day of departure as the Tuesday, *CHA*, 65.

29 Lubbock, *Chronicle*, 115.

30 Hoskin, *Discoverers*, 48.

31 Aubert to Herschel, 19 October 1786, RAS MS Herschel W.1/13.A.26.

32 Lubbock, *Chronicle*, 114.

33 Watson still entertained hopes as late as 29 June, when he wrote to Banks: "... nothing remains now to be done in order to gain him the Post he so much covets, than to inform the King of these particulars, & of Mr Herschel's ardent wishes to serve his Majesty by succeeding the late Dr Demainbray" (British Museum – Natural History, Dawson Turner Collection, ii, 144, cited by Norton and Wess, *op. cit.* (ref. 12), 35).

King need not be kept secret, but about my staying here it will be best not to say anything but only I must remain here till His Maj. has observed the Planets with my telescope."[34] Since he warned Caroline in the same letter that she was unlikely to see him in less than a month, the "staying here" presumably refers to a permanent appointment that William anticipated, and this was no doubt the Superintendency at Kew.

On Wednesday the 29th Herschel assembled his telescope at Greenwich, ready for the trial. He took the opportunity to assess the opposition: "I tryed the acchromatic telescope of D^{r} Maskelyne ... with [magnification] 920 very strong aberration & ill defined. My reflector in tollerable fine weather is hardly so bad with 3168. I tried also D^{r} Mask. 6 feet Reflector of Shorts but it would bear no higher power than 3 or 400, upon α Lyrae."[35] The omens were favourable. On the Friday he attended the King's regular concert at which George Griesbach performed. "The King spoke to me as soon as he saw me, and kept me in conversation for half an hour."[36] The next two nights Herschel was at Greenwich. On Saturday 1 June he observed with Maskelyne and his assistant Joseph Lindley.[37] Herschel was more than pleased with the outcome. "D^{r} M tried to see the small star of ε Bootis in his Achromatic but with the deepest comon night piece could not perceive it. Nor could I see it with the same piece. We saw it both extremely well in mine."[38] On the Sunday they were joined by Aubert, with equally satisfactory results. A delighted Herschel reported to his sister next day:

> We have compared our telescopes together, and mine was found superior to any of the Royal Observatory. Double stars which they could not see with their instruments I had the pleasure to show them very plainly, and my mechanism is so much approved of that Dr Maskelyne has already ordered a model to be taken from mine and a stand made by it to his reflector. He is, however, now so much out of love with his instrument that he begins to doubt whether it *deserves* a new stand.[39]

On the Tuesday Herschel dined at Lord Palmerston's, and on the Wednesday with Banks.[40] It seems he must have gone on to Greenwich after dinner with Banks, for he discovered a new double star that night.[41] On Thursday the 6th he was at the King's concert. "As soon as the King saw me he came and spoke to me, about my telescope but he has not yet fixed a time when he will see it."[42] A possible reason for the delay was the Court mourning that was shortly to begin, and Caroline was to send her brother suitable clothes. Herschel left his 7ft at Greenwich for the weekend, and went to visit Aubert. Now it was his turn to make trial:

> ... we have tried his Instruments upon the double stars and they would not at all perform what I had expected, so that I have no doubt but mine is better than any M^{r} Aubert has; and if

34 Herschel to Caroline Herschel, 25 May 1782, RAS MS Herschel W.1/8.

35 RAS MS Herschel W.4/1.3.

36 Lubbock, *Chronicle*, 115.

37 Lindley, known to Herschel as Linley, was Maskelyne's assistant from 1781 to 1786, Eric G. Forbes, *Greenwich Observatory*, i: *Origins and early history (1675–1835)* (London, 1975), 150.

38 RAS MS Herschel W.4/1.3.

39 Lubbock, *Chronicle*, 115.

40 *Ibid.*, 116.

41 RAS MS Herschel W.4/1.3.

42 Herschel to Alexander Herschel, 10 June 1782, RAS MS Herschel W.1/9.

> that is the case I can now say that I absolutely have the best telescopes that were ever made.[43]

Which was no more than the truth.

For the rest of June, Herschel languished in London waiting on the King's pleasure, and making occasional visits to Greenwich. On Tuesday the 11th and Friday the 14th he was there making observations, and on the Saturday a distinguished company once more assembled at the Observatory to look through his telescope: Maskelyne, Aubert, Playfair (perhaps John Playfair, the Scottish mathematician and geologist), Professor Antony Shepherd of Cambridge, and John Arnold the great watchmaker. On the Sunday Herschel seems to have been able to observe alone with only Lindley for company, and he was again at work on each of the following three nights.[44]

The 7ft was to remain at Greenwich for some days more, but we are poorly informed as to how Herschel spent the time, for he relayed news to Bath in letters to Watson that are lost. Watson for his part was fearful that his friend's diffidence was causing him to miss an opportunity that might never come again. Banks had promised to approach the King on Herschel's behalf, but if necessary Herschel himself must make a move.

> The King has shewn you every outward mark in his behaviour of predilection for you. But he might justly think that he ought previously to know that you are willing to accept of the place, before he makes you the offer. For want of knowing precisely your situation & wishes, how should he know but that you might be from your situation at Bath in such flourishing circumstances, as to make you above accepting of the Post of his Astronomer at Kew.... I should certainly take the first opportunity ... humbly to request that you might succeed the late D[r] Demainbray at Kew provided his Majesty thought of appointing [a] successor, & that you should look upon such a Post as the most happy event of your Life.[45]

Herschel's reply managed to pacify Watson, who replied on the 23rd: "you are perfectly right to remain quietly in Town & abide the event...." But Watson was concerned that Hornsby, who had not been at Greenwich to see for himself the excellence of Herschel's reflector, had only an indifferent opinion of Herschel because of mistakes the inexperienced amateur had made in his reporting of the positions and movement of the 'star'. Hornsby, Watson insisted, must be converted into an ally before he had the opportunity to offer the King a damaging assessment; and if this meant delaying the Kew meeting with the King, so be it.[46]

We hear nothing more of the Hornsby problem, which probably existed more in the mind of Watson than in reality. As we have seen, Hornsby had met Herschel as early as 1774 and had gone out of his way to be helpful,[47] and as a university professor he was no doubt able to discern a talent masked by limitations of education. But Watson's appeals to Herschel to take the initiative began to bear fruit. Writing on 29 June, Watson says he is glad that Herschel is

> well convinced of the necessity that the King should be apply'd to. He has done every thing on his side to shew his partiality towards you & it cannot be expected that he should conde-

43 *Ibid.*

44 RAS MS Herschel W.4/1.3.

45 Watson to Herschel, 12 June 1782, RAS MS Herschel W.1/13.W.16.

46 Watson to Herschel, 23 June 1782, RAS MS Herschel W.1/13.W.17.

47 Hornsby to Herschel, 22 December 1774, RAS MS Herschel W.1/13/H.23.

> scend to offer before he knows that his offer will be accepted.[48]

He approves of Herschel's plan to approach Dr William Heberden as an intermediary. Heberden had been personal physician to the Queen since 1761, and if Banks failed to make the promised approach to the King then Heberden would be a second friend at Court.

It is likely that the King's reluctance to make an offer stemmed not so much from the need to maintain royal dignity in negotiations as from uncertainty as to what form of offer he could reasonably make. Caroline Herschel later recounted[49] that the possibility of her brother's being astronomer to the Court in Hanover was mooted, but as the proposed salary was only £100 per annum — less than a quarter the income Herschel was currently making in Bath — we hear no more of this. But the ideal solution now occurred to the King. Not only George Griesbach but his brother Henry were members of Queen Charlotte's band, entertaining guests during dinner at Buckingham House or Windsor Castle.[50] An astronomer resident near Windsor would solve the problem of how to entertain the guests when dinner was over, as well as guaranteeing instruction for the King when he was in residence. But was Herschel equal to the task? To decide this question the would-be professional astronomer was invited, not to Kew, but to Windsor Castle. Accordingly, on Tuesday 2 July Herschel set up his reflector at Queen's Lodge (where the Royal Family lived in preference to the dilapidated castle):

> This evening His Majesty and all the Royal Family observed Jupiter Saturn and several double Stars with my 7ft Reflector. His Majesty had ordered three of his Instruments (viz a 10 or 12 f^{t} Achromatic of Dollond's a 3½ Achromatic a Short's reflector) to be brought in order that they might be compared with mine; and my Telescope shewed the heavenly bodies much more distinct than the other Instruments. His Majesty saw ε Bootis with [magnification] 460 and the Pole Star with 932. The Queen found the Newtonian construction very convenient.[51]

His delighted letter next day to Caroline tells us more:

> Last night the King, the Queen, the Prince of Wales, the Princess Royal, Princess Sophia, Princess Augusta, &c, Duke of Montague, Dr Hebberdon [Heberden], Mons Luc &c. &c, saw my telescope and it was a very fine evening. My Instrument gave a general satisfaction; the King has very good eyes & enjoys Observations with Telescopes exceedingly.
>
> This evening as the King & Queen are gone to Kew, the Princesses were desirous of seeing my Telescope, but wanted to know if it was possible to see without going out on the grass, and were much pleased when they heard that my telescope could be carried into any place they liked best to have it. About 8 o'clock it was moved into the Queen's Apartments and we waited some time in hopes of seeing Jupiter or Saturn. Mean while I shewed the Princesses & several other Ladies that were present, the Speculum, the Micrometers, the movements of the Telescope, and other things that seemed to excite their curiosity. When the evening appeared to be totally unpromising, I proposed an artificial Saturn as an object since we could not have the real one. I had beforehand prepared this little piece, as I guessed from the

48 RAS MS Herschel W.1/13.W.18.

49 In a letter to Lady Herschel, wife of her nephew John, 3 February 1842, *Memoir and correspondence of Caroline Herschel*, by Mrs John Herschel, 2nd edn (London, 1879), 320–2.

50 In time all five of the brothers were to be members of the band. On this see the biography of their mother Sophia in Michael Hoskin, *The Herschels of Hanover* (Cambridge, 2007).

51 RAS MS Herschel W.4/1.3.

> appearance of the weather in the afternoon [that] we should have no stars to look at. This being accepted with great pleasure, I had the lamps lighted up which illuminated the picture of a Saturn (cut out in pasteboard) at the bottom of the garden wall.
>
> The effect was fine and so natural that the best astronomer might have been deceived. Their Royal Highnesses and other Ladies seemed to be much pleased with the artifice. I remained in the Queen's Apartments with the Ladies till about half after ten, when in conversation with them I found them extremely well instructed in every subject that was introduced and they seem to be the most amiable Characters. To-morrow evening they hope to have better luck & nothing will give me greater happiness than to be able to shew them some of those beautiful objects with which the Heavens are so gloriously ornamented.[52]

In short, the trial had been a great success. Herschel and the King were both possessed of a natural charm and got on well together, and no doubt the encounter gave Herschel ample opportunity to hint at how pleased he would be to be able to dedicate himself to astronomy. And so it was that within days the King sent to Herschel no less an emissary than General Heinrich Wilhelm von Freytag.[53] Herschel was invited to become astronomer to the Court at Windsor with a 'pension' of £200 per annum, free to pursue his researches, his only obligation being to live close to the Castle and to be available to the Royal Family and their guests on request.

Watson expressed his delight at the news, and encouraged Herschel to ask Freytag to intercede with the King should he have any counter-requests of his own.[54] Herschel, as Caroline tells us, had had no hesitation in accepting the King's offer, for he could not bear the thought of returning to the dreary round of musical performances and the endless lessons for pupils without ability.[55] For some days he lodged with George Griesbach while his 7ft remained at Queen's Lodge, and on the 9th and on each of the six nights from the 18th to the 23rd of July he used it to continue his search for double stars.[56] His days he evidently spent searching for accommodation that had buildings for workshops and space to erect his telescopes, and he quickly found what he wanted in the village of Datchet, a couple of miles from the castle.[57] This done, he returned to Bath where with the help of Caroline and their brother Alexander he packed his instruments and belongings in a matter of days, and arranged their transport to Datchet. On the night of 1 August all three siblings slept in the inn at Datchet, and they awoke on the 2nd to find the wagon had safely arrived.[58] The next evening Herschel made his first observations from his new home.[59] There was no time to lose: nearly 44 years of age, he was at last a professional astronomer.

52 Lubbock, *Chronicle*, 118.

53 Lubbock, *Chronicle*, 119.

54 Watson to Herschel, 14 July 1782, RAS MS Herschel W.1/13.W.19.

55 *CHA*, 66.

56 RAS MS Herschel W.4/1.3.

57 Hoskin, *Discoverers*, 68.

58 Hoskin, *Discoverers*, 69–70.

59 RAS MS Herschel W.4/1.3.