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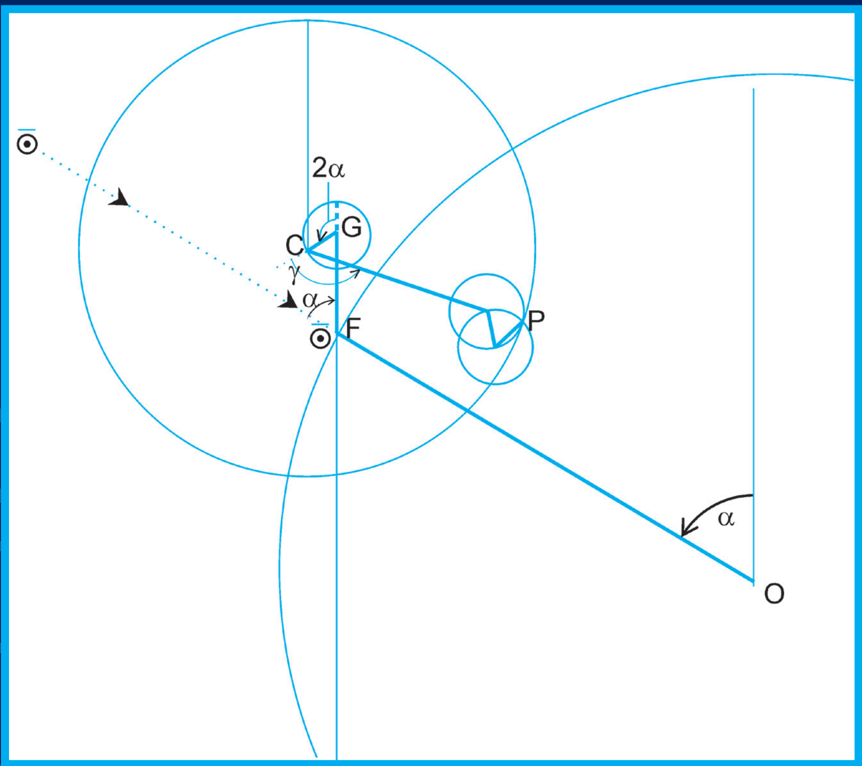
Islamic Astronomy and Copernicus



Prof. Dr. F. Jamil Ragep

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Author
Prof. Dr. F. Jamil Ragep

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Vedat Dalokay Caddesi No: 112, 06670
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FOREWORD

The Turkish Academy of Sciences (TUBA) is an autonomous apex body for the development and promotion of sciences in Turkey. The origins of the Academy go back to the Ottoman society called the "Encümen-i Dāniş" (Society of Scholars), which was founded in 1851 and known as the first Turkish science academy in the modern sense. TUBA is the single national academy in Turkey, and its interest covers all scientific fields, which are grouped under the following three categories: a) basic and engineering sciences, b) health and life sciences, and c) social sciences and humanities. TUBA is committed to contributing to the promotion of scientific research by organizing working groups, offering grants and awards, preparing, and supporting the preparation of, scientific reports, as well as by collaborating with sister academies all around the world.

Supporting and publishing studies on the history of science are among TUBA's priorities with the intention of increasing awareness regarding scholarly and scientific exchanges across cultures throughout history. In this respect, as TUBA's president, I would like to express my pleasure to mark the publication of the volume at hand entitled "Islamic Astronomy and Copernicus." It brings together 15 articles penned by F. Jamil Ragep, which were earlier published in journals, encyclopaedias, or edited books. It is important for us to reprint these articles which have made, and will continue to make, substantial contributions to the literature on the Islamic influence on Copernican astronomy. Moreover, F. Jamil Ragep was the recipient of the TUBA International Academy Prize in the Social Sciences and Humanities in 2019 thanks to his studies in the field, especially those dealing with the Islamic background of the Copernican system. We are pleased to draw attention to F. Jamil Ragep's scholarship on this subject with this publication.

This foreword is too short to highlight properly the significance and context of the articles in this volume but let me stress one of F. Jamil Ragep's remarkable historiographic achievements. His scholarship leaves no doubt that in order to make sense of the ways in which Islamic astronomy had an influence on European astronomy in general and on Copernicus in particular, along with examining technical and astronomical contents of the key texts connecting Copernicus's scholarship to Islamic astronomical traditions, one should also deal with intellectual and philosophical discussions that later stimulated the astronomical and cosmological transformations in the medieval and early modern periods. This broader perspective adopted by F. Jamil Ragep paved the way for new evidence regarding the Islamic background to the scientific and intellectual environment in which Copernicus had flourished.

I hope that the publication of this volume will provide an insight for those interested in this important episode of the history of science. In line with TUBA's mission of promoting rigorous scientific research, we are committed to sharing this volume with a wider audience. In this respect, it will be available as an open-access publication on our website, and we will send its copies to many Turkish libraries, TUBA's counterpart science academies, and several umbrella organizations.

By way of conclusion, I would like to express my heartfelt gratitude to the dear author F. Jamil Ragep, and his former student Hasan Umut, who contributed to the preparation of the work for publication. My special thanks also go to the TUBA staff who put their efforts to make this publication possible.

Prof. Dr. Muzaffer SEKER
TÜBA President

About Author

Prof. Dr. F. Jamil Ragep / TÜBA Honorary Member / McGill University (Emeritus)

Born in West Virginia (USA), he attended the University of Michigan, where he received degrees in Anthropology and Near Eastern Studies, and later took a Ph.D. in the History of Science at Harvard University. He was Canada Research Chair in the History of Science in Islamic Societies at McGill University in Montreal, Canada from 2007 until 2020, at which time he retired as Professor Emeritus. He has written extensively on the history of science in Islam and has co-edited books on the transmission of science between cultures and on water resources in the Middle East. Thanks to major grants from the Canada Foundation for Innovation and the Quebec government, and in collaboration with the Max Planck Institute for the History of Science (Berlin), Ragep was able to initiate an ongoing international effort to catalogue all Islamic manuscripts in the exact sciences and provide a means to access information online on the intellectual, institutional, and scientific contexts of these texts (Islamic Scientific Manuscripts Initiative [ISMI]). Most recently, TÜBA International Academy Prizes laureate Prof. Dr. F. Jamil Ragep has published a number of articles and co-edited a volume of essays dealing with the Islamic background to the Copernican revolution.

INTRODUCTION

There has been considerable scholarly interest in the question of the Islamic background to early modern European astronomy and particularly to the astronomy of Nicholas Copernicus (1473-1543 CE). There is virtually no controversy, at least among reputable scholars, that Islamic astronomy influenced medieval and early modern astronomy through the Latin translations or reworkings of numerous Islamic astronomical works and through the translations of Arabic translations of Greek astronomical texts. To get a sense of the range and depth of that influence, examples are numerous: one need only cite works by al-Battānī (Albategnius [Albatenius], d. 317 H/929 CE), Thābit ibn Qurra (d. 288 H/901 CE), and al-Biṭrūjī (Alpetragius, fl. ca. 1200 CE), as well as the twelfth-century Latin translation of Ptolemy's (fl. 140 CE) *Almagest* that apparently used multiple Arabic versions. Much more debatable has been the claim that Copernicus's models were borrowed—wholesale—from Islamic sources. The articles herein collected are related to this question, and I am deeply indebted to the Turkish Academy of Sciences (TÜBA) for making them more widely available.

I was initially reluctant to enter into the Copernicus question. The wealth of unexamined Islamic scientific writings, I believed and still believe, make it imperative to contextualize those works, especially the many neglected works after the so-called "Golden Age," which has been erroneously claimed to have ended about 1200 CE. The question of influence on other cultural groups seemed to me at the time to be more properly within the purview of those expert in their traditions and languages. And the contributions of scholars such as Noel Swerdlow and Otto Neugebauer certainly supported that view. But it has become clear to me that the Islamic context, which is not readily accessible to Latinists, can assist Europeanists in understanding the sometimes arcane astronomical models and epistemic choices of their subjects. The Islamic context may also help Latinists and early modernists take into account the evolution of scientific ideas and avoid the temptation to assume the ideas, models, instruments, etc. they encounter were new and unprecedented. Of course, there is always the possibility of "parallelism"; since the Islamic and European scientific traditions had similar sources, it would not be surprising that new ideas in one culture were "rediscovered" independently in the another. But recent historical research has shown over and over again that the diffusion and exchange of science was a reality long before the modern age, considerably undercutting the parallelism argument.

The fifteen articles included in this volume are divided into four sections, each emphasizing a different aspect of the Islam-Copernicus connection. The first section includes three articles that are more general in nature. "Copernicus and His Islamic Predecessors" provides a historiographical overview of the discovery of the mathematical connections between Islamic astronomers and Copernicus. But I stress that focusing on the mathematical models is not sufficient for understanding the possible influence of Islamic thinkers on Copernicus; one must also take into account the rise of a kind of "mathematical humanism" within an Islamic context that made it possible to question the Aristotelian doctrine of a non-moving Earth at the center of the universe. This argument is developed in "Freeing Astronomy from Philosophy," which deals with the influence of Islamic doctrines on the development of science in Islam. In particular, I argue that the criticism by Islamic theologians of Aristotelian tenets, especially the claim that natural philosophy dictated a stationary Earth, made it possible to consider other alternatives. This was most forcefully articulated by the fifteenth-century theologian/scientist ʿAlī Qushjī (d. 879 H/1474 CE). The last article in this section, "Islamic Reactions to Ptolemy's Imprecisions," explores the rather dramatic increase in accuracy of observations during the Islamic period and proposes Islam's creationist perspective, which prioritized the phenomenal world of the senses over the Platonic world of "Ideas," as a possible explanation. Along with the development of trigonometry and other mathematical tools, this often underappreciated aspect of the Islamic contribution to science should be seen as a significant transformation that was an important component of the transition to modern science.

The next section includes five articles concerning the Ṭūsī-couple, which is a device invented by Naṣīr al-Dīn al-Ṭūsī (d. 672 H/1274 CE) that produces a straight-line motion from two circular motions. J. L. E. Dreyer in 1906 CE had already pointed out that Ṭūsī's device was used by Copernicus,[1] and many historians of science have since then emphasized the couple as significant evidence of transmission. In addition to the articles included here, I dealt with the couple in detail in my two-volume edition, translation, and study of Ṭūsī's *Memoir on Astronomy* (*al-Tadhkira fī ʿilm al-hayʾa*).[2] In "The Two Versions of the Ṭūsī Couple," I emphasized that Ṭūsī had actually developed

1 J. L. E. Dreyer, *History of the Planetary Systems from Thales to Kepler* (Cambridge: Cambridge University Press, 1906), 269.

2 F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy,* 2 vols. (New York: Springer, 1993), 2: 427-57.

two separate devices, one that produced rectilinear oscillation, while the intention of the other was to generate a curvilinear oscillation on the surface of a sphere. The rectilinear oscillation was mainly used for longitudinal motions, allowing Ṭūsī to treat distance independently from circular motion for his planetary models. The curvilinear version was used, among other things, for planetary latitudes. In all cases, the purpose was to produce motions that would avoid Ptolemy's violations of uniform, circular motions in the celestial region. "From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple" is my most recent summary of both the mathematical and historical aspects of the couple. Among the surprising things I discovered since my initial research was that Ṭūsī's original formulations of the couple and his planetary models, presented in his Persian *al-Risāla al-Muʿīniyya* and its Supplement,[3] was different from what was in the later *Tadhkira* and had at least one significant error. This allowed me to show that the Ṭūsī-couple and models contained in the work of the Byzantine scholar George Chioniades (d. ca. 1320 CE), entitled "The Schemata of the Stars," were in fact from *al-Risāla al-Muʿīniyya,* not the *Tadhkira*. The evidence is presented in "New Light on Shams" and in "From Tūn to Toruń." It is significant that Chioniades's "Schemata" was available in the Vatican Library at the time Copernicus was in Rome around 1500 CE. To contextualize Chioniades, I also include a short encyclopedia article summarizing his life and contributions. Finally, in this section, "The Origins of the Ṭūsī-Couple Revisited" provides some recently discovered evidence that allows us to give a chronology of Ṭūsī's discovery and evolving versions of his couples. This shows that Naṣīr al-Dīn first announced his new models in *al-Risāla al-Muʿīniyya,* but did not actually present them until almost ten years later in the Supplement to that work. Shortly after completing the Supplement, he would present an adaptation of his rectilinear version in the Recension (*Taḥrīr*) of Ptolemy's *Almagest*. But it was only in writing the *Tadhkira* that he provided a corrected version of the rectilinear device and his newly developed curvilinear version.

Though Ṭūsī's influence is an important part of the Islam-Copernicus connection, a far more important role belongs to ʿAlāʾ al-Dīn al-Awsī, better known as Ibn al-Shāṭir (d. 777 H/1375-6 CE), the focus of the third section. Ibn al-Shāṭir's work came to the attention of the scholarly world in the 1950s, with the publications by Victor Roberts, later in collaboration with his teacher E. S. Kennedy, that revealed a remarkable similarity of Ibn al-Shāṭir's planetary models with those of Copernicus.[4] Noel

[3] For the critical edition, see Naṣīr al-Dīn Muḥammad al-Ṭūsī, *Al-Risāla al-Muʿīniyya (al-Risāla al-Mughniya) and its Supplement,* vol. 1: edition by Sajjad Nikfahm-Khubravan and Fateme Savadi (Tehran: Miras-e Maktoob, 2020); vol. 2: English translation by F. Jamil Ragep, Fateme Savadi, and Sajjad Nikfahm-Khubravan, forthcoming. https://ismi.mpiwg-berlin.mpg.de/page/resources

[4] See especially Edward S. Kennedy and Victor Roberts, "The Planetary Theory of Ibn al-Shāṭir," *Isis* 50/3 (1959): 227-35.

Swerdlow and Otto Neugebauer continued and supplemented the research of Roberts and Kennedy, with Swerdlow concluding that "the relation between the models is so close that independent invention by Copernicus is all but impossible."[5] Swerdlow had mainly emphasized the connections between the "first anomaly," the part of the models dealing with the planets' motions through the zodiac. Here both Ibn al-Shāṭir and Copernicus had used a double-epicycle model to resolve the irregular motion brought about by Ptolemy's equant device. For the "second anomaly," the one having to do with motion on Ptolemy's epicycle that was connected with the planet's motion with respect to the Sun, Swerdlow proposed that Copernicus had used Regiomontanus's (d. 1476 CE) models that transformed Ptolemy's epicycles into eccentrics.[6] In "Ibn al-Shāṭir and Copernicus: The Uppsala Notes Revisited," I proposed a different interpretation, one in which Ibn al-Shāṭir's models were more holistically connected to both the first and second anomalies in the Copernican models. Basing myself upon an important insight of my then student and present-day colleague Sajjad Nikfahm-Khubravan, I argued that there was a "heliocentric bias" in Ibn al-Shāṭir's models that greatly facilitated the transition from a geocentric to a heliocentric cosmology. This was followed with a more technical article on the Mercury model ("Ibn al-Shāṭir and Copernicus on Mercury"), co-authored with Nikfahm-Khubravan. We maintained there that the mathematical equivalence of Copernicus's most complex model in *De revolutionibus* with that of Ibn al-Shāṭir's was decisive evidence for Copernicus's dependence on his predecessor. But beyond this obvious point, we claimed that the rather different model in Copernicus's earlier *Commentariolus*, though clearly still dependent on Ibn al-Shāṭir, indicated that Copernicus was striving for a different sort of cosmology at that earlier stage of his career. This was a kind of quasi-homocentrism, which allowed for epicycles but disallowed eccentrics. Unfortunately, this did not work very well, so eccentrics made a reappearance in *De revolutionibus.* Finally, this section includes a brief biography of Ibn al-Shāṭir (also written with Nikfahm-Khubravan) that contributes some additional information to what is known of this remarkable fourteenth-century Damascene.

In the final section are four articles that deal in varying degrees with other Islamic connections with Copernicus. The first, "ʿAlī Qushjī and Regiomontanus," underscores the remarkable similarity of Regiomontanus's transformations of epicyclic into eccentric models with a similar, earlier endeavor by ʿAlī Qushjī. This conversion

5 Noel Swerdlow, "Copernicus, Nicolaus (1473–1543)," in *Encyclopedia of the Scientific Revolution from Copernicus to Newton*, ed. W. Applebaum (New York and London, 2000), 165.

6 For Swerdlow's analysis, see his "The Derivation and First Draft of Copernicus's Planetary Theory: A Translation of the Commentariolus with Commentary," *Proceedings of the American Philosophical Society* 117 (1973): 423-512 and N. M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De revolutionibus,* 2 parts (New York/Berlin: Springer, 1984), esp. 1: 41-54.

has been held by Swerdlow and others to be crucial for the transformation from a geocentric to heliocentric cosmology, but I have come to believe that it is far less important than the central role played by Ibn al-Shāṭir's models for both the first and second anomalies. In any event, the nearly identical figures accompanying ʿAlī Qushjī's treatise and the printed version of Regiomontanus's *Epitome of the Almagest* provide yet more evidence of the interchange of ideas between Islam and Europe during the fifteenth century.

In "Ṭūsī and Copernicus: The Earth's Motion in Context," I discuss an interesting discourse in Islam, beginning with Ṭūsī, that dealt with the question of the Earth's possible rotation. Although Ṭūsī accepted that the Earth was at rest at the center of the Universe, he did not think that the empirical proofs put forward by Ptolemy and others were valid. Instead, he proposed that a rotating Earth would not be sensed by an observer if the air and what was in it were also rotating. His conclusion was that the only proof was a natural philosophical one, based on the fact that the element earth naturally moved rectilinearly toward the center and therefore could not rotate. This position drew considerable attention and was disputed by, among others, his onetime student Quṭb al-Dīn al-Shīrāzī (d. 710 H/1311 CE). That Qushjī rejected both the empirical and natural philosophical proofs for the Earth's stasis opened up the possibility for its motion. What connects this discourse to Copernicus is a passage in *De revolutionibus* that follows Ṭūsī's wording quite closely, in particular an appeal to the daily motion of comets as an analogue to the possible rotational motion of objects in the air.

The article "Ibn al-Haytham and Eudoxus" points to an interesting use of homocentric modeling by Ibn al-Haytham (d. ca. 432 H/1040-41 CE) to provide physical orbs to achieve part of Ptolemy's planetary motions in latitude. This is one of several instances in Islamic astronomy in which homocentric modeling, along the lines advocated by Eudoxus and Aristotle (both 4th c. BCE), gained some adherents among Islamic astronomers, the most well-known being al-Biṭrūjī. Needless to say, this is part of a complex story of homocentricity and quasi-homocentricity that should form part of the story of Copernican astronomy.

The final article in this section, "Al-Battānī, Cosmology, and the Early History of Trepidation in Islam," concerns the complex and intriguing history of trepidation, an alternative to the monotonic precession of the equinoxes. It was often connected with the apparent decrease in the obliquity of the ecliptic. Although variable precession was an incorrect theory from antiquity that was eventually abandoned by later Islamic astronomers, it gained a number of adherents in the early centuries of Islamic science. Regarding the connection to medieval and early modern European astronomers, it is noteworthy that several of them continued to believe in the theory despite long-term observations in the Islamic world that showed precession to be basically monotonic. Copernicus himself gave a model for trepidation in *De revolutionibus* (Bk. III, Chs. 3-5) that was meant to account for both variable precession and the change in obliquity; remarkably, it was essentially the same as that suggested by Ṭūsī in the *Tadhkira*. Ṭūsī, though, was skeptical of the theory and only presented it "if the fact of these two motions [variable precession and the obliquity] and their variability is ascertained."[7]

Again, let me thank the Turkish Academy of Sciences and in particular its President, Prof. Dr. Muzaffer Şeker, for their encouragement and support in republishing these articles. I would also like to thank my former student and current colleague, Dr. Hasan Umut, whose assistance in getting this book published has been invaluable.

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    b. Nikfahm-Khubravan, Sajjad, and F. Jamil Ragep. "Ibn al-Shāṭir." In *The Encyclopaedia of Islam*, Third Edition, Part 2019-2, 67–72. Leiden: E. J. Brill, 2019.
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7 Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy*, 1: 222-23.

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3) John Wiley & Sons, Inc.

a. Ragep, F. Jamil. "The Two Versions of the Ṭūsī Couple." In *From Deferent to Equant: Studies in Honor of E.S. Kennedy,* edited by David King and George Saliba, 329–356. Vol. 500 of *The Annals of the New York Academy of Sciences*, 1987.

4) McGill-Queen's University Press

a. Ragep, F. Jamil. "From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple." In *Before Copernicus: The Cultures and Contexts of Scientific Learning in the Fifteenth Century,* edited by Rivka Feldhay and F. Jamil Ragep, 161–197 + notes. Montreal: McGill-Queen's University Press, 2017.

5) Sage Publishing

a. Ragep, F. Jamil. "Copernicus and His Islamic Predecessors: Some Historical Remarks." *History of Science* 45 (2007): 65–81.

b. Ragep, F. Jamil. "Ibn al-Shāṭir and Copernicus: The Uppsala Notes Revisited." *Journal for the History of Astronomy* 47 (Nov. 2016): 395–415.

c. Ragep, F. Jamil. "ʿAlī Qushjī and Regiomontanus: Eccentric Transformations and Copernican Revolutions." *Journal for the History of Astronomy* 36/4 (2005): 359–371. (Translated into Turkish by Yavuz Unat as "Ali Kuşçu ve Regiomontanus: Dişmerkezli Dönüşümler ve Kopernik Devrimi," *Osmanlı Bilimi Araştırmaları* VIII/1 (2006): 81–96.)

6) Springer Nature

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7) University of Barcelona

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8) University of Chicago Press

   a. Ragep, F. Jamil. "Freeing Astronomy from Philosophy: An Aspect of Islamic Influence on Science." *Osiris* 16 (2001): 49–71.

9) Open Access

   a. Ragep, F. Jamil. "The Origins of the Ṭūsī-Couple Revisited." In *Instruments – Observations – Theories: Studies in the History of Astronomy in Honor of James Evans*, edited by Alexander Jones and Christián Carman, 229–237, 2020, DOI: 10.5281/zenodo.3928498. Open access distribution under a Creative Commons Attribution 4.0 International (CC-BY) license. https://archive.nyu.edu/bitstream/2451/61288/59/14.%20Ragep.pdf

   b. Ragep, F. Jamil. "Chioniades, Gregor [George]." [This is the original English version, for which I own the copyright, that has appeared in Persian translation as "Khiyūniyādis [Chioniades]," in *Dāʾirat al-Maʿārif-i Buzurg-i Islāmī* (Iran) [*Great Islamic Encyclopedia*], vol. 23 (Tehran: Markaz-i Dāʾirat al-Maʿārif-i Buzurg-i Islāmī, 1396 [2018]), 355–358.]

Section I

General Works

COPERNICUS AND HIS ISLAMIC PREDECESSORS: SOME HISTORICAL REMARKS

F. Jamil Ragep
McGill University

As a result of research over the past half century, there has been a growing recognition that a number of mathematical models used by Copernicus had originally been developed by Islamic astronomers. This has led to speculation about how Copernicus may have learned of these models and the role they played in the development of his revolutionary, heliocentric cosmology. Most discussion of this connection has thus far been confined to fairly technical issues related to these models; recently, however, it has been argued that the connections may go deeper, extending into the physics of a moving Earth and the way in which astronomy itself was conceived. The purpose of this article is to give an overview of these possible connections between Copernicus and his Islamic predecessors and to discuss some of their implications for Copernican studies.

THE MATHEMATICAL BACKGROUND

That Copernicus was acquainted with a number of his Islamic predecessors has been evident since 1543, when Copernicus in *De revolutionibus* explicitly cited five Islamic authors.[1] The latest of these authors, al-Biṭrūjī, flourished in Spain in the last part of the twelfth century, so Copernicus's references end around 1200, which is the approximate terminus date for Islamic authors who were translated into Latin. Until recently, most historiography related to Copernicus has assumed that this was the end of the story, at least as far as Islamic influence goes. But since the 1950s, a series of discoveries has shaken this neatly constricted picture and caused a major re-evaluation of the relation of Copernicus (as well as other Renaissance astronomers) to later Islamic astronomy.

The first modern acknowledgement of a connection between Copernicus and a later (i.e. post-1200) Islamic astronomer was made by J. L. E. Dreyer in 1906. In a footnote, Dreyer noted that the new device invented by Naṣīr al-Dīn al-Ṭūsī (d. 1274) was also used by Copernicus in Book III, chap. 4 of *De revolutionibus.*[2] Typical for the time, Dreyer offered no further explanation or speculation; nor did anyone else until the discovery in the 1950s of a connection between another Islamic astronomer and Copernicus. E. S. Kennedy, who was a professor of mathematics at the American University of Beirut, happened by chance to notice some unusual (i.e. non-Ptolemaic) astronomical models while browsing through the *Nihāyat al-sūl* of ᶜAlā' al-Dīn Ibn al-Shāṭir, a Damascene astronomer of the fourteenth century who had been the time-keeper of the Umayyad Mosque. Upon showing these to his friend and mentor, Otto Neugebauer of Brown University, Kennedy was amazed to learn that these models were ones that had been thought to have first appeared in the works of Nicholas Copernicus. This led to a series of articles by Kennedy and his students that discussed various aspects of these models by Ibn al-Shāṭir as well as by other late Islamic astronomers.[3]

The picture that emerged can be summarized as follows. Islamic authors from an early period were critical of Ptolemy's methods, observations, and models.[4] One particular irritant was the use of devices by Ptolemy that violated the accepted physical principles that had been adopted by most astronomers in the ancient and medieval periods. Later Islamic astronomers came to list sixteen of these violations: six having to do with having the reference point for uniform motion of an orb being different from the actual centre of the orb (often referred to as the "equant" problem); nine having to do with a variety of Ptolemaic devices meant to bring about latitudinal variation in the planets' motions (i.e. deviation north or south of the ecliptic); and, finally, an irregular oscillation of the lunar epicycle due to the reference diameter being directed to a "prosneusis" point rather than the deferent centre of the epicycle.[5] The earliest systematic attempt in Islam to criticize Ptolemy's methods and devices occurred in *al-Shukūk ᶜalā Baṭlamyūs* (*Doubts against Ptolemy*) by Ibn al-Haytham (d. *c.* 1040), who was better known in Europe for his great work on optics. In addition to his blistering critique of Ptolemy, Ibn al-Haytham also wrote a treatise in which he attempted to deal with some of the problems of Ptolemy's planetary latitude models.[6] A contemporary of Ibn al-Haytham, Abū ᶜUbayd al-Jūzjānī, who was an associate of Abū ᶜAlī Ibn Sīnā (= Avicenna, d. 1037), also dealt with these issues and proposed a model to deal with the equant problem.[7]

These early attempts notwithstanding, the major thrust to provide alternative models occurred in the twelfth century and continued for several centuries thereafter. In Islamic Spain, there were a number of criticisms that questioned the very basis of Ptolemaic astronomy, in particular its use of eccentrics and epicycles, which culminated in an alternative cosmological system by al-Biṭrūjī that used only orbs that were homocentric with the Earth.[8] But though Biṭrūjī's work had important influences in Europe — indeed Copernicus mentions his view that Venus is above the Sun[9] — the Spanish "revolt" against Ptolemy should be seen as episodic rather than marking the beginning of a long-lived tradition of Islamic homocentric astronomy.

In the Islamic East the situation was otherwise. Beginning in the first half of the thirteenth century, a number of works appeared that proposed alternatives to Ptolemy's planetary models. This was the start of an extremely fruitful period in the history of science in Islam in which a series of creative mathematical models were produced that dealt with the problems of Ptolemaic astronomy. Among the most important of these new models were those of Naṣīr al-Dīn al-Ṭūsī (1201–74), Mu'ayyad al-Dīn al-ᶜUrḍī (d. *c.* 1266), Quṭb al-Dīn al-Shīrāzī (1236–1311), ᶜAlā' al-Dīn Ibn al-Shāṭir (d. *c.* 1375), and Shams al-Dīn al-Khafrī (*fl.* 1525).[10] In essence, these astronomers developed mathematical tools (such as the "Ṭūsī couple" and the "ᶜUrḍī lemma") that allowed connected circular motions to reproduce approximately the effects brought about by devices such as Ptolemy's equant.[11] In the case of the rectilinear Ṭūsī couple, two spheres, one half the size and internally tangent to the other, rotate in opposite directions with the smaller twice as fast as the larger. The result of these motions is that a given point on a diameter of the larger sphere will oscillate rectilinearly. (There is an analogous curvilinear Ṭūsī couple in which the oscillation is meant to occur on a great circle arc on the surface of a sphere.) What this allowed Ṭūsī and his successors to do was to isolate the aspect of Ptolemy's equant model that brought about a variation in distance between the epicycle centre and the Earth's centre from the aspect that resulted in a variation in speed of the epicycle centre about the Earth. Such mathematical dexterity allowed these astronomers to present models that to a great extent restored uniform circular motion to the heavens while at the same time producing motions of the planets that were almost equivalent to those of Ptolemy.[12]

THE CONNECTION TO COPERNICUS

Noel Swerdlow and Otto Neugebauer, in discussing this Islamic tradition, famously asked: "What does all this have to do with Copernicus?" Their answer was: "Rather a lot."[13] In his commentary on Copernicus's *Commentariolus*, Swerdlow made the case for this connection through a remarkable reconstruction of how Copernicus had arrived at the heliocentric system. According to Swerdlow, Copernicus, somehow aware of this Islamic tradition of non-Ptolemaic astronomy, began his work to reform astronomy under its influence. In particular Copernicus objected explicitly to Ptolemy's use of the equant, an objection that had been a staple of Islamic astronomy for some five centuries at that point (but which seems not to have been made by earlier European astronomers).[14] Swerdlow then proposed that although Copernicus was able to use some of these models, in particular those of Ibn al-Shāṭir, to deal with the irregular motion brought about by the first anomaly (the motion of the epicycle centre on the deferent), it was the second anomaly (related to the motion of the planet on the epicycle) that remained problematic. For the outer planets this motion corresponds to the motion of the Earth around the Sun, so a transformation of this motion from an epicyclic to an eccentric would lead to a quasi-heliocentric system, whereby the planet goes around the Sun. Of course the Earth could still remain at rest while the Sun, with the planets going around it, could then go around the Earth. In other words, Copernicus's transformations could have led to a Tychonic system. Swerdlow argued that this was not an option for Copernicus, since it led to the notorious intersection of the spheres of the Sun and Mars, which simply was not possible in the solid-sphere astronomy to which Copernicus was committed. Thus Copernicus was compelled to opt for a heliocentric system with the Earth, as a planet, in motion around the Sun.[15]

In his reconstruction, Swerdlow assumed that Copernicus *must* have had access to the models of his Islamic predecessors. Because of the scarcity of concrete evidence for this assertion (i.e. translated texts in Latin, earlier European references to these models, or the like), Swerdlow was clearly swayed by the similarity of complex geometrical models; independent discovery was simply not an option. As he stated with Neugebauer in 1984:

> The planetary models for longitude in the *Commentariolus* are all based upon the models of Ibn ash-Shāṭir — although the arrangement for the inferior planets is incorrect — while those for the superior planets in *De revolutionibus* use the same arrangement as ᶜUrdī's (*sic*) and Shīrāzī's model, and for the inferior planets the smaller epicycle is converted into an equivalent rotating eccentricity that constitutes a correct adaptation of Ibn ash-Shāṭir's model. In both the *Commentariolus* and *De revolutionibus* the lunar model is identical to Ibn ash-Shāṭir's and finally in both works Copernicus makes it clear that he was addressing the same physical problems of Ptolemy's models as his predecessors. It is obvious that with regard to these problems, his solutions were the same.
>
> The question therefore is not whether, but when, where, and in what form he learned of Marāgha theory.[16]

This has recently been reinforced by Swerdlow:

> How Copernicus learned of the models of his [Arabic] predecessors is not known — a transmission through Italy is the most likely path — but the relation between the models is so close that independent invention by Copernicus is all but impossible.[17]

Neugebauer and Swerdlow did have one bit of evidence that seemed to show a likely means of transmission between the Islamic world and Italy. This was a text contained in MS Vat. Gr. 211, in which one finds the Ṭūsī couple (rectilinear version) and Ṭūsī's lunar model. Apparently dating from about 1300, it is either a Greek translation or reworking of an Arabic treatise, made perhaps by the Byzantine scholar Gregory Chioniades.[18] The fact that this manuscript found its way to the Vatican, perhaps in the fifteenth century, provides a possible means for the transmission of knowledge of Ṭūsī's models. It is also noteworthy that Ṭūsī's models seem to have been widely known by contemporaries of Copernicus; examples include Giovanni Battista Amico and Girolamo Fracastoro.[19]

The historian of astronomy Willy Hartner also pointed to evidence for transmission from Islamic astronomers to Copernicus. Though he states that independent discovery of these models and devices by Copernicus was "possible", "it seems more probable that the news of his Islamic predecessor's model reached him in some way or other". Here Hartner was speaking of the model of Ibn al-Shāṭir; he was more certain that another example "proves clearly" the borrowing by Copernicus of the Ṭūsī couple inasmuch as the lettering in Copernicus's diagram in *De revolutionibus* follows the standard Arabic lettering rather than what one might expect in Latin.[20]

HISTORIOGRAPHICAL REACTIONS

One would have expected that these historical discoveries, some of which are now a half-century old, would have caused a substantial reevaluation of the origins of the "scientific revolution" or at the least an attempt to deal with the role of Islamic science in that revolution. The fact that this has not yet occurred to any significant degree may be traced to several factors. First, recent trends in the historiography of science have resulted in critiques of the very notion of a "scientific revolution", which have tended to downplay the traditional preeminence of the Copernicus-Galileo-Newton narrative.[21] But even those who still hold to some notion of a scientific revolution have tended to focus their attention on local contexts (usually European) for explanations and to look at the consequences rather than the origins of Copernicanism.[22] Second, the increasing realization that Copernicus was rather conservative in his scientific outlook, holding on, for example, to the traditional orbs and their uniform, circular motions, has called his revolutionary status into question. So there seems to be an underlying assumption that the enormous complexity in *De revolutionibus* is more or less irrelevant for the truly important innovation, heliocentricism, which, according to this view, is all that really mattered for Kepler, Galileo, *et al.* [23] Thus the convoluted story of "Copernicus and the Arabs", which is mostly about the complicated but supposedly irrelevant models, becomes more trouble than it is worth.[24] Third, despite, but in part due to, the trend towards "political correctness", there has been a tendency to essentialize different scientific traditions, sometimes because of a benign cultural relativism, sometimes for more invidious reasons. Thus the "essential" part of the scientific revolution, of which the de-centring of the Earth is fundamental, is seen as European.[25] Finally, the simple fact of academic boundaries has played a role. Because historians of science specializing in Islamic civilization have tended to be marginalized, in part for disciplinary reasons, in part because of the arcane nature of many of their publications, it has been surprisingly difficult to initiate an on-going dialogue between medieval Latinists, Islamists, and early modernists.[26]

Although the larger history of science community seems so far to have resisted dealing with the implications of the Islamic connection to Copernicus, some historians of astronomy who do not specialize in Islamic science have been influenced by the discoveries of Kennedy and his colleagues. We have already discussed Neugebauer and Swerdlow. Jerzy Dobrzycki and Richard L. Kremer also explored possible connections between Islamic astronomy and early modern European astronomy in their incisive article "Peurbach and Marāgha astronomy"; they raised the distinct possibility that Peurbach may well have developed non-Ptolemaic models based upon Islamic sources that were similar (if not the same) as ones that would be used in the next generation by Copernicus. Given this earlier possibility of transmission, they came to an interesting conclusion: "We may be looking for a means of transmission both more fragmentary and widespread than a single treatise, and at least one of the Marāgha sources must have been available to the Latin West before 1461, the year of Peurbach's death."[27] But not all historians of early modern astronomy have been so willing to accept a connection, even in the face of numerous coincidences. I. N. Veselovsky claimed that it is more likely that Copernicus got the Ṭūsī couple from a mathematically-related theorem in Proclus's *Commentary on the First Book of Euclid's Elements*.[28] More recently, Mario di Bono has maintained that independent rediscovery of the Islamic astronomical models by Copernicus and his contemporaries is at least as plausible as intercultural transmission. Somewhat surprisingly, he uses the number of similarities between Islamic and Copernican astronomy as evidence *against* transmission: "[If] derivation of Copernicus's models from Arab sources … is the case, it becomes very difficult to explain how such a quantity of models and information, which Copernicus would derive from Arab sources, has left no trace — apart from Ṭūsī's device — in the works of the other Western astronomers of the time."[29]

THE CONCEPTUAL BACKGROUND TO THE COPERNICAN REVOLUTION

Di Bono's article serves to highlight what has been missing in the analysis of the connection between Islamic astronomy and Copernicus. The emphasis on the models alone obscures several crucial historiographical, conceptual, and physical issues that need to be considered when dealing with the Copernican transformations. Let us first look briefly at some of these historiographical issues. What seems to be overlooked by those who advocate a reinvention by Copernicus and/or his contemporaries of the mathematical models previously used by Islamic astronomers is the lack of an historical context for those models within European astronomy. At the least, one would expect to find some tradition of criticism of Ptolemy in Europe in which those models would make sense. But in fact this is not the case. Copernicus's statement of his dissatisfaction with Ptolemaic astronomy, which is the ostensible reason he gives for his drastic cosmological change, had no precedent in Europe but did have a continuous five-hundred-year precedent in the Islamic world. Here is what he says in the introduction to the *Commentariolus*:

> … these theories [put forth by Ptolemy and most others] were inadequate unless they also envisioned certain *equant* circles, on account of which it appeared that the planet never moves with uniform velocity either in its *deferent* sphere or with respect to its proper centre. Therefore a theory of this kind seemed neither perfect enough nor sufficiently in accordance with reason.
>
> Therefore, when I noticed these [difficulties], I often pondered whether perhaps a more reasonable model composed of circles could be found from which every apparent irregularity would follow while everything in itself moved uniformly, just as the principle of perfect motion requires.[30]

Since the *Commentariolus* is the initial work in which Copernicus presents his new cosmology, one would assume that it would be here, and not in the much later *De revolutionibus*, in which we should search for his original motivations.[31] What do we learn from this passage? Copernicus puts himself squarely within the tradition of Islamic criticisms of Ptolemy's violations of uniform, circular motions in the heavens. It is important to keep in mind that this tradition began in the Islamic world as early as the eleventh century and led to the series of alternative models outlined above. Furthermore this tradition lasted for some six centuries in which there was a very vigorous discourse that led to various proposals, criticisms, and counter-proposals by an active group of astronomers from many regions of the Islamic world. Those who advocate parallel development would thus seem to be claiming that a centuries-long tradition with no analogue whatsoever in Europe was recapitulated, somehow, in the life of one individual who not only paralleled the criticisms but also the same models and revised models in the course of some thirty years. Needless to say, such an approach is ahistorical in the extreme.

Another point needs to be made here. Di Bono and others have pointed to the Paduan astronomers as a possible source for Copernicus's inspiration. But an important distinction needs to be made. The "return" to homocentric astronomy that was evidently advocated by the Paduans has its parallel and inspiration in the "Andalusian revolt" against Ptolemy in twelfth-century Spain. But this revolt, fomented by such figures as Ibn Bājja, Ibn Ṭufayl, Ibn Rushd (Averroes), and most importantly by al-Biṭrūjī, who advanced an alternative astronomical/cosmological system, needs to be clearly differentiated from the type of Islamic astronomy that most closely resembles that of Copernicus, i.e. the Eastern *hay'a* tradition of Ibn al-Haytham, Ṭūsī, ʿUrḍī, Shīrāzī, Ibn al-Shāṭir and others.[32] What we know from the Andalusian revolt is that its extreme position against Ptolemy's epicycles and eccentrics led to a failed project that had virtually no impact on the Eastern *hay'a* tradition. It would seem odd indeed that this Andalusian tradition, in the guise of Paduan astronomy, would have been a source for Copernicus's alternative models in which epicycles and eccentrics play such a prominent role. It is also important to note that neither among the Paduans nor among European astronomers and natural philosophers before Copernicus is there a criticism of the equant or other Ptolemaic devices that lead to a violation of uniform, circular motion.[33] One must be careful to distinguish a general criticism of Ptolemy's eccentrics and epicycles (and an advocacy of homocentric astronomy) from the tradition of criticism of Ptolemy's irregular motions that was initiated by Ibn al-Haytham, a tradition that clearly includes Copernicus.

Let us now turn to the conceptual issues involved with the Copernican revolution. In the traditional Aristotelian hierarchy of the sciences, the mathematical sciences (including astronomy) were dependent (or subalternate) to physics/natural philosophy, which itself was subordinate to metaphysics. Obviously in order to overturn the Aristotelian doctrine of a stationary Earth, a doctrine for Aristotelians firmly based upon both natural philosophical and metaphysical principles, Copernicus would have had to conceive of a different type of physics. This physics would need to be, somehow, formulated within the discipline of astronomy itself and somehow independent of Aristotelian natural philosophy. Luckily, he had a number of important precedents for this position.

The most authoritative of these precedents was Ptolemy himself. In the introduction to the *Almagest,* Ptolemy reverses the order of the sciences and places mathematics above natural philosophy and metaphysics (or "theology"), both of which, he claims, "should rather be called guesswork than knowledge". He goes on to say "that only mathematics can provide sure and unshakeable knowledge to its devotees, provided one approaches it rigorously".[34] Though his position had the *potential* to free the astronomer from the natural philosopher, in actuality a kind of compromise emerged in which the astronomer and the natural philosopher were said to differ not on the actual set of doctrines but rather on the way to prove them. This is clearly laid out in a passage from Geminus preserved in Simplicius's commentary on Aristotle's physics:

> Now in many cases the astronomer and the physicist will propose to prove the same point, e.g., that the Sun is of great size or that the Earth is spherical, but they will not proceed by the same road. The physicist will prove each fact by considerations of essence or substance, of force, of its being better that things should be as they are, or of coming into being and change; the astronomer will prove them by the properties of figures or magnitudes, or by the amount of movement and the time that is appropriate to it.[35]

Most Islamic astronomers followed this formulation, elaborating and clarifying it using the fact/reasoned fact (*quia/propter quid*) distinction of Aristotle's *Posterior analytics*. Thus the astronomers were seen as giving the facts of various cosmological issues (e.g. that the Earth was spherical) using observational and mathematical tools as is done in Ptolemy's *Almagest*, whereas the proof of the natural philosopher, such as in Aristotle's *De caelo*, provided the reason or the "why" behind these facts.[36]

This relatively benign view of the relationship between the astronomer and the physicist came, over time, to be modified in significant ways. Most likely under the influence of Islamic theologians, who were fundamentally opposed to Aristotelian notions of natural cause, we can see subtle shifts in how physical principles were presented in the introductory parts of astronomical texts.[37] Naṣīr al-Dīn al-Ṭūsī, for example, presented the critical principle of the uniformity of celestial motion in such a way that it did not depend upon the ultimate cause. Thus the monoformity of falling bodies, and the uniformity of celestial motions, both of which moved "in a single way", was what was important. It became irrelevant that the former was brought about by a "nature" while the latter was brought about by a "soul".[38]

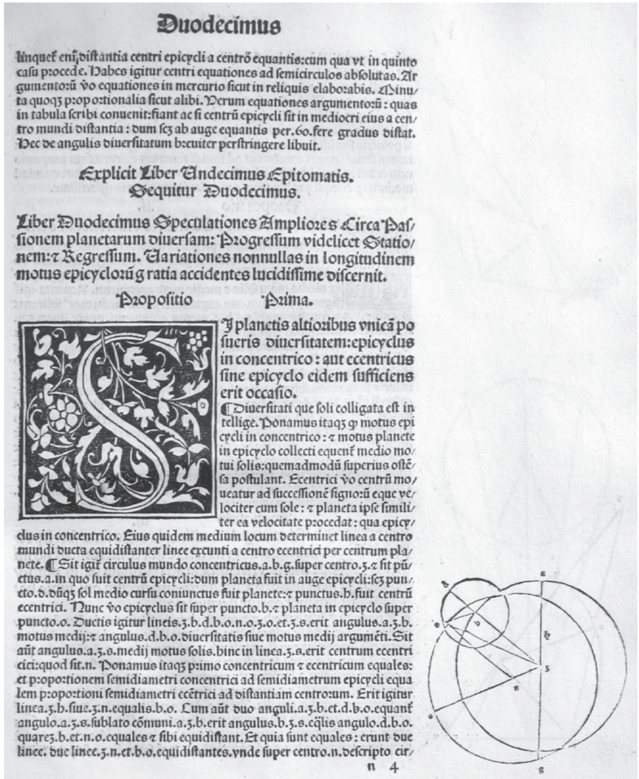
Slowly, then, we see an attempt in Islamic astronomy to provide a self-contained mathematical methodology that ran parallel to the methods of the natural philosophers. But Ṭūsī for one did not believe that this meant that the astronomer could be completely independent of the natural philosophers and metaphysicians, since there were certain principles that only the natural philosophers could provide the astronomer. In fact this was generally the position of Islamic astronomers with the notable exception of ᶜAlī Qūshjī in the fifteenth century.

Qūshjī was the son of the falconer of Ulugh Beg (1394–1449), the Timurid prince who was a generous patron of the sciences and arts. Ulugh Beg was an active supporter and participant in the magnificent Samarqand observatory, which was one of the greatest scientific institutions that had been established up to that time. As a boy, Qūshjī became his protégé and student and eventually occupied an important position at the observatory. After the assassination of Ulugh Beg, Qūshjī was attached to various courts in Iran but would end his career in Constantinople under the patronage of Mehmet II, who had conquered the city for the Ottomans.

Qūshjī held that the astronomer had no need for Aristotelian physics and in fact should establish his own physical principles independently of the natural philosophers.[39] This position had profound implications for one principle in particular, namely that the element earth had a principle of rectilinear inclination that precluded it from moving naturally with a circular motion.[40] Ṭūsī had maintained that there was no way for the astronomer, using mathematics and observation, to arrive at the "proof of the fact" that the Earth was either moving or at rest. This was contrary to Ptolemy's position in the *Almagest* (I.7), namely that one could establish a static Earth through observation. After Ṭūsī, we can trace a three-century discussion in which various authors argued whether he or Ptolemy was correct regarding the possibility of an observational proof of the Earth's state of rest. Qūshjī, though, took a somewhat different approach. Starting with his view that the astronomer should not depend on the natural philosopher, but also rejecting Ptolemy's view that an observational test was possible, Qūshjī made the remarkable claim that nothing false follows from the assumption of a rotating Earth.[41]

The connection with Copernicus, though, might seem tenuous at best. What makes this an arguable possibility is the remarkable coincidence between a passage in *De revolutionibus* (I.8) and one in Ṭūsī's *Tadhkira* (II.1[6]) in which Copernicus follows Ṭūsī's objection to Ptolemy's "proofs" of the Earth's immobility.[42] This passage, which is quoted by numerous Islamic scholars after Ṭūsī, including Qūshjī, formed the starting point for their discussion of the Earth's possible motion. The closeness of the passage in Copernicus is one more bit of evidence that he seems to have been influenced not only by Islamic astronomical models but also by a conceptual revolution that was going on in Islamic astronomy. This conceptual revolution was opening up the possibility for an alternative "astronomical" physics that was independent of Aristotelian physics.

It is this point that has been missed up to now in seeking to understand the Islamic background to Copernicus. Clearly there is more to the Copernican revolution than some clever astronomical models that arose in the context of a criticism of Ptolemy. There also needed to be a new conceptualization of astronomy that could allow for an astronomically-based physics. But there is hardly anything like this in the European tradition before Copernicus.[43] The fact that we can find a long, vigorous discussion in Islam of this issue intricately-tied to the question of the Earth's movement should indicate that such a conceptual foundation was there for the borrowing. It will be argued, of course, that the mechanism for such borrowing has yet to be found. But again, in my opinion it is more important at this point in our knowledge to focus on the *products* rather than the mechanism of transmission. By doing so, we can get a clearer idea not only of the possible Islamic connection to Copernicus but also of the Copernican revolution itself.



Duodecimus

linquẽt enĩ, diſtãtia centri epicycli a centrõ equantis: cum qua vt in quinto caſu procede. Habes igitur centri equationes ad ſemicirculos abſolutas. Argumentorũ vo equationes in mercurio ſicut in reliquis elaborabis. Minuta quoq; proportionalia ſicut alibi. Verum equationes argumentorũ: quas in tabula ſcribi conuenit: fiant ac ſi centrũ epicycli ſit in mediocri eius a centro mundi diſtantia: dum ſcz ab auge equantis per. 60. fere gradus diſtat. Hec de angulis diuerſitatum breuiter perſtringere libuit.

Explicit Liber Undecimus Epitomatis.
Sequitur Duodecimus.

Liber Duodecimus Speculationes Ampliores Circa Paſſionem planetarum diuerſam: Progreſſum videlicet Stationem: & Regreſſum. Uariationes nonnullas in longitudinem motus epicyclorũ g ratia accidentes lucidiſſime diſcernit.

Propoſitio Prima.

Si planetis altioribus vnicã poſueris diuerſitatem: epicyclus in concentrico: aut eccentricus ſine epicyclo eidem ſufficiens erit occaſio.

Diuerſitati que ſoli colligata eſt intellige. Ponamus itaq; ꝙ motus epicycli in concentrico: & motus planete in epicyclo collecti equenẽ medio motui ſolis: quemadmodũ ſuperius oſtẽſa poſtulant. Eccentrici vo centrũ moueatur ad ſucceſſionẽ ſignorũ eque velociter cum ſole: & planeta ipſe ſimiliter ea velocitate procedat: qua epicyclus in concentrico. Eius quidem medium locum determinet linea a centro mundi ducta equidiſtanter linee exeunti a centro eccentrici per centrum planete. Sit igit̃ circulus mundo concentricus. a. b. g. ſuper centro. z. & ſit pũctus. a. in quo fuit centrũ epicycli: dum planeta fuit in auge epicycli: ſcz puncto. d. dũq; ſol medio curſu coniunctus fuit planete: & punctus. b. fuit centrũ eccentrici. Nunc vo epicyclus ſit ſuper puncto. b. & planeta in epicyclo ſuper puncto. o. Ductis igitur lineis. z. b. d. b. o. n. o. z. o. et. z. s. erit angulus. a. z. b. motus medij: & angulus. d. b. o. diuerſitatis ſiue motus medij argumẽti. Sit aũt angulus. a. z. s. medij motus ſolis. hinc in linea. z. s. erit centrum eccentrici: quod ſit. n. Ponamus itaq; primo concentricum & eccentricum equales: et proportionem ſemidiametri concentrici ad ſemidiametrum epicycli equalem proportioni ſemidiametri eccẽtrici ad diſtantiam centrorum. Erit igitur linea. z. b. ſiue. z. n. equalis. b. o. Cum aũt duo anguli. a. z. b. et. d. b. o. equant angulo. a. z. s. ſublato cõmuni. a. z. b. erit angulus. b. z. s. eq̃lis angulo. d. b. o. quare. z. b. et. n. o. equales & ſibi equidiſtant. Et quia ſunt equales: erunt due linee. due linee. z. n. et. b. o. equidiſtantes. vnde ſuper centro. n. deſcripto cir

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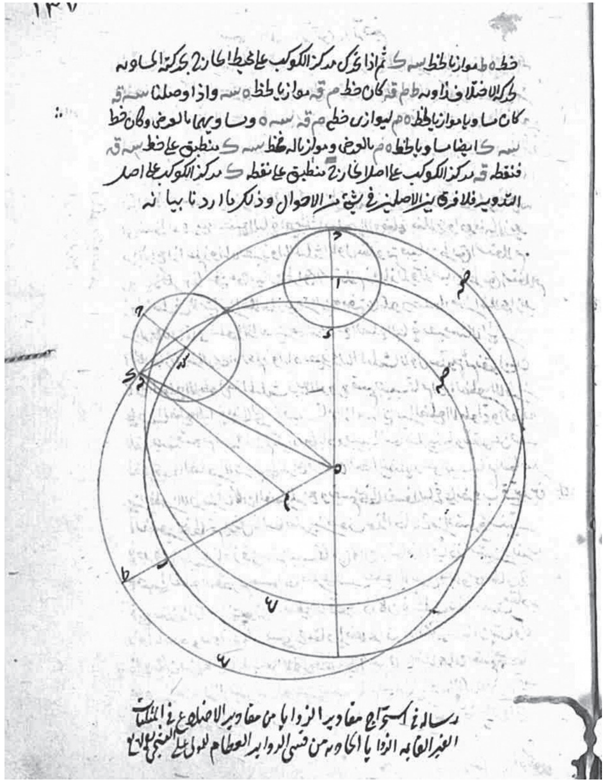

Fig. 1. Comparison of diagrams of Regiomontanus and Qūshjī. (*Left*) J. Regiomontanus and G. Peurbach, *Epytoma Joannis de monte regio In almagestum ptolemaei* (Venice, 1496), n4r, and (*right*) ʿAlī Qūshjī, *Fī anna aṣl al-khārij*…, Carullah MS 2060, f. 137a. Reproductions courtesy of the History of Science Collections, University of Oklahoma Libraries, and of the Süleymaniye Library, Istanbul, respectively.

FURTHER THOUGHTS

In the two years since I first developed the views expressed above, I have published a small treatise by ᶜAlī Qūshjī (d. 1474) that presents and proves a proposition that appears in Book XII of Regiomontanus's *Epitome of the Almagest*, which was completed in 1463.[44] The importance of this proposition can scarcely be overstated, since it allows one to transform all of Ptolemy's planetary epicyclic models into eccentric models, which is generally accepted as crucial for the transformation from a geocentric to a heliocentric cosmology (see above). In that article, I argue that the possibility of a connection to Regiomontanus was strengthened by the lack of a context or justification in which Regiomontanus presented the proposition, which stands in stark contrast to the expansive manner in which Qūshjī discusses his own discovery (as a result of dealing with the Mercury model) and his attempt to explain why Ptolemy disallowed such a transformation for the lower planets (Mercury and Venus). The striking similarity of Qūshjī's figure that accompanies his text and that of Regiomontanus (Figure 1) adds to the possibility that this is a matter of transmission.

Given that Qūshjī was also willing to allow for the possibility of the Earth's rotation, the connections to Copernicus seem irresistible. Here I should emphasize the point that I made at the end of the original article above, namely that it is important to keep in mind that more is involved than a simple transmission of propositions or mathematical models. The sudden appearance in Europe at the end of the fifteenth century of what can be called "mathematical humanism" is what really demands an explanation. Obviously the interest in reforming and/or transforming the Ptolemaic system along the lines that had developed over many centuries in the *hay'a* tradition of eastern Islamic astronomy is one aspect of this. But clearly there is much more in Regiomontanus's mathematical *Programme* than Ptolemaic astronomy (although it plays a major role in his thinking).[45] It is here that I think more work needs to be done.

James Stephen Byrne has recently argued that "Regiomontanus's vision of mathematics is that of a mathematician, rather than that of a historian, an educator, or a philosopher". Rather than viewing Regiomontanus simply through a humanist lens, Byrne contends that one should see his "mathematical humanism" as "deeply rooted in the traditional university curriculum ... [but] [a]bove all, it is rooted in mathematical texts, both curricular and extra-curricular".[46] But as Michael Shank has pointed out: "With respect to the university, it is important to note first that from almost every point of view except intrinsic interest and later historiographical significance, the mathematical sciences at Vienna were on the margins. Institutionally, they had a place, but it was a minor one. They appear in the curriculum, but do not form its core. Statistically, they are distinctly in the minority; they are taught, read, and practiced by a minority."[47] But Shank goes on to argue that this does not make them any less important or significant. And clearly there must have been some pre-existing interest in the mathematical sciences in order for Cardinal Bessarion, the Greek prelate who "desperately wanted to preserve and breathe new life into the intellectual heritage of classical Greece", to have inspired Peurbach and Regiomontanus to undertake what amounted to a resuscitation of the Ptolemaic astronomical tradition in Europe.[48]

If we accept Shank's position, and I believe we should, then we have moved at least part of the problem back to accounting for Bessarion's "mathematical humanism". This is a vexed question and raises the issue of the revival of interest in science during the Palaeologan period (1259–1453). It seems clear that Byzantine scholars were in contact with and were influenced by Islamic scientific developments.[49] But how far did this influence extend? Since we have late Islamic models in Byzantine texts, and since we have other examples of Islamic texts in Byzantine form (the "Persian Tables", for example[50]), the transmission of scientific objects is obvious. But what of the less tangible, more conceptual aspects I have spoken of above? Is it possible to transmit ideas, in particular ideas about how to do science? I have argued elsewhere that this is indeed possible.[51] Following on A. I. Sabra's notion of the "appropriation" of Greek science in Islam, I believe we can also speak of the transmission of a "moral economy" of science. (Here I borrow the terminology of L. J. Daston.) In this case, that transmission would have consisted of the notion that astronomy could, indeed should, be based upon a new set of physical principles that would be mathematically and empirically based, rather than upon Aristotelian natural philosophy. This, I contend, was also contained in the suitcase that Bessarion took with him to Vienna along with books and other *objets de science*.

Why do I not think this was not the result of the "predilections" of Peurbach and the young Regiomontanus, who somehow transmitted this to Copernicus in the next generation? For the same reason that I reject the parallelism argument. History takes time. In the Islamic world, the revolutionary rejection of Aristotelian physics in astronomy was something that took hundreds of years, dozens of scholars, and thousands of pages before it bore fruit in the person of ᶜAlī Qūshjī in Samarqand. The role of the physics of the Islamic theologians (*mutakallim*s), the attack from various quarters on the Aristotelian claim of epistemic knowledge, the development of rhetorical tools to use in scientific argumentation, and the use of science to glorify God were all things that had counterparts in medieval Europe. What did not have a counterpart until the late fifteenth century was their interaction with the advanced astronomical tradition that had developed over many centuries within the Islamic world. In short, Regiomontanus, and his successors, reflect the mathematical humanism that had a brilliant but short life in Central Asia.[52]

In his stunning, but under-appreciated work on the origins of humanism in Islam, George Makdisi asks why we should bother about influence. His answer is that "by understanding where we came from in our intellectual culture we are apt to gain a better understanding of the civilization of the Christian West, not only that of classical Islam". And he concludes with poignancy and prescience: "What is certain is that the Western Christian and Classical Islamic civilizations have strongly interacted in the Middle Ages and in Modern Times, and will continue to interact far into the future."[53]

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REFERENCES

1. These are: al-Battānī, al-Biṭrūjī, al-Zarqāllu, Ibn Rushd, and Thābit ibn Qurra. Copernicus also refers to al-Battānī in his *Commentariolus*, which remained unpublished during his lifetime. "Islamic" here refers to the civilization of Islam, not the religion, since a number of "Islamic" astronomers, such as Thābit, were not Muslims.
2. J. L. E. Dreyer, *History of the planetary systems from Thales to Kepler* (Cambridge, 1906), 269. Dreyer knew of Ṭūsī's work from the translation by Carra de Vaux of a chapter of his *al-Tadhkira fi ᶜilm al-hay'a* ("Les sphères célestes selon Nasīr Eddin-Attusī" in Paul Tannery, *Recherches sur l'histoire de l'astronomie ancienne* (Paris, 1893), Appendix VI, pp. 337–61).
3. These have been conveniently collected in E. S. Kennedy and Imad Ghanem (eds), *The life & work of Ibn al-Shāṭir: An Arab astronomer of the fourteenth century* (Aleppo, 1976), and in E. S. Kennedy *et al.*, *Studies in the Islamic exact sciences* (Beirut, 1983), 50–107. The most important of these is E. S. Kennedy, "Late medieval planetary theory", *Isis*, lvii (1966), 84–97.
4. See, for example, the very critical remarks, most likely by the Banū Mūsā (ninth century), in Régis Morelon, *Thābit ibn Qurra: Œuvres d'astronomie* (Paris, 1987), 61.
5. F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's memoir on astronomy* (2 vols, New York, 1993), i, 48–51.
6. F. Jamil Ragep, "Ibn al-Haytham and Eudoxus: The revival of homocentric modeling in Islam", in *Studies in the history of the exact sciences in honour of David Pingree*, ed. by C. Burnett *et al.* (Leiden, 2004), 786–809.
7. George Saliba, "Ibn Sīnā and Abū ᶜUbayd al-Jūzjānī: The problem of the Ptolemaic equant", *Journal for the history of Arabic science*, iv (1980), 376–403; reprinted in *idem*, *A history of Arabic astronomy: Planetary theories during the golden age of Islam* (New York, 1994), 85–112.
8. See Nūr al-Dīn abū Isḥāq al-Biṭrūjī, *On the principles of astronomy*, ed. and transl. by B. Goldstein (2 vols, New Haven, 1971). *Cf.* A. I. Sabra, "The Andalusian revolt against Ptolemaic astronomy: Averroes and al-Biṭrūjī", in *Transformation and tradition in the sciences*, ed. by E. Mendelsohn (Cambridge, 1984), 133–53.
9. Nicholas Copernicus, *De revolutionibus orbium coelestium*, transl. by E. Rosen as *On the revolutions* (Baltimore, 1978), 18.
10. Good overviews can be found in George Saliba, "The astronomical tradition of Maragha: A historical survey and prospects for future research", *Arabic science and philosophy,* i (1991), 67–99 (reprinted in *idem*, *History* (ref. 7), 258–90), and George Saliba, "Arabic planetary theories after the eleventh century AD", in *Encyclopedia of the history of Arabic science*, ed. by R. Rashed (3 vols, London, 1996), i, 58–127.
11. Today we would say that these mathematical tools were equivalent to linkages of constant-length vectors rotating at constant angular velocities; but it is important to remember that Islamic astronomers conceived of their devices as physical and not simply mathematical. *Cf.* Ragep, *op. cit.* (ref. 5), ii, 433–7.
12. F. Jamil Ragep, "The two versions of the Ṭūsī couple", in *From deferent to equant: Studies in honor*

of E. S. Kennedy, ed. by D. King and G. Saliba (*The annals of the New York Academy of Sciences*, d (1987)), 329–56.

13. N. M. Swerdlow and O. Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus* (2 parts, New York and Berlin, 1984), i, 47.
14. N. M. Swerdlow, "The derivation and first draft of Copernicus's planetary theory: A translation of the Commentariolus with commentary", *Proceedings of the American Philosophical Society*, cxvii (1973), 423–512, p. 434.
15. Admittedly, this is a grossly simplified version of a fuller and much more careful exposition that one may find in Swerdlow and Neugebauer, *op. cit.* (ref. 13), i, 41–64. A good summary is also provided by Michael H. Shank, "Regiomontanus on Ptolemy, physical orbs, and astronomical fictionalism: Goldsteinian themes in the 'Defense of Theon against George of Trebizond'", *Perspectives on science: Historical, philosophical, social,* x (2002), 179–207.
16. Swerdlow and Neugebauer, *op. cit.* (ref. 13), i, 47. *Cf.* Neugebauer's earlier remark that "The mathematical logic of these methods is such that the purely historical problem of contact or transmission, as opposed to independent discovery, becomes a rather minor one" (O. Neugebauer, "On the planetary theory of Copernicus", in *Vistas in astronomy*, x (1968), 89–103, p. 90; reprinted in *idem*, *Astronomy and history* (New York, 1983), 491–505, p. 492).
17. N. Swerdlow, "Copernicus, Nicolaus (1473–1543)", in *Encyclopedia of the scientific revolution from Copernicus to Newton*, ed. by W. Applebaum (New York and London, 2000), 165.
18. Swerdlow and Neugebauer claim that it is a translation. The recent editors and translators of the text argue that it is an original Byzantine work that is simply influenced to some degree by Arabic or Persian sources (E. A. Paschos and P. Sotiroudis, *The schemata of the stars* (Singapore, 1998), 11–18). The closeness to Islamic sources, however, and the use of the standard Arabic corruption *Kakkaous* rather than the Greek *Cepheus* argue for a greater dependence than the authors wish to admit. Clearly more research on this question is needed.
19. See N. Swerdlow, "Aristotelian planetary theory in the Renaissance: Giovanni Battista Amico's homocentric spheres", *Journal for the history of astronomy,* iii (1972), 36–48, and Mario Di Bono, "Copernicus, Amico, Fracastoro and Ṭūsī's device: Observations on the use and transmission of a model", *Journal for the history of astronomy,* xxvi (1995), 133–54. In a passage in III.4 of *De revolutionibus* that was deleted prior to publication, Copernicus himself speaks of others who had used the Ṭūsī device; see Ragep, *op. cit.* (ref. 5), ii, 431.
20. Willy Hartner, "Copernicus, the man, the work, and its history", *Proceedings of the American Philosophical Society*, cxvii (1973), 413–22, p. 421.
21. A session at a recent American History of Science Society annual meeting was entitled: "The late, great scientific revolution". *Cf.* Margaret J. Osler (ed.), *Rethinking the scientific revolution* (Cambridge, 2000).
22. Two recent examples are Peter Dear's *Revolutionizing the sciences: European knowledge and its ambitions, 1500–1700* (Princeton, 2001), and Steven Shapin's *The scientific revolution* (Chicago, 1996). Both ignore Islamic science entirely and scarcely discuss medieval European contributions to the scientific revolution.
23. Copernicus's conservatism was emphasized in 1959 both in a scholarly and in a popular context. As for the former, Derek de Solla Price's "Contra-Copernicus" provided a technical account that showed that Copernicus was really still part of ancient and medieval astronomy. As Price concludes: "... Copernicus made a fortunate philosophical guess without any observation to prove or disprove his ideas, and ... his work as a mathematical astronomer was uninspired. From this point of view his book is conservative and a mere reshuffled version of the *Almagest*" (Derek J. de S. Price, "Contra-Copernicus: A critical re-estimation of the mathematical planetary theory of Ptolemy, Copernicus, and Kepler", in *Critical problems in the history of science*, ed. by M. Clagett (Madison, 1969), 197–218, p. 216). The popular presentation of this viewpoint was made

by Arthur Koestler in his *The sleep walkers: A history of man's changing vision of the universe* (London, 1959), where Copernicus is referred to as the "timid canon". How Copernicus can be "saved" despite this conservatism and/or his connection to Islamic astronomy is well-illustrated by Erna Hilfstein's remarks regarding the significance of Copernicus's achievement: "Copernicus may have used the geometrical devices of his Greek or Arab predecessors (for example, from the 'Maragha school'), yet his system, and the perception of the cosmos it established, was entirely novel" ("Introduction to the softcover edition" of *Nicholas Copernicus, On the revolutions*, transl. and comm. by E. Rosen (Baltimore, 1992), p. xiii.

24. We should note, though, that recently Michał Kokowski, in defending Copernicus's originality, has also conceded that the Islamic models are also somehow revolutionary according to his "correspondence principle" inasmuch as they supersede those of Ptolemy by overcoming the problematic equant (*Copernicus's originality: Towards integration of contemporary Copernican studies* (Warsaw, 2004), 75–77). Here he follows George Saliba, who maintained that the Marāgha school astronomers were revolutionary because of their "realization that astronomy ought to describe the behaviour of physical bodies in mathematical language, and should not remain a mathematical hypothesis, which would only save the phenomena" ("The rôle of Maragha in the development of Islamic astronomy: A scientific revolution before the Renaissance", *Revue de synthèse*, cviii (1987), 361–73, p. 372; reprinted in *idem*, *History* (ref. 7), 245–57, p. 256). It is not clear how Saliba reconciles this position with his later claim that the sixteenth-century astronomer al-Khafrī had reached "unparalleled heights" in this tradition by "realiz[ing] that all mathematical modeling had no physical truth by itself and was simply another language with which one could describe the physical observed reality" (George Saliba, "Arabic versus Greek astronomy: A debate over the foundations of science", *Perspectives on science,* viii (2000), 328–41, p. 339). For a contrary view, see A. I. Sabra, who has argued that this Islamic scientific tradition was not revolutionary but should be regarded as "normal science" in the Kuhnian sense ("Configuring the universe: Aporetic, problem solving, and kinematic modeling as themes of Arabic astronomy", *Perspectives on science,* vi (1998), 288–330, pp. 292, 321–3). As should be clear in what follows, I believe that the emphasis on the mathematical models — whether revolutionary or not — has distracted us from what is the most significant and innovative part of Islamic theoretical astronomy.

25. Recent books by two mediaevalists, A. C. Crombie and Edward Grant, advocate the European nature of modern science, thus reverting to the more traditional viewpoint. See Crombie's *Styles of scientific thinking in the European tradition: The history of argument and explanation especially in the mathematical and biomedical sciences and arts* (London, 1994), and Grant's *The foundations of modern science in the Middle Ages: Their religious, institutional, and intellectual contexts* (Cambridge, 1996) and *idem*, *God and reason in the Middle Ages* (Cambridge, 2001). This view is held by both Western and Islamic scholars so cannot be simply ascribed to some biased antagonism towards Islamic civilization. For example, the Iranian expatriate S. H. Nasr has stated that although "all that is astronomically new in Copernicus can be found essentially in the school of al-Ṭūsī", Islamic astronomers were prescient enough not to break with the traditional Ptolemaic cosmology "because that would have meant not only a revolution in astronomy, but also an upheaval in the religious, philosophical and social domains" (S. H. Nasr, *Science and civilization in Islam*, 2nd edn (Cambridge, 1987), 174).

26. *Cf.* Sonja Brentjes, "Between doubts and certainties: On the place of history of science in Islamic societies within the field of history of science", *N.T.M.,* xi (2003), 65–79.

27. J. Dobrzycki and R. L. Kremer, "Peurbach and Marāgha astronomy? The ephemerides of Johannes Angelus and their implications", *Journal for the history of astronomy,* xxvii (1996), 187–237, p. 211.

28. I. N. Veselovsky, "Copernicus and Naṣīr al-Dīn al-Ṭūsī", *Journal for the history of astronomy,* iv (1973), 128–30. This turns out to be implausible since Copernicus probably did not know of the

Proclus theorem (actually the converse of the Ṭūsī couple) until many years after he used the device; see Ragep, *op. cit.* (ref. 5), ii, 430–1.

29. Di Bono, *op. cit.* (ref. 19), 153–4.
30. Swerdlow, *op. cit.* (ref. 14), 434–5.
31. Recently B. R. Goldstein ("Copernicus and the origin of his heliocentric system", *Journal for the history of astronomy,* xxxiii (2002), 219–35) has sought to undermine Swerdlow's reconstruction of the origins of Copernicus's heliocentric system by emphasizing a passage in *De revolutionibus* (I.10). In it Copernicus points to the distance–period relationship of the planets to justify his system, which Goldstein takes to be the initial motivation. But again, it is odd that this is hardly mentioned in the *Commentariolus*.
32. For this Spanish episode in Islamic astronomy, see Sabra, *op. cit.* (ref. 8), 133–53.
33. It is difficult, if not impossible, to prove a negative, but it is highly suggestive that one does not find the word "equant" in Edward Grant's monumental (816-page) *Planets, stars, and orbs: The medieval cosmos, 1200–1687* (Cambridge, 1994). Even in the generation immediately before Copernicus, there seems to have been no precedent for what was a commonplace in Islamic astronomy. As stated in Dobrzycki and Kremer, *op. cit.* (ref. 27), 211: "We know of no extant text by Peurbach or Regiomontanus in which the Ptolemaic models are criticized explicitly on the grounds that they violate uniform, circular motion."
34. G. J. Toomer, *Ptolemy's Almagest* (London, 1984), 36.
35. The translation is due to T. L. Heath in his *Aristarchus of Samos* (Oxford, 1913), 276; reprinted in Morris R. Cohen and I. E. Drabkin, *A source book in Greek science* (Cambridge MA, 1948), 90–91. *Cf.* G. E. R. Lloyd, "Saving the appearances", *Classical quarterly,* n.s., xxviii (1978), 202–22, pp. 212–14 (reprinted with new introduction in *idem, Methods and problems in Greek science* (Cambridge, 1991), 248–77).
36. Ragep, *op. cit.* (ref. 5), i, 38–41, 106–7; ii, 386–8.
37. Much of what follows is elaborated in F. Jamil Ragep, "Freeing astronomy from philosophy: An aspect of Islamic influence on science", *Osiris,* xvi (2001), 49–71.
38. Ragep, *op. cit.* (ref. 5), i, 44–46, 98–101; ii, 380–1.
39. Ragep, *op. cit.* (ref. 37), 61–63.
40. A discussion of this Islamic discourse on the Earth's possible rotation is in F. Jamil Ragep, "Ṭūsī and Copernicus: The Earth's motion in context", *Science in context,* xiv (2001), 145–63.
41. *Ibid.*, 157.
42. *Ibid.,* 145–8.
43. In the fourteenth century, one finds Nicole Oresme and Jean Buridan discussing the Earth's rotation. The former, in particular, presents quite cogent reasons why one might believe in this motion. But in the end he rejects them for theological reasons. In both cases, it is clear that they have no interest in a reconceptualization of astronomy along the lines that occurred in Islamic astronomy (*ibid.,* 158–60). The possibility that such a discussion might have taken place in the fifteenth century in the circle of Peurbach and Regiomontanus is being investigated by M. Shank; *cf. op. cit.* (ref. 15).
44. F. Jamil Ragep, "ᶜAli Qūshjī and Regiomontanus: Eccentric transformations and Copernican revolutions", *Journal for the history of astronomy*, xxxvi (2005), 359–71.
45. See Menso Folkerts, "Regiomontanus' role in the transmission and transformation of Greek mathematics", in *Tradition, transmission, transformation: Proceedings of two conferences on premodern science held at the University of Oklahoma*, ed. by F. J. Ragep and S. P. Ragep (Leiden, 1996), 89–113.
46. James Steven Byrne, "A humanist history of mathematics? Regiomontanus's Padua Oration in context", *Journal of the history of ideas*, lxvii (2006), 41–61, p. 61.

47. Michael H. Shank, “The classical scientific tradition in fifteenth-century Vienna”, in F. J. Ragep and S. P. Ragep (eds), *Tradition, transmission, transformation* (ref. 45), 115–36, p. 131.
48. *Ibid*., 126.
49. See above, ref. 18 (on the transmission of Islamic astronomy to Byzantium). And as Otto Neugebauer has remarked: “There is no reason to assume that there is any period in which Islamic astronomy was not known in Constantinople” (*A history of ancient mathematical astronomy* (3 parts, New York, 1975), i, 11). *Cf.* Maria Mavroudi, *A Byzantine book on dream interpretation: The Oneirocriticon of Achmet and its Arabic sources* (Leiden, 2002); Alain Touwaide, “Arabic urology in Byzantium”, *Journal of nephrology,* xvii (2004), 583–9; and Alain Touwaide, “Arabic medicine in Greek translation: A preliminary report”, *Journal of the International Society for the History of Islamic Medicine,* i (2002), 45–53.
50. Anne Tihon, “Les tables astronomiques persane à Constantinople dans la première moitié du XIVe siècle”, *Byzantion,* lvii (1987), 471–87. Reprinted in Tihon, *Études d’astronomie Byzantine* (Aldershot, 1994).
51. F. J. Ragep and S. P. Ragep (eds), *Tradition, transmission, transformation* (ref. 45), pp. xv–xviii.
52. A very important article that takes up the mathematical humanism of Samarqand is İhsan Fazlıoğlu’s “Osmanlı felsefe-biliminin arkaplanı: Semerkand matematik-astronomi okulu”, *Dîvân ilmî araştırmalar,* xiv/1 (2003), 1–66. A revised version in English will appear in a forthcoming issue of the *Journal for the history of Arabic science*.
53. George Makdisi, *The rise of humanism in classical Islam and the Christian West* (Edinburgh, 1990), 349–50, p. 354.

Freeing Astronomy from Philosophy
An Aspect of Islamic Influence on Science

F. Jamil Ragep[*]

I. INTRODUCTION

IF ONE IS ALLOWED to speak of progress in historical research, one may note with satisfaction the growing sophistication with which the relationship between science and religion has been examined in recent years. The "warfare" model, the "separation" paradigm, and the "partnership" ideal have been subjected to critical scrutiny and the glaring light of historical evidence. As John Hedley Brooke has so astutely noted, "Serious scholarship in the history of science has revealed so extraordinarily rich and complex a relationship between science and religion in the past that general theses are difficult to sustain."[1] Unfortunately, this more nuanced approach has not been as evident in studies of Islam and science. Though there has been some serious scholarship on the relation between science and religion in Islam,[2] such work has made barely a dent in either the general accounts or the general perceptions of that relationship. These latter continue to be characterized by reductionism, essentialism, apologetics, and barely masked agendas.[3]

* Department of the History of Science, University of Oklahoma, 601 Elm St., Room 622, Norman OK 73019

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[1] John Hedley Brooke, *Science and Religion: Some Historical Perspectives* (Cambridge: Cambridge Univ. Press, 1991), p. 5.

[2] Two works that deserve especial mention are A. I. Sabra, "The Appropriation and Subsequent Naturalization of Greek Science in Medieval Islam: A Preliminary Statement," *Hist. Sci.* 25 (1987):223–43 (reprinted in idem, *Optics, Astronomy and Logic: Studies in Arabic Science and Philosophy* [Aldershot, U.K.: Variorum, 1994], no. 1, and in *Tradition, Transmission, Transformation,* ed. F. Jamil Ragep and Sally P. Ragep [Leiden: Brill, 1996], pp. 3–27); and A. I. Sabra, "Science and Philosophy in Medieval Islamic Theology," *Zeitschrift für Geschichte der Arabisch-Islamischen Wissenschaften* 9 (1994):1–42. David King and George Saliba have also made valuable contributions (in works cited later in the notes).

[3] Three fairly recent books illustrate the point nicely. Although they represent vastly different viewpoints, Pervez Hoodbhoy (*Islam and Science* [London: Zed, 1991]), Toby Huff (*The Rise of Early Modern Science* [Cambridge: Cambridge Univ. Press, 1993]), and S. H. Nasr (*Science and Civilization in Islam,* 2nd ed. [Cambridge: Islamic Texts Society, 1987]) blithely move from century to century and from region to region, applying their own particular vision to whatever historical event or personage comes their way. Hoodbhoy, a contemporary physicist who is confronting religious fanaticism in Pakistan, finds religious fanaticism to be the dominant aspect of science and religion in Islam. Huff, a sociologist intent on demonstrating that science could have arisen only in the West,

But even a cursory examination of sources, many of which unfortunately remain in manuscript, reveals a remarkable diversity of opinion in Islam regarding various aspects of the relationship between science and religion, which makes attempts to generalize an "Islamic" attitude toward science especially foolhardy. And the influence of the religion of Islam upon science, and vice versa, took a surprising number of forms, sometimes unexpectedly "progressive" from a modern viewpoint.[4]

When Hellenistic astronomy found a home in Islam in the eighth and ninth centuries A.D., it was adapted in numerous ways to fit into this new domicile. There are many reasons for this transformation, but here I concentrate on how Islam—understood as both doctrine and ritual—affected and influenced the course of astronomy. I first give an overview of these influences and then examine a specific case in which one can see how a religious discourse on the compatibility of the Aristotelian natural world and God's omnipotence made itself felt within theoretical astronomy, pushing it in various degrees toward independence from natural philosophy and metaphysics. I suggest that there was no single "Islamic" viewpoint, but rather divergent views arising from a variety of historical, intellectual, and individual factors. Though it is not the focus of the essay, I occasionally point to similarities between views of Islamic scholars and their European peers, similarities that may not be completely coincidental.

II. OVERVIEW OF THE RELATION BETWEEN HELLENISTIC ASTRONOMY AND ISLAM

Broadly speaking, one can identify two distinct ways in which religious influence manifested itself in medieval Islamic astronomy. First, there was the attempt to give religious value to astronomy, what David King has called "astronomy in the service of Islam." (One might also call this, to appropriate another context, the "handmaiden rationale.") The second general way in which religious influence shows up is in the attempt to make astronomy as metaphysically neutral as possible, in order to ensure that it did not directly challenge Islamic doctrine. As we shall see, some took this to mean that Hellenistic astronomy had not only to be reconceived but also stripped of its philosophical baggage.

Let us begin by looking briefly at the first type of influence, "astronomy in the service of Islam." Astronomy could and did provide the faithful (at least those who were interested) with extensive tables and techniques for determining prayer times,

attempts unconvincingly to show that "there was an absence [in Islamic civilization] of the rationalist view of man and nature" that effectively prevented the breakthroughs that occurred in early modern Europe (p. 88). Nasr, who wishes to point the way to a new "Islamic science" that would avoid the dehumanizing and despiritualizing mistakes of Western science, finds wherever he looks in the past an Islamic science that was spiritual and antisecular, so much so that even though "all that is astronomically new in Copernicus can be found essentially in the school of al-Ṭūsī," Islamic astronomers were prescient enough not to break with the traditional Ptolemaic cosmology, "because that would have meant not only a revolution in astronomy, but also an upheaval in the religious, philosophical and social domains" (p. 174). Essentialism, endemic in Islamic studies whether produced in the East or West, is pervasive throughout these works. Huff, for whom historical context seems an especially alien concept, does not hesitate to move from Ayatollah Khomeini to medieval jurists and back again (p. 203), akin to using Jerry Falwell to analyze Thomas Aquinas.

[4] An example is provided by B. F. Musallam in his *Sex and Society in Islam* (Cambridge: Cambridge Univ. Press, 1983), where he documents the use of ancient sources by numerous Islamic jurists of various stripes to bolster their sanction of contraception and abortion; see especially pp. 39–59.

for finding the sacred direction of Mecca, for calculating the beginning of Ramadan (the month of fasting), and so on. Since Muslim ritual could have survived perfectly well without the astronomers (does God really demand that one pray to within a minute or less of arc?), it does not take too great a leap of imagination to realize that this "service to religion" was really religion's service to the astronomers, both Muslim and non-Muslim,[5] providing on the one hand a degree of social legitimation and on the other a set of interesting mathematical problems to solve.[6]

One may also find instances of a different type of "service" that astronomy could provide, namely to reveal the glory of God's creation, a point made by no less a personage than Ibn al-Shāṭir, the fourteenth-century timekeeper of the Umayyad Mosque in Damascus.[7] This type of service was not new with Islam, of course; Ptolemy, Plato, and Aristotle, among others, saw astronomy as a way toward the divine (though in practice, admittedly, this meant something different for each of them).[8] But if I were to hazard here a particular "Islamic" influence and difference, I would say that it is in the emphasis on "God's creation" rather than on some Platonic, otherworldly reality. Islamic astronomers were thus less disposed toward the two-tiered reality that one sees in Neoplatonists such as Proclus (d. A.D. 485) or even in Ptolemy himself.[9] If I am right about this difference, it would go a long way toward explaining the surprising ambiguity one finds in Ptolemy about the reality of his planetary models and the much more realist approach taken generally by Islamic

[5] An example of a non-Muslim, indeed pagan, astronomer who worked "in the service of Islam" is Thābit ibn Qurra (d. A.D. 901), who wrote at least two treatises on crescent visibility; see Régis Morelon, *Thābit ibn Qurra: Œuvres d'astronomie* (Paris: Belles Lettres, 1987), pp. XCIII–XCVI.

[6] David King has been in the forefront of research dealing with both aspects. For social legitimation, see his essay "On the Role of the Muezzin and the *Muwaqqit* in Medieval Islamic Society," in Ragep and Ragep, *Tradition, Transmission, Transformation* (cit. n. 2), pp. 285–346, where King discusses the history of timekeeping and the role of the Mosque timekeeper *(muwaqqit)* both in Islamic civilization and in the history of astronomy. For more detailed, technical studies, see his *Astronomy in the Service of Islam* (Aldershot, U.K.: Variorum, 1993).

[7] Ibn al-Shāṭir is today best remembered for his treatise on theoretical astronomy in which he presented astronomical models that are virtually identical to ones used by Copernicus. The passage referred to, though, occurs in the introduction to his *al-Zīj al-jadīd,* a book on practical astronomy; see Sabra, "Science and Philosophy" (cit. n. 2), pp. 39–40. In addition to the scientific contexts where such praise for astronomy occurs, there is a religious cosmological literature dedicated to the glorification of God's creation; see Anton M. Heinen, *Islamic Cosmology: A Study of As-Suyūṭī's al-Hay'a as-sanīya fī l-hay'a as-sunnīya* (Beirut: Steiner, 1982), especially pp. 37–52.

[8] Plato discusses the importance of astronomy for finding true Reality in *Republic* 528E–530C, especially 530A, and for understanding the Divine in *Laws* 820E–822C; Ptolemy extols the study of astronomy for making "its followers lovers of this divine beauty, accustoming them and reforming their natures, as it were, to a similar spiritual state" (*Ptolemy's Almagest,* trans. and annot. G. J. Toomer [New York: Springer, 1984], I.1, p. 37). Though Aristotle is a bit more mundane, he is not averse to associating his study of the celestial aether with the divine (*De Caelo,* I.3, especially 270b6–12) nor to recommending the use of astronomers' results for ascertaining the number of divine beings (*Metaphysics,* XII.8, 1073b1–17).

[9] This manifests itself with Proclus in his contrast between human beings, who can only approximate the truth, and the gods, who alone can know it, and in his ambivalence regarding the reality of astronomical models such as eccentrics and epicycles. This position was called "instrumentalist" by Pierre Duhem in his influential but deeply flawed *Saving the Phenomena* ("ΣΟΖΕΙΝ ΤΑ ΦΑΙΝΟΜΕΝΑ: Essai sur la notion de théorie physique de Platon à Galilée," *Ann. Philo. Chrétienne,* 4th ser., 6 (1908):113–39, 277–302, 352–77, 482–514, 561–92; issued in book form [Paris: Hermann, 1908; reprinted Paris: Vrin, 1982]; Englished as *To Save the Phenomena: An Essay on the Idea of Physical Theory from Plato to Galileo,* trans. Edmund Doland and Chaninah Maschler [Chicago: Univ. of Chicago Press, 1969]). Duhem's views have been carefully analyzed by G. E. R. Lloyd in "Saving the Appearances," *Cl. Quart.,* n. s., 28 (1978):202–22, especially pp. 204–11 (reprinted with new introduction in idem, *Methods and Problems in Greek Science* [Cambridge: Cambridge Univ.

astronomers—an approach, I should add, that led a large number of them to attempt to reform Ptolemy by proposing more physically acceptable models.[10]

So much for astronomy in the service of Islam. Let us now move on to those religious influences that led to a more "metaphysically neutral" astronomy. The first example need not detain us. Clearly the most religiously objectionable part of Hellenistic astral science was astrology, which seemed to give powers to the stars that should be reserved for God. Attacks on astrology in Islam are not difficult to find, and they came, predictably, from religious quarters but also, more surprisingly, from some Hellenized philosophers such as Ibn Sīnā (= Avicenna [d. A.D. 1037]). It is instructive that Avicenna, not noted for conventional religious piety, did not hesitate to use Qur'ānic verses and a tradition from the Prophet to bolster his case against astrology; this tends to strengthen the argument that even those scientists committed to a Hellenistic outlook were sensitive to religious objections and willing to forgo parts of their Greek heritage.[11] A more subtle influence can be detected in the separation of astrology from astronomy. In early Islamic astronomical texts and in works categorizing the sciences, astronomy and astrology, following standard Hellenistic practice, were usually listed together under a rubric such as "science of the stars" (*ʿilm al-nujūm*) or even *astronomia* (i.e., the transliterated Greek term). Starting with Avicenna, however, astrology came to be categorized as a part of natural philosophy (or physics), whereas astronomy (which became known as *ʿilm al-hayʾa*) was categorized as a strictly mathematical discipline.[12] As we shall see, this was just one of several moves whose purpose seems to have been to free a reconstituted *mathematical* astronomy, which, it was claimed, was objectively true, from the religiously objectionable parts of Greek physics and metaphysics.

In addition to these predictable objections to astrology, there were religious objec-

Press, 1991], pp. 248–77). Lloyd provides a useful corrective to Duhem and argues that Proclus, somewhat surprisingly for a Platonist, had realist attitudes regarding phenomenal astronomy even while claiming that the "true philosopher" should "say goodbye to the senses" (p. 207; reprint, p. 259). Although, unlike Proclus, Ptolemy was a working astronomer and certainly not a Platonist (at least not in any simple sense), he does warn that "it is not appropriate to compare human [constructions] with divine" and, with faint echoes of Plato's insistence in the *Timaeus* that any account of the phenomenal world is only a "likely story," admits that "one should try, as far as possible, to fit the simpler hypotheses to the heavenly motions, but if this does not succeed, [one should apply hypotheses] which do fit" (*Almagest* [cit. n. 8], XIII.2, p. 600). But these seemingly instrumentalist remarks should be balanced against his bold confidence, in the introduction to the *Almagest,* "that only mathematics [including astronomy] can provide sure and unshakeable knowledge to its devotees" and that "this is the best science to help theology along its way" (p. 36), as well as against his later attempt to provide a cosmology in his *Planetary Hypotheses.* Clearly this aspect of Greek astronomy and cosmology deserves a much more elaborate and serious study than is possible here.

[10] To connect certain aspects of Islamic religious doctrine with the Islamic tradition of reforming Ptolemaic astronomy, itself part of a seemingly more substantial interest exhibited by Islamic astronomers (compared with their Greek predecessors) in discovering a true phenomenal cosmology, would require a significant historical study that is at best in its preliminary stages. My remarks here are meant simply as a working hypothesis.

[11] For a competent discussion of the objections to astrology by both religious and philosophical writers, see George Saliba, *A History of Arabic Astronomy: Planetary Theories during the Golden Age of Islam* (New York: New York Univ. Press, 1994), pp. 53–61, 66–72. Cf. Ignaz Goldziher, "The Attitude of Orthodox Islam toward the 'Ancient Sciences,'" in *Studies on Islam,* ed. and trans. Merlin L. Swartz (New York: Oxford Univ. Press, 1981), pp. 185–215, especially pp. 195–6 (German original: "Stellung der alten islamischen Orthodoxie zu den antiken Wissenschaften," *Abhandlungen der Königlich Preussischen Akademie der Wissenschaften* 8 (Berlin, 1916).

[12] For a further elaboration of this point, see F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy,* 2 vols. (New York: Springer, 1993), vol. 1, pp. 34–5.

tions to Hellenistic astronomy as a whole. It is to these and their effects upon Islamic astronomy that we now turn.

III. ON SAVING ASTRONOMY FROM THE TAINT OF PHILOSOPHY

Because it was one of the "ancient sciences" (i.e., pre-Islamic), astronomy was sometimes tarred with the same brush that besmirched any knowledge that fell outside the domain of the religious sciences. This taint took several forms. There were certainly those who condemned all the "ancient" or "foreign" sciences.[13] On the one hand, some singled out astronomy because of its presumably close association with astrology and even magic.[14] Others saw it as advancing strange and dangerous ideas, such as the notion of regions with a midnight sun, which was a consequence of the astronomers' circular motions and spherical bodies. If true, this would make it virtually impossible under some circumstances for Muslims in extreme northern climes to maintain the daylight fast during Ramadan.[15] Al-Ghazālī (d. A.D. 1111), certainly a more subtle and profound thinker, accepts that there are parts of astronomy (for example, the theory of solar and lunar eclipses) that are based on apodeictic demonstration and are thus "impossible to deny"; such things are, in and of themselves, unconnected with religious matters. However, these "neutral" and true aspects of mathematics may seduce the unwary student into believing that certainty also exists in the physical and metaphysical theories of the philosophers, some of which stand in contradiction to Islamic religious dogma. Thus the study of these sciences must be limited and constrained, for "few there are who devote themselves to this study without being stripped of religion and having the bridle of godly fear removed from their heads."[16]

But besides these more general warnings against astronomy as a representative of the "ancient sciences," there was another, more specific objection. Ghazālī tells us that

> [t]he basis of all these objections [to natural philosophy] is the recognition that nature is in subjection to God most high, not acting of itself but serving as an instrument in the hands of its Creator. Sun and moon, stars and elements, are in subjection to His command. There is none of them whose activity is produced by or proceeds from its own essence.[17]

This is part of Ghazālī's criticism of what we might term Aristotelian natural causation.

[13] Goldziher, "The Attitude of Orthodox Islam" (cit. n. 11), provides several examples.

[14] This is the insinuation made by Qāḍī (Judge) Tāj al-Dīn al-Subkī (14th c.); see David King, "On the Role of the Muezzin" (cit. n. 6), pp. 306–7 (p. 329 for the Arabic text). For Subkī's hostile attitude toward all of philosophy (with the exception of logic), which could well be the underlying reason for his disdain of astronomy, see Goldziher, "The Attitude of Orthodox Islam" (cit. n. 11), p. 207.

[15] Cf. Goldziher, "The Attitude of Orthodox Islam" (cit. n. 11), p. 197.

[16] Abū Ḥāmid al-Ghazālī, *al-Munqidh min al-ḍalāl,* ed. ʿAbd al-Karīm al-Marrāq (Tunis: al-Dār al-Tūnisiyya li-'l-Nashr, 1984), pp. 49–52. The translation used here is from W. Montgomery Watt, *The Faith and Practice of al-Ghazālī* (London: George Allen & Unwin, 1953), pp. 33–5. Cf. the more recent English translation by Richard J. McCarthy, *Freedom and Fulfillment* (Boston: Twayne, 1980), pp. 73–4, which is somewhat less elegant but rather more reliable. For an informed discussion of Ghazālī's attitude and its possible implications for the course of Islamic science, see Sabra, "Appropriation and Subsequent Naturalization" (cit. n. 2), pp. 239–41.

[17] Ghazālī, *Munqidh,* p. 54; translation by Watt, *The Faith and Practice of al-Ghazālī* (both cit. n. 16), p. 37; cf. McCarthy, *Freedom and Fulfillment* (cit. n. 16), p. 76. This point is closely related to the issue of cause and effect and to the occasionalist position of the Ashʿarite *mutakallims* (theologians).

> The connection between what is habitually believed to be a cause and what is habitually believed to be an effect is not necessary, according to us. . . . Their connection is due to the prior decree of God, who creates them side by side, not to its being necessary in itself, incapable of separation. On the contrary, it is within [divine] power to create satiety without eating, to create death without decapitation, to continue life after decapitation, and so on to all connected things. The philosophers denied the possibility of [this] and claimed it to be impossible.[18]

This is the well-known position of the Ashʿarite theologians,[19] sometimes referred to as Islamic "occasionalism."[20] Exactly how this might work for establishing, say, a science of astronomy (something Ghazālī is not particularly interested in) is unclear. But there are some intriguing hints. For example, in Ghazālī's *al-Munqidh min al-ḍalāl* (Deliverance from error), written as an intellectual biography in the latter part of his life, he warns against the man, "loyal to Islam but ignorant," who tries to defend the faith by "the denial of the mathematical sciences." Such a person "even rejects their theory of the eclipse of sun and moon, considering what they say is contrary to the sacred Law." Ghazālī perceptively notes that someone who understands the certainty of the mathematical proofs involved might conclude "that Islam is based on ignorance and the denial of apodeictic proof" and that such a person "grows in love for philosophy and hatred for Islam." After quoting the Prophet, Ghazālī judges that "there is nothing here obliging us to deny the science of arithmetic which informs us in a specific manner of the paths of sun and moon, and of their conjunction and opposition."[21]

What Ghazālī seems to be proposing is an acceptance of the mathematical aspect of astronomy but not the physical part of that discipline, which might compel one to accept a "natural" motion in the heavens that was somehow independent of God's will. This view has been called "instrumentalist" inasmuch as it would tend to remove astronomers from theoretical considerations regarding the causes of celestial motion and confine them, presumably, to matters of calculation, more likely than not in the service of religion.[22] Of course, interpreted another way, "instrumentalism" could also free astronomers to pursue alternative hypotheses regarding celestial motion and the configuration of the heavens, a point to which we shall return later in this essay.[23]

[18] Al-Ghazālī, *The Incoherence of the Philosophers,* ed. and trans. Michael E. Marmura (Provo, Utah: Brigham Young Univ. Press, 1997), p. 170.

[19] From the eleventh century or so, the Ashʿarites became the dominant theological *(kalām)* group among the Sunnī Muslims, succeeding the Muʿtazilites. They did, though, continue the atomist tradition of their predecessors as well as, for the most part, a rationalist approach to physical and theological matters.

[20] For a lucid discussion of this position in the context of Islamic *kalām,* see Sabra, "Science and Philosophy" (cit. n. 2); he also compares it with the position of Descartes (pp. 29–32).

[21] Ghazālī, *Munqidh,* pp. 51–2. I have somewhat modified Watt's translation, *The Faith and Practice of al-Ghazālī* (cit. n. 16), pp. 34–5; cf. McCarthy, *Freedom and Fulfillment* (cit. n. 16), p. 74.

[22] This position has been laid out by Sabra, "The Appropriation and Subsequent Naturalization of Greek Science" (cit. n. 2), pp. 238–42.

[23] It is worth noting that Ghazālī himself proposes possible alternatives to the view (held by both philosophers and astronomers such as Ptolemy) that the entire heaven is an animal with a soul that causes its motion. On this latter view, see Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 2, pp. 408–10. For Ghazālī's alternatives, see *The Incoherence* (cit. n. 18), pp. 149–51. The possibility, *pace* Sabra, that Ghazālī's position could open up theoretical as well as instrumentalist possibilities needs a much more careful and sustained study than is possible here. (Cf. P. Duhem's controversial views regarding the liberating effects of the medieval European condemnations of Aristotle.)

Ghazālī's warnings about being overly zealous in condemning all of ancient science, even the apodeictic parts, indicates that he was trying to establish some "middle position." But what was the extreme theological position, and how might it work for understanding celestial phenomena? We learn from al-Qūshjī (d. A.D. 1474), a Central Asian scientist associated first with the Samarqand observatory and later with the scientific community of Constantinople (after its conquest by the Ottomans), what these may have been. In his major theological *(kalām)* work, a commentary on Naṣīr al-Dīn al-Ṭūsī's *Tajrīd al-ʿaqāʾid,* he presents what he sees as some of the absurd implications of the standard Ashʿarite denial of natural causation:

> On the assumption {*taqdīr*} of the validity {*thubūt*} of the volitional Omnipotent, it is conceivable that the volitional Omnipotent could by His will {*irāda*} darken the face of the Moon during a lunar eclipse without the interposition of the Earth and likewise during a solar eclipse the face of the Sun [would darken] without the interposition of the Moon; likewise, he could darken and lighten the face of the Moon according to the observed full and crescent shapes.[24]

It is not clear whether he was setting up a straw man or whether Qūshjī was responding to an actual argument he had encountered. Whichever, it is interesting that Ghazālī had, as we have seen, raised just this sort of example in his warning against taking the condemnation of the ancient sciences too far. But in one of the most, if not the most, influential of the late Ashʿarite textbooks, the *Mawāqif fī ʿilm al-kalām* by the Persian ʿAḍud al-Dīn al-Ījī (ca. A.D. 1281–1355), we do not find this extreme viewpoint regarding the explanation of eclipses but, surprisingly, a full and quite well-informed exposition of Ptolemaic astronomy.[25]

By this time, the Ashʿarites had adopted much of the terminology of Greek philosophy, and Ījī was no exception; this did not mean, however, that he adopted the doctrines of Greek philosophy.[26] In particular, he maintained, contra Aristotle, that the universe was atomistic in structure and contingent, depending on God's will to exist from instant to instant. When it came to astronomy, Ījī, who was well acquainted with the basic picture of Ptolemaic astronomy, held that the orbs were "imaginary things" *(umūr mawhūma)* and more tenuous than a spider's web *(bayt al-ʿankabūt).*[27] But Ījī did not draw the conclusion that astronomers' constructions were to be censured or condemned, as implied in the passage from Qūshjī's *Sharḥ al-tajrīd.* Rather he insisted, echoing Ghazālī, that "[religious] prohibition does not extend to them, being neither an object of belief nor subject to affirmation or negation."[28]

Viewed from the perspective of the possible range of religious positions on this matter, one would have thought that the astronomers would have been grateful for this seemingly generous solution to their problems; they could use whatever mathematical tools they needed for their craft as long as they did not declare them real. In

[24] ʿAlī b. Muḥammad al-Qūshjī, *Sharḥ Tajrīd al-ʿaqāʾid* [Tehran, 1890 (?)], p. 186 (line 28) through p. 187 (line 2). A translation and Arabic text of the larger passage of which this is a part is contained in the Appendix. Square brackets ([ ]) are used for editorial additions and explanations. Curly brackets ({ }) are used for original Arabic words or an English translation.

[25] For a brief but informative exposition of this section of Ījī's text, see Sabra, "Science and Philosophy" (cit. n. 2), pp. 34–8.

[26] The adoption by a number of Muslim theologians of the terminology but not necessarily the doctrines of Greek philosophy is a fascinating story, for which see ibid., pp. 11–23.

[27] Ibid., p. 37.

[28] Ibid.

essence, they were being given an "instrumentalist" option. But the astronomers, as we shall see, were hardly thrilled with this solution to the science–religion problem, and we will need to explore why they were not. But before that, we need to ask ourselves another question: Why did Ījī feel the necessity to offer them a solution in the first place? After all, he was not an astronomer himself, and in the main he rejected many of their most fundamental claims about the nature of the universe.

To answer this question, we need to understand something of the context and historical period in which this debate was occurring. For the most part, the participants were either Persians or Central Asians; the period was the aftermath of the Mongol invasions of the thirteenth century, which considerably reshaped the political and intellectual landscape of the area. Not only the traditional political but also the religious leadership in the East was either destroyed or considerably weakened. The Mongols preferred to fill their courts and bureaucracies with some relatively heterodox figures. (The reasons for this are fairly easy to grasp.) The most significant of these from an intellectual standpoint was Naṣīr al-Dīn al-Ṭūsī (A.D. 1201–1274). Ṭūsī was a crucial figure for a number of reasons, but especially because he left behind a corpus of writings that became the main vehicle not only for studying but also for defending Greek science and philosophy, at least in eastern Islam, until modern times. He also wrote on religious matters, and in these works he continued the process of bringing Greek philosophical terms and ideas into the theological context. Though he was born a mainstream Shīʿite and had dabbled for a time with Ismāʿīlism, a much more heterodox Shīʿite doctrine, by the time Ṭūsī began working for the Mongols in 1256, his intellectual allegiance was firmly with the Hellenistic tradition of Islam, which for him was not only a way of unifying the sciences but also a means of transcending religious differences and disputes. As such he hearkens back to an earlier period of Islamic intellectual history, to the Kindīs, the Fārābīs, and especially to Avicenna, for whom Greek philosophy became a kind of transcendent religion. For this Ṭūsī was bitterly reviled by the religious establishment in Mameluke Egypt and Syria, which had mostly escaped the Mongol onslaught. Curiously, though, the Persian theologians, such as Ījī, seem to have been mostly respectful toward him—but not simply respectful. I have no doubt that Ījī, who was born less than ten years after Ṭūsī's death, learned his astronomy, and perhaps even his Greek philosophy, from Ṭūsī's writings; in that case, he was swept up in Ṭūsī's discourse even while disagreeing with it. It should therefore not surprise us that Ījī would try to reassure the Ashʿarite faithful that they had nothing to fear from the surging tide of Hellenistic science and philosophy in Iran while at the same time accommodating Ṭūsī and his many followers by offering them a respectable way to be both good astronomers and good Muslims.[29]

Returning to the astronomers, why would some of them feel uneasy with Ījī's, and for that matter Ghazālī's, compromise? That they would reject this accommodation tells us something about their self-confidence and the strength of their tradition during these centuries.[30] But this was not simply a case of disciplinary pride. Some

[29] For a more detailed and documented discussion of the points made in this paragraph, see Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, pp. 3–20.

[30] The continuing strength of the tradition of science in Islam after A.D. 1200 has only recently been recognized by researchers in the field. The reasons for this long neglect have a great deal to do with the Eurocentric nature of most history of science, which has tended to assume, whether consciously or not, that once the twelfth-century translation movement from Arabic into Latin was com-

were led to this rejection by what they saw as the requirements of an astronomy that could provide a correct picture *(hay'a)* of the universe as well as insight into God's creation (as we have seen). This can be clearly observed in the response of al-Sharīf al-Jurjānī (A.D. 1339–1413) to Ījī's dismissive remarks regarding the "imaginary" and "tenuous" nature of the astronomers' orbs. In addition to his many other hats, which included being a renowned theologian, Jurjānī was an astronomer who wrote a widely read and appreciated commentary to Ṭūsī's astronomical masterpiece, the *Tadhkira.* With his astronomer's turban firmly in place, he responded to Ījī as follows, by trying to explain that the mathematical objects of the astronomers, though "imagined," do have a correspondence with reality:

> Even if they do not have an external reality, yet they are things that are correctly imagined and correspond to what [exists] in actuality {*fī nafs al-amr*} as attested by sound instinct {*al-fiṭra al-salīma*}; they are not false imaginings such as ghouls' fangs, ruby mountains and two-headed men. By means of these [astronomical] notions, the conditions of [celestial] movements are regulated in regard to speed and direction, as perceived [directly] or observed with [the aid of] instruments. [By means of these notions also] discovery is made of the characteristics {*aḥkām*} of the celestial orbs and the earth, and of what they reveal of subtle wisdom and wondrous creation—things that overcome whoever apprehends them with awe, and facing him with the glory of their Creator, prompt him to say: "Our Lord, thou has not created this in vain." This then is a valuable lesson that lies hidden in those words [of the astronomers] and that ought to be cherished, while ignoring whoever is driven to disdain them by mere prejudice.[31]

It is important to note here that Jurjānī's commentary quickly became an integral part of Ījī's textbook and was studied with it in the school tradition. (It was still being studied in Islamic theological schools, such as Cairo's al-Azhar, into the twentieth century!) Thus Ījī's conventionalist/instrumentalist view of astronomical models would have been read with Jurjānī's forceful rejoinder.[32]

Jurjānī, though, while defending astronomy's integrity and its religious value against Ījī's dismissive remarks, does not here deal with the issue of astronomy's alleged dependence upon suspect religious doctrines, such as natural causation and the eternity of the world. Most, though not all, Islamic astronomers felt that at least some of these doctrines were indispensable. As Ṭūsī says in the *Tadhkira,* "Every science has . . . principles, which are either self-evident or else obscure, in which case they are proved in another science and are taken for granted in this science . . . [T]hose of its principles that need proof are demonstrated in three sciences: metaphysics, geometry, and natural philosophy."[33] Thus in addition to mathematics and observation, Ṭūsī is claiming that certain physical and metaphysical principles need to be imported from philosophy. This importation was not taken lightly; indeed, in general one finds among Islamic astronomers a great reluctance to use physical principles from philosophy as a substitute for basing their conclusions on what they

pleted, Islamic intellectuals, having fulfilled their historical mission of preservation for Europe, must have given up their scientific endeavors.

[31] al-Ījī, *Kitāb al-Mawāqif fī 'ilm al-kalām* (with the commentary of al-Jurjānī), ed. Muḥammad Badr al-Dīn al-Na'sānī (Cairo, A.H. 1325/A.D. 1907), pt. vii, p. 108. This is mostly Sabra's translation (with minor changes) from his "Science and Philosophy" (cit. n. 2), p. 39.

[32] One hopes that such examples might give pause to those who insist on treating Islamic religious views as monolithic.

[33] Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, pp. 90–1.

saw as mathematics, which included observation. In this they seem to have followed trends that had already been established in antiquity. In a passage preserved by Simplicius (6th c. A.D.) in his commentary on Aristotle's *Physics,* he quoted Geminus (ca. 1st c. A.D.), who was, we are told, "inspired by the views of Aristotle," to the effect that a clear demarcation can be made between the role of the physicist and the role of the astronomer.[34] "The physicist will in many cases reach the cause by looking to creative force; but the astronomer, when he proves facts from external conditions, is not qualified to judge of the cause, as when, for instance, he declares the earth or the stars to be spherical." This is elucidated in an earlier part of the passage:

> Now in many cases the astronomer and the physicist will propose to prove the same point, e.g., that the sun is of great size or that the Earth is spherical, but they will not proceed by the same road. The physicist will prove each fact by considerations of essence or substance, of force, of its being better that things should be as they are, or of coming into being and change; the astronomer will prove them by the properties of figures or magnitudes, or by the amount of movement and the time that is appropriate to it.[35]

Geminus, no doubt "inspired by the views of Aristotle," declares that the astronomer "must go to the physicist for his first principles, namely, that the movements of the stars are simple, uniform and ordered." But this was a view that was not universally held in antiquity. Ptolemy, for example, refers to physics and metaphysics as "guesswork" and proclaims that "only mathematics can provide sure and unshakeable knowledge to its devotees."[36] One would assume that he would therefore try to avoid physical and metaphysical principles in his astronomy, and, indeed, in the introductory cosmological sections of the *Almagest,* he generally establishes such things as the sphericity of the heavens and the Earth, the Earth's centrality and its lack of motion, according to observational and mathematical principles, in contrast to the more physical means used by Aristotle in, say, *De Caelo.*[37]

Ptolemy's stated position had some major support among Islamic astronomers. The Persian scholar Quṭb al-Dīn al-Shīrāzī (A.D. 1236–1311), onetime student and associate of Naṣīr al-Dīn al-Ṭūsī, paraphrases Ptolemy: "Astronomy is the noblest of the sciences. . . . [I]ts proofs are secure—being of number and geometry—about which there can be no doubt, unlike the proofs in physics and theology."[38]

But several Islamic astronomers note, often with dismay, that Ptolemy had broken his own rule and had used "physical" principles. In particular, the eminent Central Asian scientist Abū Rayḥān al-Bīrūnī (A.D. 973–1048) chides him for using arguments based on physics to prove the sphericity of the heavens in the *Almagest* (I.3) and insists that "each discipline has a methodology and rules and that which is exter-

[34] This is probably in reference to Aristotle, *Physics* II.2; cf. Lloyd, "Saving the Appearances" (cit. n. 9), pp. 212–13.

[35] Translation by T. L. Heath in his *Aristarchus of Samos* (Oxford: Clarendon, 1913), p. 276; reprinted in Morris R. Cohen and I. E. Drabkin, *A Source Book in Greek Science* (Cambridge, Mass.: Harvard Univ. Press, 1948), pp. 90–1. Cf. Lloyd, "Saving the Appearances" (cit. n. 9), pp. 212–14.

[36] *Ptolemy's Almagest* (cit. n. 8), I.1, p. 36.

[37] For a discussion of how this is viewed in the Islamic context, see Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, pp. 38–41; vol. 2, pp. 382–8.

[38] Quṭb al-Dīn al-Shīrāzī, preface to "Nihāyat al-idrāk fī dirāyat al-aflāk," Ahmet III MS 3333 (2), fol. 34b, Topkapı Saray, Istanbul.

nal to it cannot be imposed {*yastaḥkimu*} upon them; therefore, what [Ptolemy] has set forth that is external to this discipline is persuasive rather than necessary."[39]

Looking at Bīrūnī's insistence upon a clear separation of astronomy from physics (or natural philosophy) and Ṭūsī's introductory remarks regarding the need of astronomy for principles from natural philosophy and metaphysics, one might well be tempted to conclude that what we have is a continuation of the ancient debate between the mathematicians (such as Ptolemy, who insisted upon an autonomous astronomy) and the philosophers (represented, as we have seen, by Aristotle and Geminus, who placed the astronomers in a dependent role).[40] But this would be misleading. Even the more philosophically inclined of the Islamic astronomers seem, for the most part, to be intent not only on demarcating astronomy from natural philosophy but also on making it as independent as possible. We have already seen how Avicenna separated astronomy (as a mathematical discipline) from astrology (considered to be part of natural philosophy). Furthermore Ṭūsī himself made clear in the *Tadhkira* that an astronomer should prove most cosmological matters using "proofs of the fact" (that simply establish their existence using observations and mathematics) rather than "proofs of the reasoned fact" (that "convey the necessity of that existence" using physical and/or metaphysical principles); the latter kind of proofs, he tells us, are given by Aristotle in *De Caelo*.[41] In other words, the astronomer should avoid dealing with ultimate causes and instead establish the foundations of his discipline by employing the apodeictic tools of mathematics. This attitude is reinforced as well in the physical principles that Ṭūsī uses to explain regular motion. He analyzes it in such a way that the source of that motion, whether an Aristotelian "nature" (as in the case of the four elements) or a soul (as in the case of the celestial orbs) becomes irrelevant for astronomy; in both cases, he maintains (departing here from Aristotle) that regular motion is always due to an innate principle (*mabda*ʾ = αρχή) called a "nature" (*ṭab*ʿ), thus sidestepping the problem of ultimate causation.[42] Muḥammad Aʿlā al-Tahānawī (18th c. A.D.) nicely summarizes the situation: "In this science [i.e., astronomy], motion is investigated [in terms of] its quantity and direction. The inquiry into the origin (*aṣl*) of this motion and its attribution {*ithbāt*} to the orbs is part of Natural Philosophy (*al-ṭab*ʿ*iyyāt* [sic])."[43]

[39] Abū Rayḥān al-Bīrūnī, *Al-Qānūn al-Masʿūdī*, 3 vols. (Hyderabad: Dāʾirat al-maʿārif al-ʿUthmāniyya, 1954–1956), vol. 1, p. 27. The criticism is directed at Ptolemy's use of "certain physical considerations" regarding the aether to prove the sphericity and circular motion of the heavens (*Ptolemy's Almagest* [cit. n. 8], I.3, p. 40). Elsewhere in the *Qānūn* (vol. 2, pp. 634–5), Bīrūnī strongly criticizes Ptolemy for using assumptions and ideas from outside of astronomy in his *Planetary Hypotheses;* see Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, p. 40, for a translation and discussion of this passage.

[40] Thanks to the recent work of Lloyd and others, we can make such a distinction without resorting to Duhem's reductionist rhetoric of "instrumentalists" versus "realists"; cf. n. 9.

[41] Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, pp. 106–7. For an examination of this passage and its relation to the *quia–propter quid* distinction made in Aristotle's *Posterior Analytics,* see vol. 1, pp. 38–41, and vol. 2, pp. 382, 386–8.

[42] Ṭūsī seems to be trying to account for the fact that the ensouled celestial orbs, even though they have volition, "choose" to move uniformly, unlike entities with souls in the sublunar realm. This was obviously a problem with a long history from ancient to early modern times; see Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 1, pp. 44–6; vol. 2, p. 380. Cf. Harry Wolfson, "The Problem of the Souls of the Spheres from the Byzantine Commentaries on Aristotle through the Arabs and St. Thomas to Kepler," *Dumbarton Oaks Papers* 16 (1962):67–93, and Richard C. Dales, "The De-Animation of the Heavens in the Middle Ages," *J. Hist. Ideas,* 41 (1980):531–50.

[43] Muḥammad Aʿlā b. ʿAlī al-Tahānawī, *Kashshāf iṣṭilāḥāt al-funūn: A Dictionary of the Technical Terms Used in the Sciences of the Musalmans,* edited by Mawlawies Mohammad Wajih, Abd

Let us take stock. Islamic scientists inherited an astronomy from the ancients that already had been differentiated to a lesser or greater degree from natural philosophy. Islamic astronomers, though, carried this process much farther along, and it does not seem unreasonable to see this, at least in part, as a response to religious objections directed at Hellenistic physics and metaphysics, on the one hand, and to religious neutrality toward mathematics, on the other. An attentive reader, though, might still have questions about these tentative conclusions. Why, for example, did someone like Ṭūsī still insist that astronomy needed physical and metaphysical principles even while he contributed toward making it more independent? Did any Islamic astronomer ever defend an astronomy completely independent of philosophy? And finally, can we make a stronger, more explicit and less circumstantial case for a connection between religion and this freeing of astronomy from philosophy? In the remaining part of the essay, I explore these questions.

As we have seen, Bīrūnī implies that the physics one needs for astronomy could be generated within the astronomical context using mathematics and observation; hence one would not need to import "philosophical physics." But was this really feasible? Could one claim that uniform circular motion in the heavens, the straight-line motions of the sublunar realm, and, most important of all, the Earth's state of rest were not based upon Aristotelian physics? As mentioned earlier, Ṭūsī certainly did not believe one could go that far. In part, this was due to one particular instance that became a *cause célèbre* of late medieval Islamic astronomy.[44] In a famous and controversial passage, Ṭūsī explicitly says that the Earth's state of rest cannot be observationally determined and explicitly denies Ptolemy's claim that it can be.[45] In at least this one instance, mathematics and observation fail us, and we therefore need to import from natural philosophy the physical principle that the element earth's natural motion is rectilinear and therefore the Earth cannot rotate naturally. In a more general form, this position was reiterated forcefully and at some length by Ṭūsī's sixteenth-century commentator al-Bīrjandī.[46] This, then, was a bottom line that shows us why some astronomers could not abide Ījī's compromise and why Ṭūsī and others insisted on astronomy's need for natural philosophy.

But not every astronomer agreed with Ṭūsī. In fact his own student Quṭb al-Dīn

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al-Haqq, and Gholam Kadir under the superintendence of A. Sprenger and W. Nassau Lees, 2 vols. (Calcutta: W. N. Lees' Press, 1862), vol. 1, p. 47.

[44] This question, namely whether the Earth's state of rest could be determined by observational tests, is dealt with in my "Ṭūsī and Copernicus: The Earth's Motion in Context," to appear in *Science in Context*. It is also discussed, more summarily, in Ragep, *Naṣīr al-Dīn* (cit. n. 12), vol. 2, pp. 383–5.

[45] The passage, which is from the *Tadhkira* (Ragep, *Naṣīr al-Dīn* [cit. n. 12], vol. 1, pp. 106–7), is as follows: "It is not possible to attribute the primary motion to the Earth. This is not, however, because of what has been maintained, namely that this would cause an object thrown up in the air not to fall to its original position but instead it would necessarily fall to the west of it, or that this would cause the motion of whatever leaves the [Earth], such as an arrow or a bird, in the direction of the [Earth's] motion to be slower, while in the direction opposite to it to be faster. For the part of the air adjacent to the [Earth] could conceivably conform *(yushāyiʿu)* to the Earth's motion along with whatever is joined to it, just as the aether [(here) = upper level of air] conforms *(yushāyiʿu)* to the orb as evidenced by the comets, which move with its motion. Rather, it is on account of the [Earth] having a principle of rectilinear inclination that it is precluded from moving naturally with a circular motion." The similarity to Copernicus, *De Revolutionibus* (Nuremburg, 1543), 6a, lines 16–34, is discussed in the references listed in the preceding footnote.

[46] ʿAbd al-ʿAlī al-Bīrjandī, "Sharḥ al-Tadhkira," Houghton MS Arabic 4285, fol. 39b, Harvard College Library, Cambridge, Mass.; for his more general statements defending the use of natural philosophy in astronomy, see fols. 7a–7b and 38a.

al-Shīrāzī took issue with his sometime master and claimed that one could establish the Earth's state of rest by an observational test, thus obviating the need for importing a physical principle from philosophy.[47] This position, of course, goes well with what we have seen of Shīrāzī's insistence, following Ptolemy, that the mathematical proofs of astronomy were more secure than those of physics and theology; by claiming that observational tests could establish the Earth's state of rest, one could protect astronomy's integrity from the encroachment of natural philosophy and metaphysics.

But because this debate was mainly being carried out within the confines of the scientific literature, the religious dimensions are not very explicit. We may feel justified in claiming that Bīrūnī and Shīrāzī were being influenced by religious considerations in trying to separate astronomy from philosophy, but this is merely a conjecture. In contrast, there can be no doubt as to the religious context of this debate in the already mentioned commentary on Ṭūsī's theological work, the *Tajrīd al-ʿaqāʾid* (Epitome of belief), written by ʿAlī al-Qūshjī.

Qūshjī was the son of Prince Ulugh Beg's falconer and grew up in or close to the Timurid court in Samarqand in the fifteenth century. Samarqand at the time, with its observatory, large scientific staff, brilliant individuals, and scientifically accomplished patron Ulugh Beg, was without a doubt the major center of science in the world and certainly could rival its thirteenth-century predecessor that had been established by Ṭūsī in Marāgha under Mongol patronage.[48] After the assassination of his patron Ulugh Beg, Qūshjī traveled through Iran and Anatolia and eventually assumed a chair in astronomy and mathematics at the college *(madrasa)* of Aya Sofia in the newly Islamic city of Istanbul.[49] It should be emphasized that the teaching of science in the religious schools, and later the establishment of an observatory in Istanbul, were opposed, sometimes bitterly, by the religious establishment.[50] Qūshjī, writing his commentary on Ṭūsī's "Epitome of Belief" after the assassination but before assuming his chair, was no doubt mindful of this religious opposition and sought to answer the objection to astronomy that I have previously quoted from him.

Let us summarize some of the key points he makes. (The entire Arabic text, with my translation, is in the Appendix.) Qūshjī is clearly sensitive to the Ashʿarite

[47] Shīrāzī's discussion can be found in maqāla II, bāb 1, faṣl 4 (fols. 46a–47b) of his "Nihāyat al-idrāk fī dirāyat al-aflāk" (cit. n. 38), which was completed in A.D. 1281. A similar passage is in his "al-Tuḥfa al-shāhiyya fī al-hayʾa," which appeared in A.D. 1284 (bāb II, faṣl 4 [Jāmiʿ al-Bāshā MS 287, Mosul (= Arab League *falak muṣannaf ghayr mufahras* Film 346), fols. 15a–18a, and MS Add. 7477, British Museum, London, fols. 9b–11a]). This section of the "Nihāya" was translated into German by Eilhard Wiedemann in "Ueber die Gestalt, Lage und Bewegung der Erde, sowie philosophisch-astronomische Betrachtungen von Quṭb al-Dīn al-Schīrāzī," *Archiv für die Geschichte der Naturwissenschaften und der Technik* 3 (1912):395–422 (reprinted in E. Wiedemann, *Gesammelte Schriften zur arabisch-islamischen Wissenschaftsgeschichte,* 3 vols. [Frankfurt am Main: Institut für Geschichte der Arabisch-Islamischen Wissenschaften, 1984], vol. 2, pp. 637–64).

[48] On the Samarqand observatory, see Aydin Sayılı, *The Observatory in Islam* (Ankara: Turkish Historical Society, 1960), pp. 259–89. See also E. S. Kennedy, "The Heritage of Ulugh Beg," in idem, *Astronomy and Astrology in the Medieval Islamic World* (Brookfield, Vt.: Ashgate, 1998), no. XI.

[49] See A. Adnan Adivar, "ʿAlī b. Muḥammad al-Ḳūshdjī," *Encyclopedia of Islam,* 2nd ed. (Leiden: Brill, 1960), vol. 1, p. 393, and idem, *La Science chez les Turcs ottomans* (Paris: Maisonneuve, 1939), pp. 33–5.

[50] Adivar discusses this in his *La Science chez les Turcs ottomans* (cit. n. 49). For the Istanbul observatory, which the religious establishment forced to be demolished, see Sayılı, *The Observatory* (cit. n. 48), pp. 289–305.

position on causality, and he makes the interesting observation that part of their objection to it, at least as regards astronomy, has to do with the astrological contention of a causal link between the positions of the orbs and terrestrial events (especially "unusual circumstances"). To get around such objections, Qūshjī insists that astronomy does not need philosophy, since one could build the entire edifice of orbs necessary for the astronomical enterprise using only geometry, reasonable suppositions, appropriate judgments, and provisional hypotheses. These premises allow astronomers

> to conceive {*takhayyalū*} from among the possible approaches the one by which the circumstances of the planets with their manifold irregularities may be put in order in such a way as to facilitate their determination of the positions and conjunctions of these planets for any time they might wish and so as to conform with perception {*ḥiss*} and sight {*ʿiyān*}.

What this will allow us to do is make presumptions that best explain "or save" the phenomena. Of course God might, by His will, cause the phenomena directly; Qūshjī gives the example of God darkening the Moon without the Earth's shadow and causing an eclipse. But just as we go about our everyday lives using what he calls ordinary *(ʿādiyya)* and practical *(tajribiyya)* knowledge, thus should we proceed in science. Here he allows himself a bit of sarcasm, arguing that we could (for example) claim that after we had left our house one day, God turned all the pots and pans into human scholars who took to investigating the sciences of theology and geometry; insofar as we feel confident in assuming that this has not happened, so also should we have confidence that the heavens normally follow a regular pattern that we have the capacity to explain. We do not, however, need to make the further claim that our explanation represents the only possible one; in this way, Qūshjī believes he has made astronomy independent of philosophy.

What makes Qūshjī's position especially fascinating are some of the repercussions it had for his astronomical work. Since he claims to be no longer tied to the principles of Aristotelian physics, he feels free to explore other possibilities, including the Earth's rotation. Clearly within the tradition of the debate that we outlined earlier, he agrees with Ṭūsī, thus countering Ptolemy and Shīrāzī, and argues that the question of the Earth's motion cannot be determined by observation. But unlike Ṭūsī, he refuses to settle the matter by appealing to Aristotelian natural philosophy. Instead he states that "it is not established that what has a principle of rectilinear inclination is prevented from [having] circular motion."[51] He then ends with a startling conclusion: "Thus nothing false *(fāsid)* follows [from the assumption of a rotating Earth]."[52]

Qūshjī also showed that he was true to his principles in his elementary astronomy work, *Risālah dar ʿilm-i hayʾa;* in it, he took the highly unusual step of dispensing with the section on natural philosophy with which almost all other similar treatises began.[53]

[51] Qūshjī, *Sharḥ Tajrīd* (cit. n. 24), p. 195. The same point is made by Copernicus in *De Revolutionibus* (cit. n. 45), I.8.

[52] Ibid. Qūshjī's position, and the possible relation of this Islamic debate to Copernicus, is dealt with more fully in my "Ṭūsī and Copernicus" (cit. n. 44).

[53] This work was originally in Persian and, given the evidence of the extant manuscripts, quite popular. It was translated by Qūshjī himself into Arabic and dedicated to Mehmet, the Conqueror (Fātiḥ) of Constantinople, whence it was called *al-Risāla al-Fatḥiyya.* Cf. Tofigh Heidarzadeh, "The Astronomical Works of ʿAlī Qūshjī" (in Turkish), M. A. thesis, (İstanbul Univ., 1997), pp. 24, 30–32,

But in freeing himself from Aristotle, did Qūshjī also free himself from seeking reality? In other words, instead of being the precursor of Copernicus, is he rather the predecessor of Osiander, the Lutheran minister whose anonymous preface to *De Revolutionibus* proclaimed, "[L]et no one expect anything certain from astronomy"? My tentative answer is that I do not think Qūshjī's position is instrumentalist in the same sense as Ījī's (or Osiander's).[54] And the reason, in a way, is quite simple. Ījī was a theologian, whereas Qūshjī, in his heart of hearts, was a scientist, whose work was ultimately a way to know and understand God's creation. Qūshjī makes this clear with his remarks at the end of his discussion of premises. The astronomers' models may be calculating devices that cannot be claimed as unique, but nevertheless they are, he tells us, a source of wonder, because of their correspondence with the observed phenomena. He continues, "Whoever contemplates the situation of shadows on the surfaces of sundials will bear witness that this is due to something wondrous and will praise [the astronomers] with the most laudatory praise." Qūshjī here seems to echo the words of Jurjānī, cited earlier, in which the latter countered Ījī by insisting that through astronomy we can behold God's subtle wisdom and wondrous creation. Qūshjī, though, in rejecting the view that somehow we can know true reality, is attempting to present a rather more sophisticated position: that the correspondence between our human constructions and external reality is itself a source of wonder.[55]

Ultimately, then, for Jurjānī, Qūshjī, and many other Islamic scientists, Ījī's well-meant instrumentalist compromise was rejected. As would also occur in Europe, they held that one could glorify God with science; one could not glorify God with conventions.

IV. CONCLUSION

In the generation or two following Qūshjī, science in the Islamic East continued to thrive. Several major astronomical works were produced by two contemporaries of Copernicus, ʿAbd al-ʿAlī al-Bīrjandī (d. A.D. 1525 or 1526) and Shams al-Dīn al-Khafrī (fl. A.D. 1525). As we have already noted, Bīrjandī continued the debate regarding the Earth's motion and strongly defended the need to use both natural philosophy and metaphysics in astronomy. In fact, he quotes and directly argues against the passage that I have quoted from Qūshjī.[56] In developing his position, Bīrjandī

41; E. Ihsanoğlu et al., *Osmanlı Astronomi Literatürü Tarihi,* 2 vols. (Istanbul: IRCICA, 1997), vol. 1, pp. 27–35; and David Pingree, "Indian Reception of Muslim Versions of Ptolemaic Astronomy," in *Tradition, Transmission, Transformation* (cit. n. 2), p. 474.

[54] For a comparison of Ījī and Osiander, see Sabra, "Science and Philosophy" (cit. n. 2), pp. 38–9. It would be quite interesting to compare the later manifestations of Ījī's position in the Islamic schools with what Robert Westman has called the "Wittenberg interpretation" of Copernican theory, which allowed the hypothesis of a Sun-centered universe to be studied in sixteenth-century Lutheran circles while it condemned any attempt to embrace it as true or real.

[55] Cf. Albert Einstein, *Ideas and Opinions* (New York: Dell, 1973), p. 285: "The very fact that the totality of our sense experiences is such that by means of thinking (operations with concepts, and the creation and use of definite functional relations between them, and the coordination of sense experiences to these concepts) it can be put in order, this fact is one which leaves us in awe, but which we shall never understand. One may say 'the eternal mystery of the world is its comprehensibility.' It is one of the great realizations of Immanuel Kant that the postulation of a real external world would be senseless without this comprehensibility."

[56] Bīrjandī, "Sharḥ al-Tadhkira" (cit. n. 46), fol. 7a–7b. Curiously, Bīrjandī does not mention Qūshjī by name but simply refers to him as "one of the eminent scholars" *(baʿḍ al-afāḍil).*

makes an interesting analysis of what might occur if the Earth were rotating (which he himself rejects) and hypothesizes something quite close to Galileo's notion of "circular inertia."[57]

The point is not to claim that Copernicus (or Galileo) read Bīrjandī (though this does not now seem as far-fetched as it might once have appeared), but rather to indicate the remarkable intensity of scholarship and diversity of opinion that continued in Islamic lands well into the sixteenth century (and in fact even later). This is a time that until recently was seen as a period characterized by the steep decline, or even absence of scientific work. Since the vast majority of texts written during this late period in the history of Islamic science have yet to be studied (much less published), many exciting surprises might well be anticipated. But whether or not this proves to be the case, the present discussion of one small aspect of the situation of science in Islam should alert us to the fact that science was still a major force well into the early modern period and can shed light not only on Islamic intellectual history but the history of European science as well. And one hopes that part of that light will help us to understand the relation between science and religion in both the Islamic world and in Christendom.

That religion played a role in Islamic science—perhaps even a crucial role—should not surprise us. What is surprising, especially to a Western audience in the twenty-first century, is that that role was not simply one of opposition and obstruction but rather, at least sometimes, of constructive engagement. I hope I will not be misunderstood as being an apologist for religion if I make the historical observation that religious attacks on aspects of science and philosophy in both Islam and Christendom led to a more critical attitude toward scientific and philosophical doctrines and that this often resulted in some interesting and even productive outcomes. This has been a point increasingly accepted by historians of European science, and one that would greatly help Islamists, and those who write on Islam, to understand the complexity of the interaction of secular and religious knowledge in Islamic civilization.

[57] Ibid., fol. 37a. See further my "Ṭūsī and Copernicus" (cit. n. 44).

Appendix
Concerning the Supposed Dependence of Astronomy upon Philosophy

ʿAlī al-Qūshjī

[186] It is stated that the positing of the orbs in [that] particular way depends upon false principles taken from philosophy {*falsafa*}, for example, the denial of the volitional Omnipotent and the lack of possibility of tearing and mending of the orbs, and that they do not intensify nor weaken in their motions, and that they do not reverse direction, turn, stop, nor undergo any change of state but rather always move with a simple motion in the direction in which they are going, as well as other physical and theological matters, some of which go against the Law {*sharʿ*} and some of which are not established inasmuch as their proofs are defective {*madkhūla*}. For if it were not based upon those principles, we could say that the volitional Omnipotent by His will moves those orbs in the observed order, or we could say that the stars move in the orb as fish do in water, speeding up and slowing down, going backward, stopping and moving forward without need for those many orbs. But by assuming the validity {*thubūt*} of those principles, what they have stated is an affirmation {*ithbāt*} of a cause based upon the existence of an effect; but this will not be valid unless one knows the correlation {*musāwāt*} [note under the line: "i.e., the correlation of the effect to the cause"]. But this is not known, since there is no necessary [connection]; nor is there a demonstration {*burhān*} of the impossibility that the observed irregularities are for reasons other than the ones they have stated.

However, there is nothing to the above, since it stems from a lack of study of the problems and proofs of this discipline. Most of [its principles] are suppositions [{*muqaddamāt ḥadsiyya*} = (literally) conjectural premises] that the mind {*ʿaql*}, upon observing the above-mentioned irregularities, resolves to posit according to an observed order and a reliance upon geometrical premises that are not open to even a scintilla of doubt. For example: the sighting of the full and crescent shapes [of the Moon] in the manner in which they are observed makes it certain that the light of the Moon is derived from the Sun and that a lunar eclipse occurs because of the interposition of the Earth between the Sun and Moon, and that a solar eclipse occurs because of the interposition of the Moon between the Sun and the eye, this despite the assertion of the validity of the volitional Omnipotent and the denial

This appendix is my translation of ʿAlī al-Qūshjī's *Sharḥ tajrīd al-ʿaqāʾid* (cit. n. 24), p. 186 (line 11) through p. 187 (line 29); part of this passage is cited by Bīrjandī in his "Sharḥ al-Tadhkira" (cit. n. 46), fol. 7a–7b, and a good part of it is quoted by Tahānawī in his *Kashshāf iṣṭilāḥāt al-funūn* (cit. n. 43), vol. 1, pp. 48–9.

شرح تجريد العقائد
لعلي القوشجي
(ص ١٨٦-١٨٧)

وما يقال من أنّ إثبات الأفلاك على الوجه المخصوص مبنيّ على أصول فاسدة مأخوذة من الفلاسفة من نفي القادر المختار وعدم تجويز الخرق والالتيام على الأفلاك وأنّها لا تشتدّ في حركاتها ولا تضعف ولا يكون لها رجوع ولا انعطاف ولا وقوف ولا اختلاف حال غيرها بل تكون أبداً متحرّكة حركة بسيطة في الجهة التي تتحرّك إليها إلى غير ذلك من المسائل الطبيعيّة والإلهيّة التي بعضها مخالف للشرع وبعضها لم يثبت لكون أدلّتها مدخولة إذ لو لم يُبْنَ على تلك الأصول نقول إنّ القادر المختار بحسب إرادته يحرّك تلك الأفلاك على النظام المشاهد أو نقول إنّ الكواكب تتحرّك في الفلك كالحيتان في الماء تسرع وتبطئ وترجع وتقف وتقيم من غير حاجة إلى تلك الأفلاك الكثيرة وعلى تقدير ثبوت تلك الأصول فما ذكروا إثبات للملزوم بناء على وجود لازمة ولا يصح إلّا إذا علم المساواة ﴿تحت السطر : «أي مساواة اللازم للملزوم»﴾ وليست بمعلومة إذ لا ضرورة ولا برهان على امتناع أن يكون تلك الاختلافات المشاهدة لأسباب أخر غير ما ذكروا : فليس بشيء إذ منشاؤه عدم الاطّلاع على مسائل هذه الفنّ ودلائله فإنّ أكثرها مقدّمات حدسيّة يجزم العقل بثبوتها عند مشاهدة الاختلافات المذكورة على النظام المشاهد والاستعانة بالمقدّمات الهندسيّة التي لا يتطرّق إليها شائبة اشتباه مثلاً مشاهدة التشكّلات البدرية والهلاليّة على الوجه المرصود يوجب اليقين بأنّ نور القمر مستفاد من الشمس وأنّ الخسوف إنّما هو بسبب حيلولة الأرض بين الشمس والقمر والكسوف إنّما هو بسبب حيلولة القمر بين الشمس والإبصار مع القول بثبوت القادر المختار ونفي

of those above-mentioned principles. For the validity of the volitional Omnipotent and the denial of those principles does not preclude the situation being as stated; at most, they would allow for other possibilities. For example: on the assumption of the validity {*thubūt*} of the volitional Omnipotent, it is conceivable that the volitional Omnipotent could by His will {*irāda*} darken the face of the Moon during a lunar eclipse without the interposition of the Earth and likewise during [187] a solar eclipse the face of the Sun [would darken] without the interposition of the Moon; likewise, he could darken and lighten the face of the Moon according to the observed full and crescent shapes. Furthermore, on the assumption of the possibility of the irregularity in the motions as well as the other circumstances of the celestial bodies {*falakiyyāt*}, it is possible that one half of each of the luminaries is luminous whereas the other is dark. The luminaries would then move about their centers in such a way that their dark sides would face us during lunar and solar eclipses, either completely, when they are total, or partially in magnitude, when they are not total. By an analogous argument, the situation of the full and crescent shapes [can be explained]. Nevertheless, despite the raising of the previously mentioned possibilities {*iḥtimālāt*}, we affirm {*najzimu*} that the situation is as stated, namely that the Moon derives its light from the Sun and that lunar and solar eclipses occur because of the interposition of the Earth and Moon. This same sort of presumption {*iḥtimāl*} is made in ordinary {*ʿādiyya*} and practical {*tajribiyya*} knowledge {*ʿulūm*}—indeed, for all necessary [direct?] knowledge {*ḍarūriyyāt*}. For we assert that after leaving a house the pots and pans inside do not turn into human scholars who take to investigating the sciences of theology and geometry, despite the fact that the volitional Omnipotent might make it thus in virtue of His will.

But [on the other hand], on the assumption that the principle {*mabdaʾ*} is made causal {*mūjab*}, an unusual circumstance {*waḍʿ gharīb*} may be realized {*yataḥaqqaqu*} from the positions of the orbs; according to the doctrine of the proponents of causality, the manifestation of that unusual occurrence is required by the dependency of events upon the positions of the orbs. This and other examples are embedded in the skepticism {*shubah*} of those who condemn necessary knowledge.

The upshot is that that which is stated in the science of astronomy {*ʿilm al-hayʾa*} does not depend upon physical {*ṭabīʿiyya*} and theological {*ilāhiyya*} premises {*muqaddamāt*}. The common practice by authors of introducing their books with them is by way of following the philosophers; this, however, is not something necessary, and it is indeed possible to establish [this science] without basing it upon them. For of what is stated in [this science]: (1) some things are geometrical premises, which are not open to doubt; (2) others are suppositions {*muqaddamāt ḥadsiyya*}, as we have stated; (3) others are premises determined by {*yaḥkumu bihā*} the mind {*al-ʿaql*} in accordance with the apprehension {*al-akhdh*} of what is most suitable and appropriate. Thus they say that

تلك الأصول المذكورة فإنّ ثبوت القادر المختار وانتفاء تلك الأصول لا ينفيان أن يكون الحال على ما ذكر غاية الأمر أنّهما يجوّزان الاحتمالات الأخر مثلاً على تقدير ثبوت القادر المختار يجوز أن يسوّد القادر المختار بحسب إرادته وجه القمر عند الخسوف من غير حيلولة الأرض وكذا عند /١٨٧/ الكسوف وجه الشمس من غير حيلولة القمر وكذا يجوز أن يُسَوِّدَ وينوّر وجه القمر على ما يشاهد من التشكّلات البدريّة والهلاليّة وأيضاً على تقدير جواز الاختلاف في حركات الفلكيّات وسائر أحوالها يجوز أن يكون أحد نصفي كلّ من النيّرين مضيئاً والآخر مظلماً ويتحرّك النيّران على مركزيهما بحيث يصير وجهاهما المظلمان مواجهين لنا في حالتي الخسوف والكسوف إمّا بالتمام وذلك إذا كانا تامّين وإمّا بالبعض على قدرهما إذا كانا غير تامّين وعلى هذا القياس حال التشكّلات البدرية والهلالية لكنّا نجزم مع قيام الاحتمالات المذكورة على أنّ الحال على ما ذكر من استفادة القمر نوره من الشمس وأنّ الخسوف والكسوف إنّما يكونان بسبب حيلولة الأرض والقمر ومثل هذا الاحتمال قائم في العلوم العاديّة والتجربيّة بل في جميع الضروريّات فإنّا نجزم بأن أواني البيت بعد خروجنا عنه لم تصر أناساً فضلاء محقّقين في العلوم الإلهيّة والهندسيّة مع أنّ القادر المختار يجوز أن يجعلها كذلك بحسب إرادته بل على تقدير أن يكون المبدأ موجَباً ﴿تحت السطر : «أي مضطرا»﴾ يجوز أن يتحقّق وضع غريب من الأوضاع الفلكيّة فيقتضي ظهور ذلك الأمر الغريب على ما هو مذهب القائلين بالإيجاب من استناد الحوادث إلى الأوضاع الفلكيّة وغير ذلك ممّا هو مركوز في شبه القادحين في الضروريّات والحاصل أنّ المذكور في علم الهيئة ليس مبنيّاً على المقدّمات الطبيعيّة والإلهيّة وما جرت به العادة من تصدير المصنّفين كتبهم بها إنّما هو بطريق المتابعة للفلاسفة وليس ذلك أمراً واجباً بل يمكن إثباته من غير ابتناء عليها فإنّ المذكور فيه بعضه مقدّمات هندسيّة لا يتطرّق إليها شبهة وبعضه مقدّمات حدسيّة كما ذكرنا وبعضه مقدّمات يحكم بها العقل بحسب الأخذ بما هو الأليق والأولى كما يقولون إنّ

the convexity of the deferent touches the convexity of the parecliptic at a common point, as is the case with the concavities. They have no other reason {*mustanad*} [for this] except that it is more proper that there not be any useless part in the heavens. Similarly they say that the Sun's orb is above the orb of Venus and of Mercury, since the best arrangement and order dictate that that which is farther away or having a larger circuit has the slowest motion among the planets; or that in the order and arrangement the Sun is in the middle—in the manner of the tassel of a necklace—between those that reach the four elongations from it, i.e., the sextiles, quadratures, trines, and oppositions, and those whose elongation is only the least, i.e., the sextile; and (4) other premises that they state are indefinite {*ʿalā sabīl al-taraddud*}, there being no final determination {*al-jazm*}. Thus they say that the irregular speed in the Sun's motion is either due to an eccentric or to an epicyclic hypothesis without there being a definitive decision for one or the other.

If one were to grant that the establishing of the orbs in the manner in which they have stated was based on those false principles, this would doubtless be due to a claim by the practitioners of this science that there was no possibility other than the approach we have stated. But if their claim was that it was possible for it to be by this approach, even though it was possible that it could be by other approaches, one could not then imagine a dependency. It is more than sufficient for them to conceive {*takhayyalū*} from among the possible approaches the one by which the circumstances of the planets with their manifold irregularities may be put in order in such a way as to facilitate their determination of the positions and conjunctions of these planets for any time they might wish and so as to conform with perception {*ḥiss*} and sight {*ʿiyān*}, this in a way that the intellect and the mind find wondrous {*tataḥayyaru*}. Whoever contemplates the situation of shadows on the surfaces of sundials will bear witness that this is due to something wondrous and will praise [the astronomers] with the most laudatory praise.

محدّب الحامل مماسّ محدّب الممثّل على نقطة مشتركة وكذا مقعّره لمقعّره ولا مستند لهم غير أنّ الأولى أن لا يكون في الفلكيات فضل لا يحتاج إليه وكما يقولون إنّ فلك الشمس فوق فلك الزهرة وعطارد لأنّ حسن الترتيب والنظام يقتضي أن يكون ما هو أكثر بعداً أو أعظم مداراً أبطأ حركة من الكواكب وأن يكون الشمس واسطة في النظم والترتيب بمنزلة شمسة القلادة بين ما يبعد عنها الأبعاد الأربعة أعني التسديس والتربيع والتثليث والمقابلة وبين ما لا يبعد عنها إلّا أقلّ الأبعاد المذكورة أعني التسديس وبعض مقدّمات يذكرونها على سبيل التردّد دون الجزم كما يقولون إنّ اختلاف حركة الشمس بالسرعة والبطؤ إمّا بناءً على أصل الخارج وإمّا بناءً على أصل التدوير من غير جزم بأحدهما ولو سلّم أنّ إثبات الأفلاك على الوجه الذي ذكروه يتوقّف على تلك الأصول الفاسدة فلا شكّ أنّه إنّما يكون ذلك إذا ادّعى أصحاب هذا الفنّ أنّه لا يمكن إلّا على الوجه الذي ذكرنا أمّا إذا كان دعواهم أنّه يمكن أن يكون على ذلك الوجه وإن أمكن أن يكون على الوجوه الأخر فلا يتصوّر التوقّف حينئذ وكفى بهم فضلاً أنّهم تخيّلوا من الوجوه الممكنة ما ينضبط به أحوال تلك الكواكب مع كثرة اختلافاتها على وجه تيسّر لهم أن يعيّنوا مواضع تلك الكواكب واتّصالات بعضها مع بعض في كلّ وقت أرادوا بحيث يطابق الحسّ والعيان مطابقة تتحيّر فيها العقول والأذهان ومن تأمّل في أحوال الأظلال على سطوح الرخامات شهد بأنّ هذا لشيء عجاب وأثنى عليهم بثناء مستطاب

Islamic Reactions to Ptolemy's Imprecisions

F. Jamil Ragep

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

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

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

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

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

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

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

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

The Measurement of the Earth

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

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

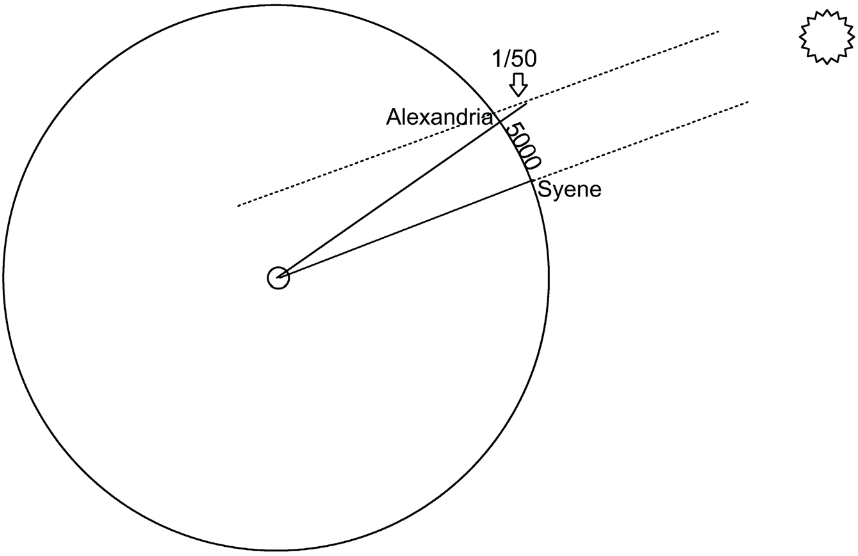

Fig. 1 Eratosthenes' measurement of the Earth's circumference

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

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

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

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

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

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

The conclusion that these values were unreliable is, interestingly enough, the judgment reached during the early ʿAbbāsid period. We have very good evidence that indicates that the Caliph al-Maʾmūn (r. 813–833) was not happy with Ptolemy's 180,000-stade figure and wished to have it tested. (The following is a summary of a more extensive treatment in Ragep 1993, v. 2, pp. 501–510, which includes references; cf. King 2000 and Mercier 1992, both of whom evince a certain degree of skepticism regarding the Maʾmūnī measurement of the Earth. Though certain details are in doubt, in my opinion the amount of contemporaneous evidence makes a strong case for some sort of scientific observations ordered by Maʾmūn. Furthermore, there is no reason to distrust the evidence regarding Muḥammad ibn Mūsā, which is based upon his own words.) A text attributed to Muḥammad ibn Mūsā, one of the famous Banū Mūsā who was a protégé of Maʾmūn, as well as later sources, indicates that Muḥammad undertook a "confirmation" by simply taking the latitude difference of two Syrian cities, Raqqa and Palmyra (assumed on the same meridian) with Ptolemaic latitudes of 35°20′ and 34°, respectively. (The modern values are 35°58′ and 34°35′; in actuality, Raqqa is about 45′ east of Palmyra.) Since the Ptolemaic distance was given as 90 Roman miles, this did more or less confirm the Ptolemaic value of 66 ⅔ miles/meridian degree or 180,000 stades for the Earth's circumference. (Note this is based upon a Roman mile of 7.5 Ptolemaic stades rather than the 8 Attic stades presumably used by Eratosthenes; see above.) What is interesting about this story is that Maʾmūn seems not to have been happy with this "confirmation," perhaps because he was, correctly, not convinced that his astronomers knew the exact length of a Roman mile. Maʾmūn's reaction, judging from a number of reports, was then to order a scientific expedition to find a meridian degree by means of a survey. A group was sent to the Plain of Sinjār in upper Mesopotamia. (The Sinjār area is located in the northwestern part of Iraq and constitutes approximately 2,250 km^2 of a flat plain. Sinjār Mountain (1,460 m height) is the major geomorphological feature in the area.) The method we find described in Ibn Yūnus (d. 1009 CE) is instructive. Two groups, one going due north, the other due south, laid out survey lines using long ropes until the Sun's altitude descended or ascended one degree. The two groups then came back to the starting point and compared notes and arrived at an average figure of 56 Arabian miles. (There are other reports giving slightly different numbers.) Since we know that each of these miles was 4,000 cubits, and we also know that the cubit used at the time of Maʾmūn was approximately 49 cm, Carlo Nallino in the early 1900s concluded that the Maʾmūnī value for the circumference of the Earth was within a few hundred kilometers (off by less than 1%). It is instructive to compare this with a recent attempt by the MIT physicist Phillip Morrison and his wife Phyllis Morrison to measure a meridian line along 370 miles of US 183, running between Nebraska and Kansas. Taking two observations of Antares at the beginning and end of the trip and using the car's odometer to measure distance, they came up with a circumference of 26,500 statute miles, off by about 6% (actual value 24,900) (as reported by Dutka 1993, p. 64).

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

The Length of the Year and the Sun's Motion

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

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

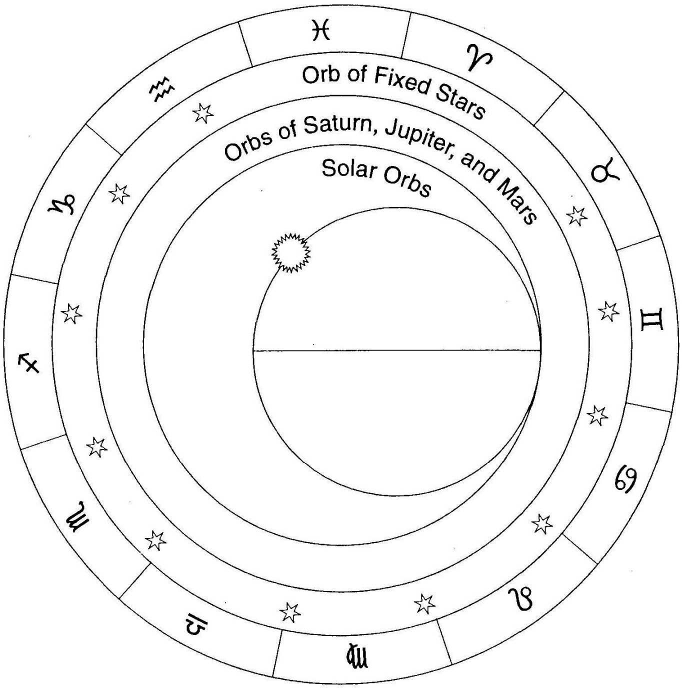

Fig. 2 A continuous speeding up (by trepidation or some other means) of the motion of the Eighth/Fixed Star Orb is here transmitted to the Sun's orbs, causing the Sun to reach the fixed vernal equinox sooner than it would with a simple monotonic precession. Battānī claims this might explain the differences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

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

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

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

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

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

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

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

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

The Obliquity of the Ecliptic

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

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

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

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

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

Confirming vs. Testing

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

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

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

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

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

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

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

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

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

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

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Section II

The Ṭūsī-couple and Its Ambulations

From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple

F. Jamil Ragep

IN DISCUSSIONS OF THE POSSIBLE CONNECTIONS between Nicholas Copernicus and his Islamic predecessors, the Ṭūsī-couple has often been invoked by both supporters and detractors of the actuality of this transmission. But, as I have stated in an earlier article, the Ṭūsī-couple, as well as other mathematical devices invented by Islamic astronomers to deal with irregular celestial motions in Ptolemaic astronomy, may be of secondary importance when considering the overall significance of Islamic astronomy and natural philosophy in the bringing forth of Copernican heliocentrism.[1] Nevertheless, the development and use of Naṣīr al-Dīn al-Ṭūsī's (597–672/1201–74) astronomical devices does provide us with important evidence regarding the transmission of astronomical models and with lessons about intercultural scientific transmission. So in this chapter, I attempt to summarize what we know about that transmission, beginning with the first diffusion from Azerbaijan in Iran to Byzantium and continuing to the sixteenth century. Although there are still many gaps in our knowledge, I maintain, based on the evidence, that intercultural transmission is more compelling as an explanation than an assumption of independent and parallel discovery.

THE MULTIPLE VERSIONS OF THE ṬŪSĪ-COUPLE

It will be helpful if we first analyze what exactly is meant by the "Ṭūsī-couple." The first thing to notice is that the term "Ṭūsī-couple" does not refer to a single device or model but actually encompasses several different mathematical devices that were used for different purposes (see table 7.1). Because this understanding is not always upheld in the modern literature, there has been considerable divergence, often leading to

Table 7.1 Versions of the Ṭūsī-couple

Name of device (Ragep)	*Description*	*Intended use*	*Other names*	*First appearance*
Mathematical rectilinear version (fig. 7.1)	Two circles uniformly rotating in opposite directions – the smaller internally tangent to the larger with half its radius – that produces rectilinear oscillation of a given point	Replacing the equant (and its like) in planetary models	Plane version (Saliba and Kennedy) Spherical version with parallel axes and radii in the ratio of 1:2 (Di Bono, 136) Device (*aṣl*) of the large and the small (circles)* Two-unequal-circle version	643/1245
Physicalized rectilinear version (fig. 7.2)	Three solid spheres based on the mathematical version that produces rectilinear oscillation of a given point	Replacing the equant (and its like) in planetary models	Physicalized two-circle version with maintaining sphere	643/1245
Two-equal-circle version (fig. 7.5)	Mathematically equivalent to the rectilinear version but using two circles of equal radius, each circle's circumference going through the centre of the other, one circle rotating twice as fast as the other to produce rectilinear oscillation of a given point	To account for Ptolemaic motions needing curvilinear oscillation on a great circle arc (but actually produces oscillation on a chord)	Plane version with equal radii (Di Bono, 137–8) Pseudo-curvilinear version	644/1247
Three-sphere curvilinear version (figs 7.6 and 7.7)	Three concentric spheres with different axes, one inside the other, rotating uniformly	Intended to account for Ptolemaic motions needing curvilinear oscillation on a great circle arc (mostly works as intended but with a minor distortion)	Spherical version (Saliba and Kennedy) Spherical version with oblique axes and equal radii (Di Bono, 136)	659/1261
Two-sphere curvilinear version (fig. 8.4)	Truncated version of the full three-sphere curvilinear version	For certain astronomical models (used by Ibn Naḥmias and later Copernicus)		ca. 1400

* It was often referred to as such in astronomical texts after Ṭūsī. He himself does not explicitly use this term to refer to the device although it is implied in the terminology he uses in the *Tadhkira*, as distinct from the *Ḥall* (see below).

Sources: Mario Di Bono, "Copernicus, Amico, Fracastoro, and Ṭūsī's Device: Observations on the Use and Transmission of a Model," *Journal for the History of Astronomy* 26, no. 2 (1995): 133-54; F. Jamil Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy (al-Tadhkira fī ʿilm al-hayʾa)* (New York: Springer-Verlag, 1993), vol. 2, 427-56; George Saliba and Edward S. Kennedy, "The Spherical Case of the Tūsī Couple," *Arabic Sciences and Philosophy* 1, no. 2 (1991): 285-91.

confusion, about what exactly the Ṭūsī-couple is. This confusion, in turn, has made it difficult to trace transmission. So a quick historical overview is in order.[2]

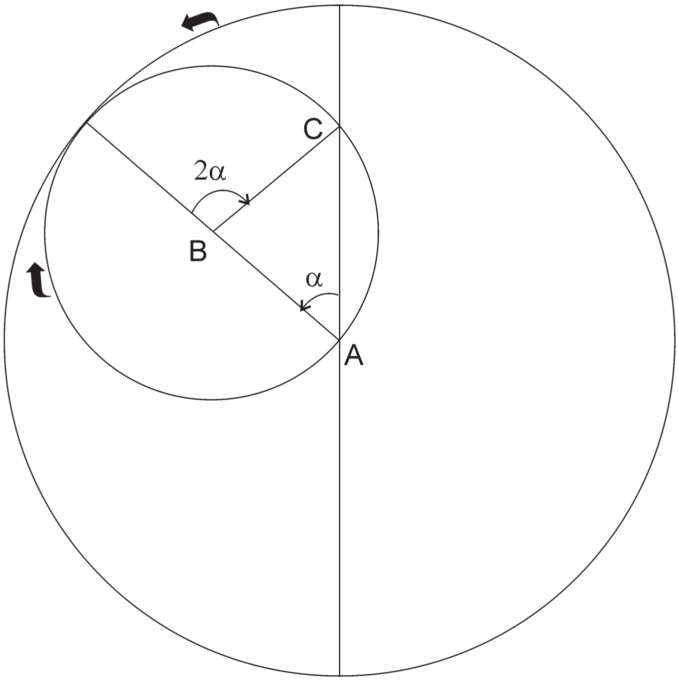
MATHEMATICAL RECTILINEAR VERSION

The first version of the Ṭūsī-couple was announced by Naṣīr al-Dīn al-Ṭūsī in a Persian astronomical treatise entitled *Risālah-i Muʿīniyya* (*Muʿīniyya* Treatise), the first version of which was completed on Thursday, 2 Rajab 632 (22 March 1235).[3] Dedicated to the son of the Ismāʿīlī governor of Qūhistān, in the eastern part of modern Iran, the treatise is a typical *hayʾa* (cosmographical) work, one that provides a scientifically based cosmology covering both the celestial and terrestrial regions. But in presenting the Ptolemaic configuration of the Moon's orbs and their motions, Ṭūsī notes that the motion of the epicycle centre on the deferent is variable, which is inadmissible according to an accepted rule of celestial physics, namely that all individual motions of orbs in the celestial realm should be uniform. He goes on to say, "This is a serious doubt with regard to this account [of the model], and as yet no practitioner of the science has ventured anything. Or, if anyone has, it has not reached us." But "there is an elegant way to solve this doubt but it would be inappropriate to introduce it into this short treatise." He then teasingly turns to his patron: "If at some other time the blessed temper of the Prince of Iran, may God multiply his glory, would be so pleased to pursue this problem, concerning that matter a treatment will be forthcoming." In the chapter on the upper planets and Venus, as well as the one on Mercury, he makes a similar claim, namely that he has a solution that will be presented later. In addition to the problem of the irregular motion of the deferent (sometimes referred to as the "equant problem," although it is somewhat different for the Moon), Ṭūsī brings up another "doubt" or difficulty, namely that pertaining to motion in latitude – that is, north or south of the ecliptic. Claudius Ptolemy had rather complex models in his *Almagest* and *Planetary Hypotheses* that generated quite a bit of discussion among Islamic astronomers. One of these was Abū ʿAlī al-Ḥasan ibn al-Haytham (d. ca. 430/1040), who objected to the lack of physical movers for these models and provided his own in a treatise that is currently not extant. However, Ṭūsī refers to it in the *Muʿīniyya* and also notes that it is not entirely satisfactory; but as with his purported models for longitude, he eschews any details.[4]

Since Ṭūsī claims to have an elegant solution, one assumes that he would have presented it to his patron in short order. But, as we shall see, he waited almost ten years to present his new models. One clue to the delay could well be overoptimism on the part of the young Naṣīr al-Dīn; he claimed in the *Muʿīniyya* that he had solutions for all the planets, but as it turned out he was never able to solve the complexities of Mercury. Indeed, as an older man many years later, he was to admit this setback in his *Al-Tadhkira fī ʿilm al-hayʾa* (Memoir on the Science of Astronomy): "As for Mercury, it has not yet been possible for me to conceive how it should be done."[5]

The partial solution occurs in a short treatise that was again dedicated to his patron's son, Muʿīn al-Dīn. This work has come to us with a variety of names: *Dhayl-i Muʿīniyya* (Appendix to the *Muʿīniyya* [Treatise]), *Ḥall-i mushkilāt-i Muʿīniyya* (Solution to the Difficulties of the *Muʿīniyya*), *Sharḥ-i Muʿīniyya* (Commentary on the *Muʿīniyya*), and so on.[6] In all cases of which I know, the work is explicitly tied to *Risālah-i Muʿīniyya*, leading one to assume that it must have been written a short time after the treatise to which it is appended. This assumption, however, turns out not to be correct. Thanks to the recent discovery in Tashkent of a manuscript witness of the *Dhayl-i Muʿīniyya* with a dated colophon, we can now date this treatise, as well as the first appearance of the Ṭūsī-couple, to 643/1245: "The treatise is completed. 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 [i.e., late October 1245], within the town of Tūn in the garden known as Bāgh Barakah."[7] As we can infer from the colophon, Ṭūsī was still in the employ of the Ismāʿīlī rulers of Qūhistān in southern Khurāsān. Tūn, present-day Firdaws, lay some eighty kilometres (or fifty miles) west-north-west of the main town of the region, Qāʾin, which was the primary regional capital of the Ismāʿīlīs.[8]

It clearly took Naṣīr al-Dīn longer than he anticipated to reach a solution, and even then it was not complete by any means. This "first version" of the Ṭūsī-couple consisted of a device composed of two uniformly rotating circles that could produce oscillating straight-line motion in a plane between two points. One of these two circles was twice as large as the second, the smaller one being inside the larger one and tangent at a point (see figure 7.1). The rotation of the smaller circle was twice that of the larger one. Although mathematically speaking the production of an oscillating point on a straight line could also be produced by the small circle "rolling" inside the larger, Ṭūsī is explicit that the larger circle "carries" (*mī bard*) the smaller one. The reason for this is that Ṭūsī will transform these circles into the equators of solid orbs rotating in the celestial realm, where any penetration of one solid body by another is expressly forbidden.[9] The transformation into solid orbs, the "physicalized rectilinear version," is shown in figure 7.2. Note that one needs a

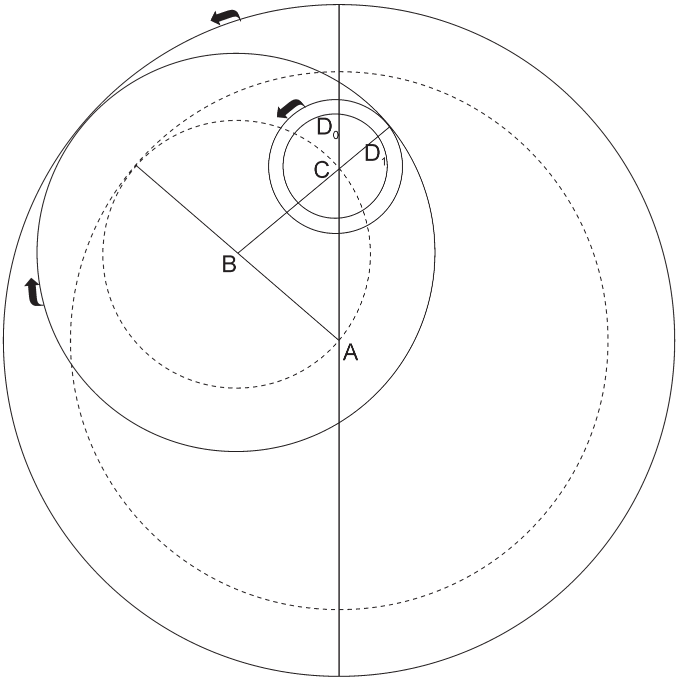


7.1 Mathematical rectilinear version of the Ṭūsī-couple.

third orb, what he calls the "enclosing sphere [*muḥṭīa*] for the epicycle," in order not to disrupt the epicycle; this third orb keeps D aligned with C and A. More on this later when I discuss Nicole Oresme.

Ṭūsī then proceeds to use the device to construct his alternative to Ptolemy's lunar model. It will be instructive, and important for tracing transmission, to compare this model from the *Ḥall* with the model Ṭūsī would present in *Al-Tadhkira fī ʿilm al-hayʾa*, which, unlike the *Muʿīniyya* and the *Ḥall*, was written in Arabic rather than Persian. The first version of the *Tadhkira* was completed in 659/1261 when Ṭūsī was in the employ of his new patrons, the Mongol Īlkhānid conquerors of Iran. Table. 7.2 provides a summary.

In the *Tadhkira*, Ṭūsī has made a number of changes in the lunar model that he first presented in the *Ḥall*. The most obvious is the change in terminology: "the dirigent orb" (*mudīr*) has now become the "large sphere," and the "epicycle's deferent orb" (*ḥāmil*) has been renamed the "small sphere." This change is most likely due to the confusion resulting from using the terms "dirigent" and "deferent," which are employed for other parts of the planetary models, to also designate the two outer spheres



7.2 Physicalized rectilinear version of the Ṭūsī-couple.

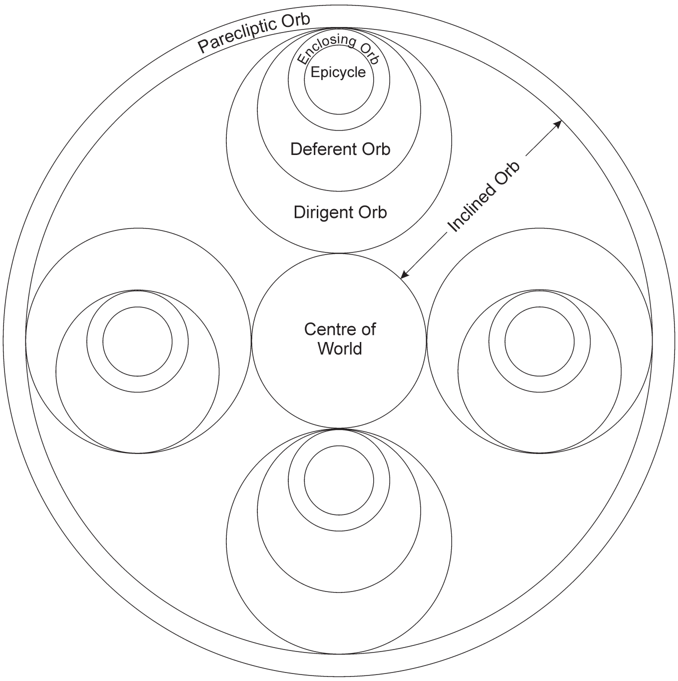
making up the Ṭūsī-couple. Another more significant change is dividing the inclined orb of the *Ḥall* into two orbs in the *Tadhkira*, namely a different inclined orb (actually the inclined orb of the Ptolemaic model) and a different deferent. The resultant motion of these two orbs is 13;14°/day in the sequence of the signs, which is different from the 13;11°/day of the *Ḥall*'s inclined orb. In fact, this difference corrects the mistake in the *Ḥall*, where Ṭūsī made the inclined orb move at the rate of the mean motion of the Moon (*wasaṭ-i qamar*), apparently forgetting that this rate would result in the pareclíptic motion being counted twice.

From this overview, we can conclude that the rectilinear Ṭūsī-couple and its applications to various planetary models emerged in stages and rather slowly. After Ṭūsī came up with the idea, apparently when writing the *Muʿīniyya*, it took many years before he felt comfortable enough presenting it in the *Ḥall*. But even then, the model still had a number of problems in both terminology and substance, which weren't solved until the writing of the *Tadhkira* some fifteen years later. But as we shall see, these differences help us in tracing the transmission of the device and models. They also help us to make the case, almost a

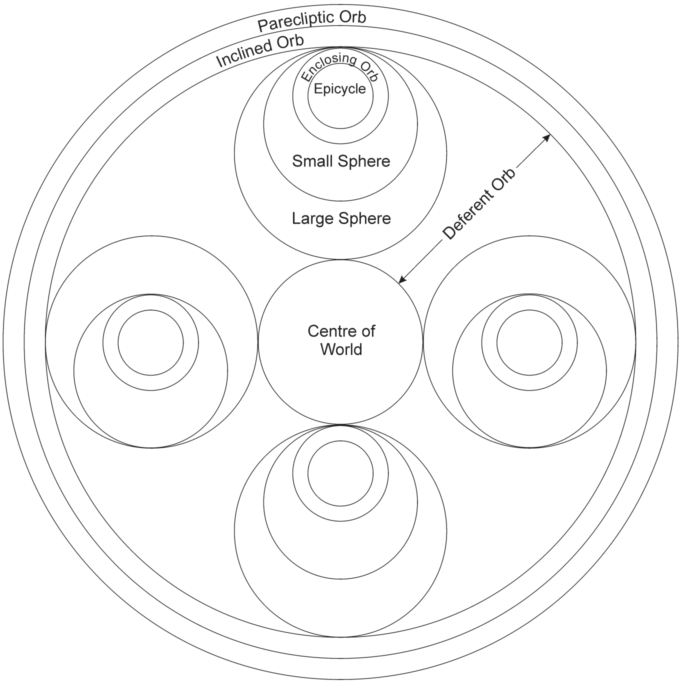
Table 7.2 Ṭūsī's lunar models from the *Ḥall* and the *Tadhkira*

Ḥall		*Tadhkira*	
Orbs	*Parameters*	*Orbs*	*Parameters*
Parecliptic orb (*mumaththal*)	0;3°/day (cs)	Parecliptic orb (*mumaththal*)	0;3°+/day (cs)
Inclined orb (*māʾil*)	13;11°/day (s)	Inclined orb (*māʾil*) Deferent orb (*ḥāmil*)	11;9°/day (cs) 24;23°/day (s) Net: 13;14°/day (s)
Dirigent orb (*mudīr*)	24;23°/day (s) or (cs)	Large sphere (*al-kabīra*)	24;23°/day (s)
Epicycle's deferent orb (*ḥāmil-i tadwīr*)	48;46°/day (opposite direction of dirigent)	Small sphere (*al-ṣaghīra*)	48;46°/day (cs)
Epicycle's enclosing orb (*muḥīṭ bi-tadwīr*)	24;23°/day (same direction as dirigent)	Enclosing orb (*al-muḥīṭa*)	24;23°/day (s)
Epicycle (*tadwīr*)	13;4°/day (cs)	Epicycle (*al-tadwīr*)	13;4°/day (cs)

Note: Motion in the sequence (s) or countersequence (cs) of the signs is determined by the orb's apogee point.



7.3 Lunar model from the *Ḥall*, showing six orbs in four different positions.



7.4 Lunar model from the *Tadhkira*, showing seven orbs in four different positions.

truism in the history of science, that such devices and models take time to evolve and be perfected. A sudden appearance of a complete and perfected theory or model should make us wary of claims of no transmission or influence.

TWO-EQUAL-CIRCLE VERSION

In addition to the rectilinear version of the Ṭūsī-couple, Ṭūsī also developed a curvilinear version that was meant to produce a linear oscillation on a great circle arc. This version was used to rectify a number of difficulties in Ptolemy's latitude theory, as well as a curvilinear oscillation caused by the prosneusis point in the latter's lunar model. In fact, as Ṭūsī mentions, it could be used wherever a curvilinear oscillation was needed, such as for motions of the celestial poles and vernal equinox, if observation showed such phenomena to be real.[10]

But before the final curvilinear version was introduced in the *Tadhkira* in 1261, it evolved slowly over a considerable period of Ṭūsī's lifetime. In the *Muʿīniyya*, when discussing the models for latitude, Ṭūsī notes that Ibn al-Haytham had dealt with latitude in a treatise and gives a brief sketch of his theory. But he finds this solution lacking, and criticizes it without going into details since "this [i.e., the *Muʿīniyya*] is not the place to discuss it." Despite this criticism, Ṭūsī does not claim to have a solution for the problem of latitude, unlike the case of the longitudinal motions of the Moon and planets.[11] In the *Ḥall*, Ṭūsī refrains from the earlier criticism of Ibn al-Haytham and instead presents the latter's model for latitude. Basically, this is an adaptation of the Eudoxan system of homocentric orbs, described in Aristotle's *Metaphysics*, applied to Ptolemy's latitude models, which used motion on small circles to produce latitudinal variation.[12] It is curious that Ṭūsī offers no model of his own, nor does he note, as he does later in the *Tadhkira*, that motions in circles will produce not only latitudinal variations but also unwanted longitudinal changes.

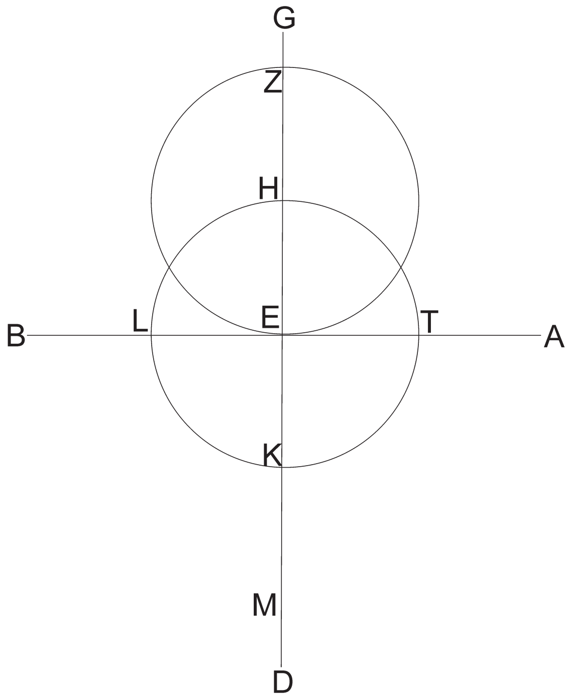
But a little over a year later, on 5 Shawwāl 644 (13 February 1247), to be exact, Ṭūsī published a sketch of another version of his couple that was meant to resolve some of the difficulties of Ptolemy's latitude models.[13] This version was presented in the context of his discussion of these models in book 13 of his *Taḥrīr al-Majisṭī* (Recension of the *Almagest*). After presenting a summary of Ptolemy's latitude model for the planets, and his special pleading regarding the complicated nature of these models, which include the endpoints of the epicycle diameters rotating on small circles to produce latitude in a northerly or southerly direction,[14] Ṭūsī provides the following comment:

> I say: this discussion is external to the discipline (*ṣināʿa*) [201b] and is not persuasive for this matter. For it is necessary for a practitioner of this discipline to establish circles and bodies having uniform motions according to an order and arrangement [such that] from all of them [circles and bodies] these various perceived motions will be constituted. For then these motions being on the circumferences of the mentioned small circles, just as they result in the epicycle diameters departing from the planes of the eccentrics in latitude northward and southward, so too will they result in their departing from alignment with the centre of the ecliptic, or from being parallel with diameters in the plane of the ecliptic with the exact same longitude, through accession and recession in the exact same amount of that latitude. And this is contrary to reality. And it is not possible to say that that difference is perceptible in latitude but not perceptible in longitude since they are equal in size and distance from the centre of the ecliptic.

Now, if the diameter of the small circle were made in the amount of the total latitude in either direction, and one imagines that its centre moves on the circumference of another circle equal to it whose centre is in the plane of the eccentric in the amount of half the motion of the endpoint of the diameter of the epicycle on the circumference of the first circle and opposite its direction, there will occur a shift to the north and south in the amount of the latitude without there occurring a forward or backward [motion] in longitude.

To show this, let AB be a section of the eccentric and GD be from the latitude circle that passes through the endpoint of the diameter of the epicycle. And they intersect at E. EZ EM are the total latitude in the two directions. And EH is half of it in one of them. We draw about H with a distance EH a circle EZ and about E with a distance HE a circle HTKL. We imagine the endpoint of the diameter of the epicycle at point Z to move on circle EZ in direction G to B and the center H to move on circle HTKL in the direction G to A with half that motion. Then it is clear that when H traverses a quarter and reaches T, Z will traverse a half and reach E. Then when H traverses another quarter and reaches K, Z will traverse another half and reach M. And when H traverses a third quarter and reaches L, Z will traverse another half and will reach E once again. And when H completes a rotation, Z will return to its original place so that it will always oscillate in what is between ZM on the line GD without inclining from it in directions AB. This is the explanation of this method. However, it requires that the time the diameter is in the north be equal to the time it is in the south; in reality, it is different from that. As for what is said regarding its motion on the circumference of a circle about a point that is not its centre, as stated by Ptolemy, this needs consideration to verify it according to what has preceded. We now return to the book [i.e., the *Almagest*].[15]

There are several things we can say about this device. First of all, as Ṭūsī notes, it does not accurately model Ptolemy's latitude theory since it results in equal times in the north and in the south.[16] Second, the motion of the epicycle endpoint is uniform with respect to the epicycle's mean apex, which again is contrary to what Ptolemy's model requires. Third, and more significant for our purposes, this model is actually a slightly modified version of the rectilinear Ṭūsī-couple that was first presented in the *Ḥall*. The problem, however, is that the motion of the endpoint of the epicycle's diameter is on a straight line, ZM, whereas the necessary motion should be on a great circle arc. This problem is curious. Surely, Ṭūsī is aware that the motion in latitude should occur on the surface of a sphere; why, then, does he have this rather stripped-down version of his couple that can result only in rectilinear oscillation? The answer, it seems, is that at this point he does not have a curvilinear version. He is dissatisfied with Ptolemy's small circles and also realizes that

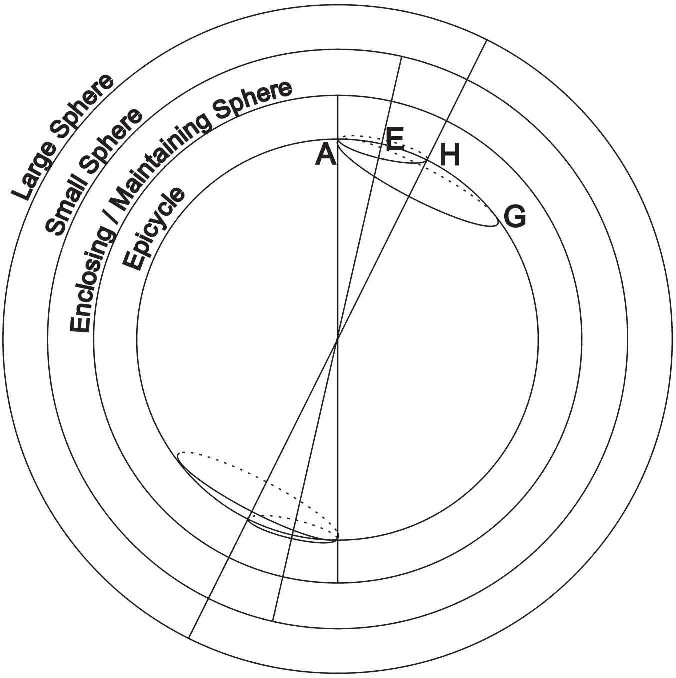


7.5 Two-equal-circle version of the Ṭūsī-couple.

Ibn al-Haytham's model does little more than provide a solid-sphere basis for the inadequate small circles, but all he has to offer is a kind of vague notion that his couple might be modified to create the necessary motion in latitude. He clearly is still in the thinking stage.

THREE-SPHERE CURVILINEAR VERSION

Ṭūsī does not in fact offer a true curvilinear version until almost fifteen years later, during the first part of Dhū al-qaʿda 659 (September or October 1261), at which time he publishes the first version of his *Al-Tadhkira fī ʿilm al-hayʾa.* There, he puts forth a model consisting of three additional orbs enclosing the epicycle that are meant to produce a curvilinear oscillation that results in the motion in latitude (see figures 7.6 and 7.7).[17] It is interesting that Ṭūsī presents this new model as a modification of Ibn al-Haytham's earlier attempt,[18] which, as we have seen, simply provides a physical basis for Ptolemy's small circles using

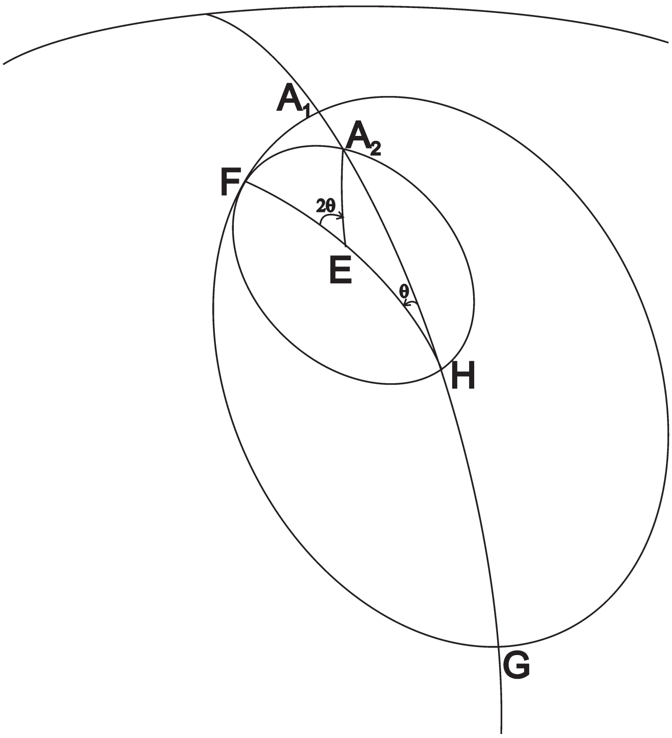


7.6 Complete curvilinear version of the Ṭūsī-couple, showing three embedded solid orbs (or hollowed-out spheres) with different axes enclosing the epicycle.

homocentric orbs, which we may call the Eudoxan-couple (see figures 7.8 and 7.9).[19] In addition to using the curvilinear version to resolve the difficulties related to the motion of the planetary epicycles in latitude, Ṭūsī notes that it may also be used for moving the inclined orb of the two lower planets in latitude and for resolving the irregular motion brought about by the Moon's so-called prosneusis point. Finally, he states that this version could also be used to model the variable motion of precession ("trepidation") and the variability of the obliquity if these two motions were found to be real.[20] As we will see, these suggestions for extended usage of the couple turn out to be significant.

USE OF THE COUPLE FOR *QUIES MEDIA*

There is another issue related to the rectilinear couple that may have a bearing on tracing transmission. Quṭb al-Dīn al-Shīrāzī, one of Ṭūsī's associates in Marāgha and subsequently one of the eminent philosophers and scientists at Mongol courts in Tabrīz, remarks in his *Al-Tuḥfa al-shāhiyya fī al-hayʾa*, written after Ṭūsī's death in 684/1285, that "it is

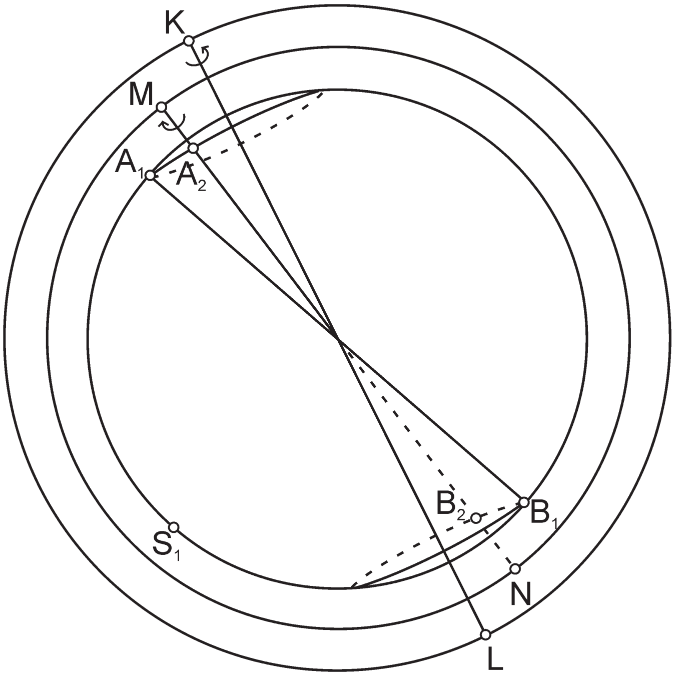


7.7 Polar view of the complete curvilinear version, showing the motion of the endpoint of the diameter of the epicycle along a great circle arc.

possible to use this [lemma] to show the impossibility (*imtināʿ*) of rest between a rising and falling motion on the line (*samt*) of a terrestrial diameter."[21] The idea here is that the Ṭūsī-couple, by showing that oscillating straight-line motion can be continuous, counters Aristotle's contention that there would be a "moment of rest" (*quies media*) between rising and falling.[22] This view was contested, and in fact Shams al-Dīn al-Khafrī (fl. 932/1525), in his commentary on the *Tadhkira*, disputes Shīrāzī on this point. As we shall see, there are echoes in Latin Europe of this debate, which could well be due to transmission.

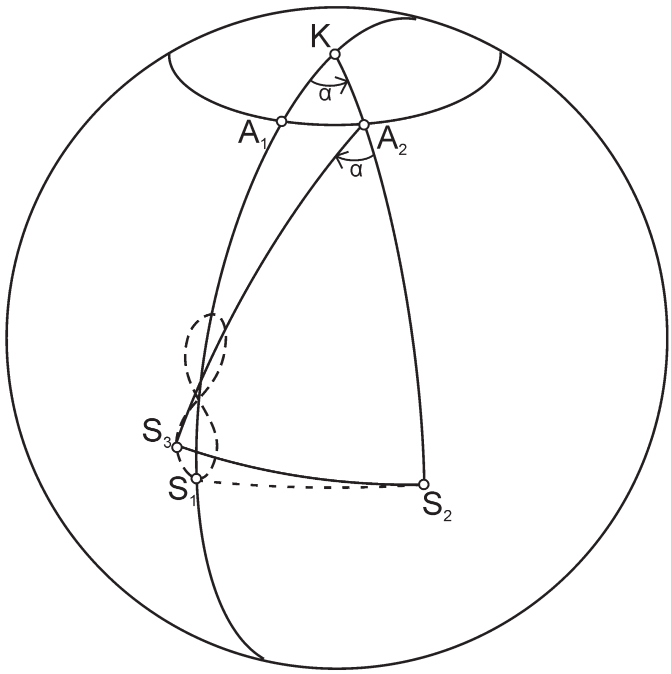
Sightings of the Ṭūsī-Couple in Non-Islamic Cultural Contexts before 1543[23]

We should note here that the development of the different versions of the Ṭūsī-couple, and the models based upon them, took place over a twenty-five-year period. The use, further development, and discussion of



7.8 Ibn al-Haytham's Eudoxan-couple, showing two spheres.

the various versions of the couple in an Islamic context, such as I have noted above in the case of the *quies media* debate, can be traced over many centuries; the couple, which became known as the "large and small model [or *hypothesis*]"[24] (*aṣl al-kabīra wa-l-ṣaghīra*), was incorporated into other theories and systems, as well as explained in a number of commentaries and independent works. There can be no question that these later developments and discussions in an Islamic context, in whatever language, can be traced back to one or more of Ṭūsī's works. However, when we cross cultural boundaries, the situation becomes less clear-cut, and here one is faced with a variety of opinions about the origin of "Ṭūsī-couple sightings" in these other cultural contexts. With the exception of one example, and possibly a second, there are no cases of translations of Ṭūsī's writings on the couple into non-Islamicate languages. So in order to advocate that the appearance, or "sightings," of the couple in other contexts is due to intercultural transmission, we will be faced in most cases with the need to postulate either nonextant texts or nontextual transmission. Such arguments will thus need to be based on plausibility rather than direct evidence; but many arguments of



7.9 Motion of the endpoint of the diameter of the epicycle on a circular path rather than a great circle arc.

transmission in the history of science are based upon such plausibility arguments and often become virtually irrefutable, especially when precise numeration is involved. The case for the transmission of the Ṭūsī-couple is not quite so iron-clad, but given the various types of evidence that can be brought to bear, I argue that independent rediscovery, especially multiple times, becomes much less compelling.

But before presenting that evidence, I shall list and discuss the various sightings. Because of the problematic nature of some of the material, especially in the case of Oresme, I will devote considerably more space to some examples than to others.

Transmission to Byzantium

The first known appearance of the Ṭūsī-couple outside Islamic societies occurred around 1300, most likely through the efforts of a certain Gregory Chioniades of Constantinople, who is known for translating a number of astronomical treatises from Persian (or perhaps Arabic) into

Greek.[25] Included in these works is a short theoretical treatise that has been dubbed *The Schemata of the Stars.*[26] The lunar model in the *Schemata* uses the Ṭūsī-couple, and there are diagrams in one of the codices that greatly resemble diagrams in Ṭūsī's works.[27]

As I argue in a recent paper, the *Schemata* is mostly a translation of certain parts of Ṭūsī's *Muʿīniyya*, with the Ṭūsī-couple and lunar model coming from the *Ḥall*;[28] thus what we are dealing with is a case of the abridgement into Greek of a Persian original that we can confidently identify. It seems that Chioniades was tutored by a certain Shams al-Dīn al-Bukhārī (almost certainly Shams al-Dīn Muḥammad ibn ʿAlī Khwāja al-Wābkanawī al-Munajjim), who chose to teach his tutee using Ṭūsī's earlier Persian works rather than his revised and up-to-date *Tadhkira.*[29] It is not known whether this was for linguistic reasons (Chioniades perhaps knowing Persian but not Arabic) or because of a reluctance to give a Byzantine access to cutting-edge astronomical knowledge.[30] In any event, we can safely say that the version of the Ṭūsī-couple and lunar model found in the *Schemata* came from the *Ḥall* since both have six orbs for the lunar model and the same mistake in the inclined orb, namely 13;11°/day (s) rather than the correct 13;14°/day (s).[31]

The surprising conclusion is that the first known transmission of Ṭūsī's models came from his earlier Persian works, which contained a significant error. Furthermore, the only planetary model transmitted was the lunar model, and there is no hint in the *Schemata* of the models for latitude, either from the *Taḥrīr* or from the *Tadhkira.* Nevertheless, there can be no question that some of Ṭūsī's innovations had made their way into Greek by the early fourteenth century, and the existence in Italy of the only three known manuscript witnesses strongly suggests that the transmission of this knowledge had made it into the Latin world by the fifteenth century.[32]

I should also mention here that since Chioniades read the *Ḥall*, he would no doubt have been exposed to Ibn al-Haytham's latitude theory, which made up chapter 5 of that work.[33] This influence may well have relevance to the question of how that rather obscure theory might have reached scholars in Latin Europe.

The Ṭūsī-Couple and the Eudoxan-Couple in Latin Europe

Historians have identified multiple sightings of the Ṭūsī-couple and the Eudoxan-couple (i.e., Ibn al-Haytham's) in Latin Europe, starting in the fourteenth century. What follows is a chronological list, although certainly not exhaustive, of the figures associated with these sightings.

AVNER DE BURGOS

The Jewish philosopher and polemicist Avner de Burgos (ca. 1270–1340), a convert to Christianity who became known as Alfonso de Valladolid, proved a theorem in a Hebrew work identical to a rectilinear Ṭūsī-couple. Tzvi Langermann has noted that Avner "adduces his theorem in a mathematical context, the stated purpose of which is 'to construct (*li-ṣayyer*) a continuous and unending rectilinear motion, back and forth along a finite straight line, without resting when reversing direction [literally: "between going and returning"].'"[34] What is interesting here is that this use of the couple, as part of the *quies media* debate, is not something one finds in Ṭūsī but is to be found in the work of his associate and student Shīrāzī. As we will see, this may well have implications for the transmission of the couple to Europe.

NICOLE ORESME

Nicole Oresme (ca. 1320–82), in his *Questiones de spera*, which treats Johannes de Sacrobosco's *On the Sphere of the World*, describes some sort of model that will produce reciprocating rectilinear motion from three circular motions. Both Garrett Droppers and Claudia Kren raised the possibility that Oresme was somehow influenced by "Ṭūsī's device."[35] Recently, André Goddu has challenged this possibility and has raised another one, namely that Oresme hit upon a solution similar to Ṭūsī's for producing rectilinear motion from circular motions – although still leaving open the (weak?) alternative that Oresme may have come across some description of it.[36] Because Goddu's speculations, discussed below, depend upon several misinterpretations of both Ṭūsī and Oresme, we need to carefully consider what Oresme is proposing. Here is Kren's translation of the relevant passage with my suggested revisions:[37]

> Concerning this problem [i.e., whether celestial bodies move in circular motion], I propose three interesting conclusions. First, it is possible for some planet to be moved perpetually according to its own nature in a rectilinear motion composed of several circular motions. This motion can be brought about by several intelligences, any one of which may endeavor to move in a circular motion, *nor would this purpose be in vain* [rev: and (the intelligence) is not frustrated in this endeavor].
>
> Proof: Let us propose, conceptually, as do the astrologers, that A is the *deferent* [rev: deferent circle] of some planet, or its center; B is the *epicycle* [rev: epicycle circle] of the same planet; and C is the body of the planet, or its center; I take these [latter two?] as equivalent. Let us also imagine line BC from the center of the epicycle to the center of the planet, and CD, a line in the planet on which BC

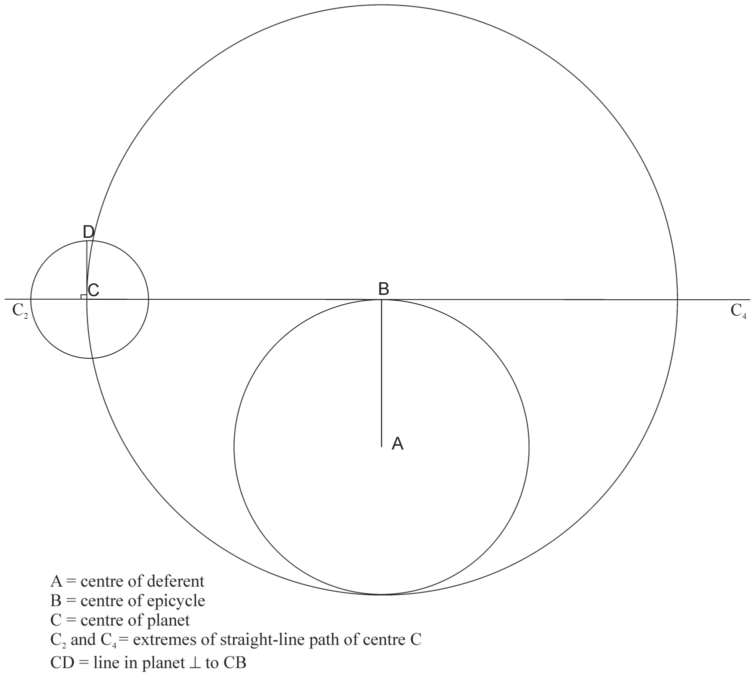
falls perpendicularly. Let circle A move on its center toward the east, and B toward the west. The planet, C, revolves on its own center toward the east. Moreover, since line BC is of constant length, because it is a radius, let us propose that the *distance* [rev: amount] B descends *in* [rev: according to] the motion of the deferent is *the distance which* [rev: as much as] point C *may ascend* [rev: ascends] with the motion of the epicycle. From this one can obviously observe that point C in some definite time will be moved in a straight line. *Let us then further assume that point B would ascend by its own motion on just the circumference on which it may descend with the motion of the planet* [rev: Let us then further assume that the circuit on which B would ascend by its own motion is as much as the motion of the planet descends]. It is further clear that point D will move continually on the same line; thus the entire body of the planet will be moved to some terminus in a rectilinear motion and will return again with a similar motion.[38]

To analyze this passage, and to understand Oresme's intention, we should note from the last sentence that the *body* of the planet is meant to move rectilinearly. Furthermore, not only does the centre of the planet (C) move in a straight line but a certain point (D), which is the endpoint of a planetary radius (CD), does as well.

Droppers, and Goddu who follows him, do not take the rectilinear motion of D into account; inexplicably, both have D at the end of a planetary radius whose starting point is C, the centre of the planet (see figure 7.10).[39]

In contrast, Kren does follow Oresme's text and provides a plausible reconstruction based upon a more or less correct interpretation of Ṭūsī's *Tadhkira* as she found it in Carra de Vaux's flawed 1893 French translation. Oresme provides no diagram, and Kren must admit that "as it appears in Oresme's *Questiones de spera*, the passage makes no sense whatsoever."[40] Nevertheless, following Kren's lead and making a few modifications, I believe we can reconstruct both Oresme's model and his intention.[41] In essence, what Kren proposes is that Oresme is not discussing the simple two-circle Ṭūsī-couple, which results in the rectilinear oscillation of a point between two extrema, but rather Ṭūsī's physicalized rectilinear version, which we have already encountered above.[42]

With reference to figure 7.2 and using Oresme's description, let us take A to be the centre of the deferent, B the centre of the epicycle, and C the centre of the planet. The solid lines indicate the outer surfaces of solid bodies, whereas the dotted lines indicate "inner equators" of these solid bodies. Note that the solid orbs are the actual moving bodies; they "accidentally" produce the mathematical Ṭūsī-couple indicated by the broken lines. So for this model to work, the epicycle (B) needs to move with twice the angular speed as the deferent (A) and in the opposite



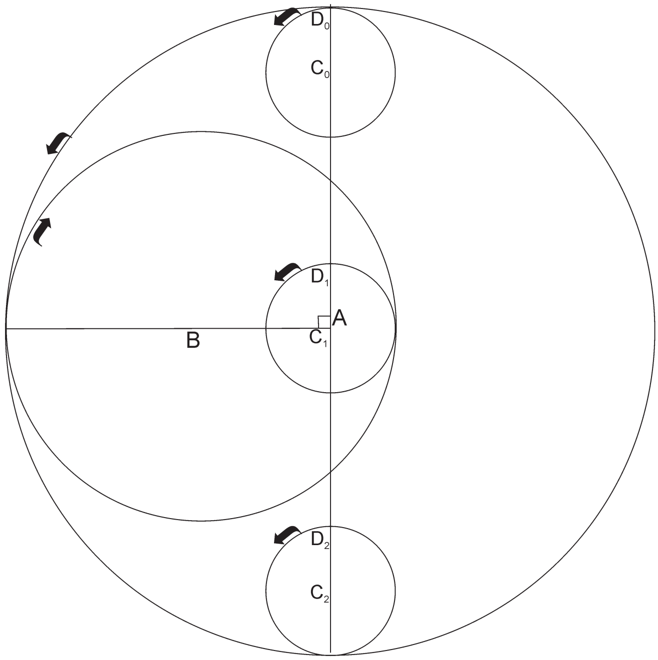
7.10 Oresme's construction as proposed by Droppers.

direction. These movements will then result in the planet's centre (C) oscillating on a straight line. They will not, however, result in the apex of the planet (D) moving rectilinearly. As shown in the diagram, when the deferent and epicycle have rotated from an initial position (where A, B, C, and D were on the same line), D will move from D_0 to D_1. To deal with this issue, Ṭūsī introduces what he calls an enclosing sphere (*kura muḥīṭa*), which is shown in the diagram as an orb enclosing and concentric with the planet (C). This orb would then have the job of moving D from D_1 back to its initial position of D_0. Since $\angle BAC = \angle D_0CD_1$, the enclosing sphere needs to move with the same speed and direction of the deferent (A) in order to keep D oscillating on the straight line.

Kren has assumed that Oresme is simply copying Ṭūsī's physicalized rectilinear version, and she has some tortured readings that would introduce this fourth, enclosing orb into Oresme's account. But Oresme clearly says he only needs three circular motions, and in fact Ṭūsī's commentators indicate that one could replace orb C and the enclosing orb by combining their motions into a single orb. Ṭūsī does not do so, probably because for him orb C is an epicycle, not an otherwise stationary planet, and he does not want to lose its parameters, which are critical for Ptolemaic planetary theory, by combining it with another orb. But Oresme has no such constraints since for him the construction does not represent an actual planetary model. So the planet (C) can move as needed – in this case, with just the rotational direction and speed of the deferent (A) that will keep line CD aligned with the line of oscillation.

How well does this interpretation fit with the existing text? Actually, rather well, all things considered. Turning to figure 7.11, let us go through the various features as presented by Oresme:

1 A is the deferent, which "carries" (*deferre*) the epicycle (B); the planet (C) is moved by the epicycle. According to most standard medieval accounts, and presumably this idea is what Oresme intends by referring to the conceptualization of the astrologers, the epicycle is embedded in the deferent and the planet is embedded in the epicycle, as shown.
2 A radius (CD) of the planet would in general not be perpendicular to line BC in this construction; however, it would be perpendicular at the quadratures, as noted by Kren. As mentioned above, the alternative given by Droppers and followed by Goddu (see figure 7.10) does not fit the stipulation that D remain on the line of oscillation.
3 The directions of the motions (A eastward, B westward, and C eastward) is consistent with Ṭūsī's model.
4 Oresme emphasizes that BC is a radius of constant length, which probably indicates that he is aware that this stipulation is part of the proof for the Ṭūsī-couple. For this model to work so that point C remains on a straight line, Oresme needs to make B rotate twice as fast as A (or in his terms, point B will descend due to A, while C will ascend with twice the speed due to B). However, he seems to imply that the deferent and epicycle rotate at the same speed (or descend and ascend in equal amounts). Unless he has some other sense for "ascend" and "descend," Oresme does not seem to be in control of this rather critical part of the model.
5 If one accepts my emended translation, Oresme does understand that the planet will need to rotate in the direction opposite that of the epicycle. Again, we are not provided with any amounts, but it seems that Oresme is conceiving of D_0 being displaced to D_1 by the "ascending" motion of B, which would then need to be countered by the descending motion of the planet (see figure 7.2). The flow of the argument is then clear: he begins by "proving" that C will oscillate on a straight line and follows with his "proof" that D will follow



7.11 Oresme's physicalized rectilinear version of the Ṭūsī-couple.

suit and stay on the straight line by means of the additional motion of the planet.

What conclusions can we reach? On the one hand, Oresme is evidently aware of what we may call Naṣīr al-Dīn's physicalized Ṭūsī-couple as presented in the *Tadhkira.* But Oresme makes no claim to have invented this model on his own; and given his apparent lack of understanding of the necessity of having the epicycle move at twice the speed of the deferent, it would be implausible in the extreme to assume that he reinvented this model. On the other hand, the three-sphere version that Oresme presents, as a deferent-epicycle-planet construction, is not to be found explicitly in Ṭūsī or other Islamic sources of which I am aware; thus it seems likely that Oresme or an intermediary had adapted the model for this philosophical discourse. Finally, we should note that there is an echo of the use of the Ṭūsī-couple for the *quies media* debate that we first encountered with Shīrāzī. Oresme states, "By the imagination, it is possible that rectilinear motion be eternal, with the exception that in the point of reflection the movable would not be said to be moved nor at rest."[43]

JOSEPH IBN NAḤMIAS

In his *The Light of the World*, Joseph ibn Naḥmias, a Spanish Jew living in Toledo around 1400, used a double-circle device in his astronomical models that is mathematically equivalent to Ṭūsī's curvilinear version from his *Tadhkira* but in its truncated, two-sphere version. He also incorporates it into his recension of *Light of the World*. Note that despite living in the Christian part of the Iberian Peninsula, Ibn Naḥmias wrote *Light of the World* in Judeo-Arabic (Arabic in Hebrew script), although the recension is in Hebrew. In chapter 8 of the present volume, Robert Morrison details Ibn Naḥmias's use of the Ṭūsī-couple and also discusses the vexed question of its possible transmission to Ibn Naḥmias and other Jewish scholars.[44] I shall return to this question below.

GEORG PEURBACH

From an extensive mathematical analysis of the 1510 and 1512 annual ephemerides of Johannes Angelus, Jerzy Dobrzycki and Richard Kremer have concluded that they were based upon modifications of the *Alfonsine Tables*, these modifications consisting of mechanisms meant to produce harmonic motion that were somehow added to the standard Ptolemaic models.[45] Because Angelus seems to indicate that these were based upon a new table of planetary equations due to Georg Peurbach (d. 1461), Dobrzycki and Kremer speculate that the underlying models used by Peurbach incorporated one of the Marāgha models, perhaps the Ṭūsī-couple or the mathematically equivalent epicycle/epicyclet of ʿAlāʾ al-Dīn ibn al-Shāṭir. Aiton has also raised the possibility that Peurbach in his *Theoricae novae planetarum* may be referring to Ibn al-Haytham's Eudoxan-couple when he states, "On account of these inclinations and slants of the epicycles, some assume that small orbs have the epicycles within them, and that the same things happen to their motion."[46] Although speculative, these authors' conclusions do point to the possibility that European astronomers in the late fifteenth and early sixteenth centuries, other than Copernicus, used and adapted devices that we normally associate with Islamic astronomy. This is an important point that we will revisit when we discuss some of the objections that have been raised to astronomical transmission from Islam to Latin Europe.

JOHANN WERNER

In his *De motu octavae sphaerae*, Johann Werner (1468–1522) uses a two-equal-circle device to deal with the issue of variable precession, or trepidation. According to Dobrzycki and Kremer, "Werner allotted the trepidational motion of 'Thabit's' [Thābit ibn Qurra's] and Peurbach's models to the solstitial points of two concentric spheres. Two circles of trepidation, of equal radii and centred on the solstitial points of the next higher sphere, rotate in opposite directions so that trepidational variations in longitude do not introduce shifts in the obliquity of the ecliptic. Werner thus managed to generate linear harmonic motion by the uniform motions of two circles."[47] This model sounds a lot like the two-equal-circle version of the Ṭūsī-couple, but we need to be cautious. Werner does not use a 2:1 ratio for the motions of the two circles, and in his earlier analysis, Dobrzycki specifically states that this is not the Ṭūsī-couple as used, for example, by Copernicus.[48] However, since Werner's intention is to generate a linear oscillation to avoid shifts in the obliquity, one can indeed see a connection. However, further research would be needed to establish a relationship between Werner's use and earlier uses of the Ṭūsī-couple.[49]

GIOVANNI BATTISTA AMICO

Giovanni Battista Amico (d. 1538) used the three-sphere curvilinear version as described in the *Tadhkira* in his *De motibus corporum coelestium*, published in 1536;[50] in other words, he used the version with three spheres, two producing the curvilinear oscillation on the surface of a sphere and the third functioning as a counteracting sphere so that only the curvilinear oscillation of its pole is transmitted to the next lower sphere.[51] According to Mario Di Bono, "It is of particular interest that in the 1537 [revised] edition of his work Amico is aware that on the surface of a sphere the demonstration does not function as it should; but since the inclination of the axes is not great, he considers the error negligible."[52]

GIROLAMO FRACASTORO

Girolamo Fracastoro in his *Homocentrica*, published in 1538, refers to a device for producing rectilinear motion but does not incorporate it into his astronomy. The description and diagram make it clear that he is referring to the two-equal-circle version.[53]

NICHOLAS COPERNICUS

Noel Swerdlow and Otto Neugebauer succinctly summarize Copernicus's use of the various devices invented by Ṭūsī: "In *De revolutionibus* he uses the form of Ṭūsī's device with inclined axes for the inequality of the precession and the variation of the obliquity of the ecliptic, and in both the *Commentariolus* and *De revolutionibus* he uses it for the oscillation of the orbital planes in the latitude theory. In the *Commentariolus* he uses the form with parallel axes for the variation of the radius of Mercury's orbit, and by implication does the same in *De revolutionibus* although without giving a description of the mechanism."[54]

However, we will need to examine the situation a bit more closely.[55] Let us take *De revolutionibus orbium coelestium* first. In fact, the device put forth and the proof given in book 3, chapter 4, for variable precession and the variation of the obliquity are, *pace* Swerdlow and Neugebauer, for the two-equal-circle version, not for the two- or three-sphere curvilinear version (i.e., "Ṭūsī's device with inclined axes"). And in all other cases in which he uses it in *De revolutionibus* (for Mercury's longitude model in book 5, chapter 25, and for the latitude theory in book 6, chapter 2), Copernicus refers the reader back to book 3, chapter 4. We may then conclude that Copernicus wishes to use the two-equal-circle version exclusively in *De revolutionibus*. As Swerdlow and Neugebauer note, Copernicus's statement that he will be using chords rather than arcs (as necessitated by the use of the rectilinear rather than curvilinear version) is reasonable since the deviation from a curvilinear version is relatively minor.[56] But it does raise questions about the kind of modelling Copernicus uses in *De revolutionibus*, in contrast to the *Commentariolus*. In the *Commentariolus*, it is the truncated two-sphere curvilinear version that is used for the latitude models,[57] and it is the physicalized rectilinear version that is used to vary the radius of Mercury's orbit but in a truncated, two-sphere version without the enclosing/maintaining sphere.[58] The conclusion seems to be that Copernicus was attempting to provide actual spherical models for the two versions of the Ṭūsī-couple he uses in the *Commentariolus* but that he cut a corner or two by not dealing with the disruption of the contained orb, which, after all, is why Ṭūsī (and Amico) have their maintaining (or withstanding) spheres. In *De revolutionibus*, Copernicus abandons any pretense of full physical models for his Ṭūsī-couples and instead relies only on the two-equal-circle version, which, as we have seen, is a mathematical, not a physical, model.[59]

THE TRANSMISSION SKEPTICS[60]

Although difficult to gauge in a precise way, impressionistically it seems that a majority of historians of early astronomy have accepted, to a lesser or greater degree, the influence of late-Islamic astronomy on early modern astronomers, particularly Copernicus. This acceptance is perhaps most explicitly set forth by Swerdlow and Neugebauer: "The question therefore is not whether, but when, where, and in what form he [Copernicus] learned of Marāgha theory."[61]

Nevertheless, there have been a number of skeptics who have raised various issues that are worth exploring. In 1973, for example, Ivan Nikolayevich Veselovsky called attention to what is the converse of the Ṭūsī-couple, namely a device for producing a circular motion from straight-line motions, which was set forth by Proclus in his commentary on book 1 of Euclid's *Elements*.[62] Copernicus refers to just this passage in Proclus when he uses the Ṭūsī-couple for his Mercury model.[63] But there are numerous problems with attributing Copernicus's source to Proclus rather than Ṭūsī. In the first place, Proclus, as mentioned, is setting forth a way to produce circular motion from linear motions, which is the opposite of what the Ṭūsī-couple does.[64] Second, as noted by Swerdlow, Edward Rosen, and originally Leopold Prowe, Copernicus only received a copy of Proclus's book in 1539 as a gift from Georg Joachim Rheticus, which is many years after first using the couple in the *Commentariolus*.[65] Di Bono proposes, as a way to save Veselovsky's suggestion, the possibility that Copernicus may have seen a copy of the original Greek while in Italy, this idea gaining some plausibility because it was part of the library that Cardinal Basilios Bessarion had bequeathed to the Venetian Senate.[66] But again this suggestion raises numerous other problems, namely that Copernicus is then required to have read, or to have had read to him, a Greek manuscript and that he was then inspired by an obscure passage in it talking about something only vaguely related to a device that, as we have seen, was certainly available from other sources. And Copernicus himself does not even get the reference to Proclus correct; he has Proclus claiming that "a straight line can also be produced by multiple motions,"[67] but as we have seen, Proclus refers to the production of a circle, not a straight line. And in any event, Copernicus himself mentions "some people" who refer to the Ṭūsī device as producing "motion along the width of a circle,"[68] which indicates that the device is used by others (and almost certainly is not of his own making) and that Proclus is not one of these people since Proclus does not, and could not, refer to the motion as such.

Di Bono is certainly the most thoughtful skeptic, and his skepticism is nuanced and tempered. As an alternative to an Islamic connection, which he does not reject out of hand, he proposes that Copernicus, with the same aim of resolving the issues of irregular motion in Ptolemy's models, basically came up with the same set of devices and planetary models.[69] "As to Amico and Fracastoro, there is no need to imagine a source or a specific author from whom both authors derived the same device, nor to imagine a strict interdependence between them."[70] What is ironic here is that Di Bono begins his article insisting on examining the differences between the various models and their uses among the different astronomers he examines. As he puts it, "Moreover, as in this case even marginal similarities or differences may be of relevance, it is of the utmost importance not to cause such differences to disappear in the reduction to the mathematical formalism in use today."[71] But in the conclusion of the article, where he needs to reduce these differences in order to argue against transmission and for multiple rediscovery (or parallel development), he falls back upon Neugebauer's point that "[t]he mathematical logic of these methods is such that the purely historical problem of the contact or transmission, as opposed to independent discovery, becomes a rather minor one."[72] But the problem with this position is that the differences on which Di Bono is so insistent earlier in his article here fade to irrelevance since the "internal logic" supersedes any attempt to understand the historical developments involved; each actor is foreordained to come up with the "same" solution, even when these solutions are not the same. Yet another problem with Di Bono's position is that none of his European actors has left any hint that they developed the basic devices on their own. And where we do have a discussion of sources, namely in *De revolutionibus*, Copernicus on the one hand makes a somewhat irrelevant gesture toward Proclus – which has all the hallmarks of a humanist need to pad his text with a classical reference – and on the other hand, as we have seen, refers to others who have used the device. So Di Bono's contention that "the reciprocation device … could equally well have derived from an independent reflection [by Copernicus] on these same problems" seems to be undermined by what evidence is at hand.

A more recent skeptic is André Goddu, who agrees with Di Bono's skepticism about an Islamic influence but is equally skeptical about Di Bono's suggestion of a Paduan source. Instead, he proposes Oresme as the ultimate source of the reciprocating device in Europe, someone Di Bono does not mention in his own, wide-ranging article. As we have seen, Oresme does indeed describe a reciprocation device, but it is rather different from the one Goddu envisions.[73] Be that as it may, Goddu proposes the following: "The path to Copernicus would have proceeded from Oresme to Hesse, Julmann, and Sandivogius, and from them to Peurbach, Brudzewo, and Regiomontanus." But in making such a proposal, Goddu has confused, or conflated, two totally different models. Henry of Hesse (ca. 1325–97), a certain magister Julmann (alive in 1377), Albert of Brudzewo (1445–95), and perhaps Peurbach are not describing ("using" would be misleading here) some version or other of the Ṭūsī-couple but rather something like Ibn al-Haytham's Eudoxan-couple (see above). As for Sandivogius of Czechel (fl. 1430), what is being put forth is an additional epicycle for the Moon that would counter the original epicycle's motion; without this additional epicycle, we should be able to see both faces of the Moon, something that is not observed.[74] Goddu seems to be depending mainly on José Luis Mancha for his information on Hesse, Julmann, Peurbach, and Brudzewo, but

Mancha makes it very clear that what they are dealing with is Ibn al-Haytham's Eudoxan-couple, not the Ṭūsī-couple.[75] Thus when Goddu seeks to make Oresme the source for Hesse and subsequent writers, he is making a fundamental mistake, namely having something that is likely to have been some sort of Ṭūsī device be the source for a totally different type of model. Oresme was seeking to produce rectilinear motion from circular motion, whereas most of the other authors Goddu deals with (excepting Copernicus, of course) are simply reporting a way to physicalize the small circle motion of Ptolemy's latitude theory or are using the same device for the oscillation of the lunar apogee due to the Moon's prosneusis point.[76] That Goddu further claims that an adaptation by Copernicus of the Eudoxan model that Brudzewo describes is equivalent to the wholesale incorporation of Ibn al-Shāṭir's models into the *Commentariolus* is, to say the least, bizarre in the extreme.[77]

EMPIRICAL EVIDENCE FOR TRANSMISSION

Both Di Bono and Goddu ask for more evidence for transmission before passing judgment. This is a fair comment, and in what follows I present some of the evidence that has been discovered over the past twenty-five years or so.[78] I divide this evidence up into different pathways that transmission did take or could have taken.

The Byzantine Route

As mentioned above, it is now clear that the Ṭūsī-couple first made its way into another cultural context through Byzantine intermediaries, first and foremost Gregory Chioniades, who travelled to Tabrīz around 1295 and studied with a certain Shams Bukharos, whom we can now identify as Shams al-Din al-Wābkanawī.[79] That this transmission occurred through an adapted translation from Persian into Greek raises some interesting issues of intercultural exchange. Was this translation a result of the fact that the language of trade between Byzantium and Iran was mainly in Persian? If so, Chioniades may have had an easier time finding someone to teach him Persian than Arabic. And indeed, most of the Islamic astronomical works that found their way into Greek seem to have been from Persian sources.[80] This Persian bias may help us to understand why an ostensibly out-of-date treatise, such as Ṭūsī's Persian *Muʿīniyya* and its appendix, the *Ḥall*, which, as we have seen, contained the first versions of Ṭūsī's rectilinear couple and lunar model, were provided and taught to Chioniades rather than the mature versions found in Ṭūsī's later *Tadhkira*, which was in Arabic. But there could be other reasons. One of

Chioniades's successors, George Chrysococces (fl. 1350), relates the following story, which was told to him by his teacher Manuel:

in a short while he [i.e., Chioniades] was taught by the Persians, having both consorted with the King, and met with consideration from him. Then he desired to study astronomical matters, but found that they were not taught. For it was the rule with the Persians that all subjects were available to those who wished to study, except astronomy, which was for Persians only. He searched for the cause, which was that a certain ancient opinion prevailed among them, concerning the mathematical sciences, namely, that their king will be overthrown by the Romans, after consulting the practice of astronomy, whose foundation would first be taken from the Persians. He was at a loss as to how he might come to share this wonderful thing. In spite of being wearied, and having much served the Persian king, he had scarcely achieved his objective; when, by Royal command, the teachers were gathered. Soon Chioniades shone in Persia, and was thought worthy of the King's honor. Having gathered many treasures, and organized many subordinates, he again reached Trebizond, with his many books on the subject of astronomy. He translated these by his own lights, making a noteworthy effort.[81]

This passage of course reminds us, if we need reminding, that intercultural transmission at the time did take considerable effort and was not always a straightforward process. But it also teaches us that transmission was indeed possible. In this case, the transmission of the couple and models based on it is clear since they occur in Chioniades's *Schemata*. Less clear are the circumstances under which the *Schemata* itself was further transmitted. And did other knowledge contained in the *Muʿīniyya* and the *Ḥall*, but not contained in the *Schemata*, also get transmitted? An example of this latter case would be Ibn al-Haytham's Eudoxan-couple, which, as mentioned, was presented in a separate chapter in the *Ḥall* by Ṭūsī. Ibn al-Haytham's work itself is not extant, and the presentation in the *Tadhkira* is much more succinct than what is in the *Ḥall*. So a transmission of the Eudoxan-couple via Chioniades would provide an important link taking us to Henry of Hesse and beyond.

The *Schemata* is currently witnessed by three manuscripts: two in the Vatican (Vat. Gr. 211, fols 106v–115r [text], fols 115r–121r [diagrams]; and Vat. Gr. 1058, fols 316r–321r) and one at the Biblioteca Medicea Laurenziana in Florence (Laur. 28, 17, fols 169r–178r).[82] The Vatican manuscripts have diagrams, whereas the Florence one does not.[83] In Vaticanus Graecus 211, one diagram represents the mathematical rectilinear version of the Ṭūsī-couple (fol. 116r), and another represents al-Ṭūsī's lunar model from the *Ḥall* (fol. 117r), the one with six rather than seven orbs. The Florence manuscript was copied in 1323 according to the colophon on folio 222v, but it is not clear when the manuscript arrived in Italy. Vaticanus Graecus 211 was copied in the early fourteenth century and was recorded in the Vatican inventory of 1475; Vaticanus Graecus 1058 was copied in the middle of the fifteenth century and was perhaps in the Vatican inventory of 1475 but certainly, according to David Pingree, in the inventory made around 1510.[84] These sources provide us with evidence that the work, with diagrams, was available in Italy as early as 1475; on this basis, Swerdlow and Neugebauer favour this Italian transmission route for the Ṭūsī-couple to Copernicus, who studied and travelled in Italy between 1496 and 1503 (mainly Bologna, Padua, and Rome).[85] It may be significant that Copernicus spent part of the Jubilee year 1500 in Rome, perhaps to do an apprenticeship at the Papal Curia, which would have given him access to the *Schemata*.

The Spanish Connection

Relations between the two main branches of Christendom were fraught, and it seems likely that one of the reasons the twelfth-century translation movement brought Greek classics into Latin via Arabic translations, rather than directly from the Greek, was that it was easier to obtain Arabic versions of Greek texts in Spain than it was to obtain Greek manuscripts from Byzantium. Thus we must be cautious before assuming that Byzantine astronomy would have made its way westward before the fifteenth century. But there is another route that could have brought the new astronomy of thirteenth-century Iran to the Latin West. There is considerable historical evidence of ongoing diplomatic activity between the Spanish court of Alfonso X of Castile and the Mongol Īlkhānid rulers of Iran. The late Mercè Comes wrote an important article on the subject and noted a number of cases of similar astronomical theories and instruments appearing in both Christian Spain and Iran during the thirteenth century.[86] But perhaps the most striking example of a scientific theory from Īlkhānid Iran appearing in Europe is the attempted proof of Euclid's parallels postulate, produced in the important Tabrīz scientific milieu of the 1290s, which pops up in the work of Levi ben Gerson (Gersonides) in southern France, probably shortly after 1328, according to Tony Lévy, who made this important identification.[87] This is the proof found in the *Commentary on Euclid's Elements* published at the Medici Press in Rome in 1594 and incorrectly attributed to Ṭūsī; the proof was later discussed by the Italian mathematician Giovanni Saccheri.[88] If something as complicated as this proof of the parallels postulate could travel from Iran to Avignon in twenty-five years or so, the

Ṭūsī-couple, already translated into Greek, could presumably make it to France as well and be available for Nicole Oresme. As mentioned above, Ibn al-Haytham's Eudoxan-couple is a bit more difficult to trace, but the fact that Chioniades would have no doubt encountered it in his studies of the *Ḥall* provides another plausible vehicle of transmission, as does whatever means brought pseudo-Ṭūsī's parallels proof westward.

The Jewish Link

As we see with Gersonides, perhaps the most important agents of transmission from Islam to Christendom were Jewish scientists and mathematicians. Recent work by Tzvi Langermann and Robert Morrison has been ground-breaking in shedding light on a host of characters involved in this transmission. In addition to bringing Avner de Burgos's proof of the Ṭūsī-couple to our attention, Langermann has shown that in fifteenth-century Italy, Mordecai Finzi knew the *Meyashsher ʿaqov* of Avner de Burgos, in which, as we have seen, Avner proved that one could produce continuous straight-line oscillation by means of a Ṭūsī-couple. According to Langermann, Finzi clearly knew of the *Meyashsher ʿaqov*, as indicated by his copying of the interesting conchoid construction found in Avner's text.[89] It seems reasonable to assume, as Langermann does, that Finzi knew the other parts of the *Meyashsher ʿaqov*, including the Ṭūsī-couple proof. Furthermore, Finzi had extensive contacts with Christian scholars, as he notes in several places in his works and translations.[90] Thus here we have a Jewish scholar who most likely knew of the Ṭūsī-couple in contact with north Italian mathematicians a generation or so before Copernicus would be in the neighbourhood.

In chapter 8 of the present volume, Robert Morrison discusses another avenue through which the Ṭūsī-couple may have become known to Italian scholars via Jewish intermediaries. In addition to summarizing recent work on Ibn Naḥmias, Morrison traces the interesting career of a certain Moses ben Judah Galeano (Mūsā Jālīnūs). Galeano had ties to Crete and the Ottoman court of Sultan Bāyazīd II (r. 1481–1512) and also travelled to the Veneto region around 1500. Most interesting is that Galeano knew of the work of Ibn al-Shāṭir, whose models are so instrumental in the *Commentariolus*. Galeano also knew the writings of Ibn Naḥmias, whose models incorporated the Ṭūsī-couple and are quite similar to ones we find in Johannes Regiomontanus and Giovanni Battista Amico. Thus we have another route by which the Ṭūsī-couple may well have found its way to Italy in the late fifteenth century.

Manuscripts Galore

Something often overlooked in discussions of the transmission of devices like the Ṭūsī-couple (both within Islamic realms and interculturally) is that we are not dealing with a limited number of texts and manuscript witnesses. If we confine ourselves to Ṭūsī's works that present one or more versions of his couple and to works derived from them (i.e., commentaries, supercommentaries, and closely related works) that were composed before 1543 CE, we find at least fourteen texts represented by hundreds of witnesses (see table 7.3).[91] This table does not include philosophical, theological, and encyclopaedic works, or Quran commentaries, in which the couple is mentioned or discussed.[92]

I do not claim that the almost 400 manuscript witnesses enumerated in table 7.3 would have somehow been available to early modern European astronomers. Indeed, some of these manuscript witnesses were copied well after the sixteenth century. Nevertheless, a fair number of them currently reside in Istanbul and other former Ottoman lands, including those in eastern Europe. Although most of the Islamic manuscripts currently in European libraries were collected after 1500,[93] there were presumably Islamic scientific manuscripts that were available in various parts of Europe previous to that date.[94]

The last bit of empirical evidence for transmission is indirect but highly suggestive. Recently, it has come to light that the critical proposition that Swerdlow has claimed was used by Copernicus to transform the epicyclic models of Mercury and Venus into eccentric models, which is found in Regiomontanus's *Epitome of the Almagest*, was put forth earlier in the fifteenth century by ʿAlī Qushjī of Samarqand.[95] Although it is not known how Qushjī's treatise came to be known by Regiomontanus – which seems much more likely to me than independent rediscovery of the proposition[96] – a likely candidate is Cardinal Basilios Bessarion (d. 1472), the Greek prelate who almost became the Roman pope. Bessarion travelled to Vienna in 1460, where he met both Peurbach and Regiomontanus. That Qushjī's proposition occurs in the *Epitome*, which was completed around 1462, suggests that Bessarion is the intermediary. This idea gains further plausibility since he was originally from Trebizond and spent considerable time in Constantinople before its fall to the Ottomans in 1453. Consequently, he could have easily been in contact with Islamic scholars, who were in various centres in Anatolia, including Bursa, the home of Qāḍīzāde al-Rūmī, one of Qushjī's teachers and associates in Samarqand. Qushjī himself later came to Constantinople, in 1472, probably at the behest of Sultan

Table 7.3 Manuscript witnesses to the Ṭūsī-couple

Author	*Title*	*Date of composition*	*Manuscript witnesses*
Naṣīr al-Dīn al-Ṭūsī	*Ḥall-i mushkilāt-i Muʿīniyya* (Persian)	1245 CE	19
Naṣīr al-Dīn al-Ṭūsī	*Taḥrīr al-Majisṭī* (Arabic)	1247 CE	93
Naṣīr al-Dīn al-Ṭūsī	*Al-Tadhkira fī ʿilm al-hayʾa* (Arabic)	1261 CE	72
Quṭb al-Dīn al-Shīrāzī	*Nihāyat al-idrāk fī dirāyat al-aflāk* (Arabic)	1281 CE	37
Quṭb al-Dīn al-Shīrāzī	*Ikhtiyārāt-i Muẓaffarī* (Persian)	1282 CE	10
Quṭb al-Dīn al-Shīrāzī	*Al-Tuḥfa al-shāhiyya fī al-hayʾa* (Arabic)	1285 CE	49
Quṭb al-Dīn al-Shīrāzī	*Faʿalta fa-lā talum* (supercommentary on the *Tadhkira*; Arabic)	ca. 1300 CE	3
Ḥasan ibn Muḥammad ibn al-Ḥusayn Niẓām al-Dīn al-Aʿraj al-Nīsābūrī	*Tawḍīḥ al-Tadhkira* (Arabic)	1311 CE	53
ʿUmar b. Daʾūd al-Fārisī	*Takmīl al-Tadhkira* (commentary on the *Tadhkira*; Arabic)	1312 CE	1
Jalāl al-Dīn Faḍl Allāh al-ʿUbaydī	*Bayān al-Tadhkira wa-tibyān al-tabṣira* (commentary on the *Tadhkira*; Arabic)	1328 CE	1
al-Sayyid al-Sharīf ʿAlī ibn Muḥammad ibn ʿAlī al-Ḥusaynī al-Jurjānī	*Sharḥ al-Tadhkira al-Naṣīriyya* (commentary on the *Tadhkira*; Arabic)	1409 CE	51
Fatḥ Allāh al-Shīrwānī	*Sharḥ al-Tadhkira* (commentary on the *Tadhkira*; Arabic)	1475 CE	2
ʿAbd al-ʿAlī ibn Muḥammad ibn al-Ḥusayn al-Bīrjandī	*Sharḥ al-Tadhkira* (commentary on the *Tadhkira*; Arabic)	1507 CE	1
Shams al-Dīn Muḥammad ibn Aḥmad al-Khafrī	*Al-Takmila fī sharḥ al-Tadhkira* (supercommentary on the *Tadhkira*; Arabic)	1525 CE	2

Mehmed II. Admittedly, Bessarion was hardly the person to acknowledge the scientific achievements of Muslims; after all, he came to Vienna as a legate of Pope Pius II (Aeneas Silvius Piccolomini) in order to seek support for a crusade against the Turks that would recapture Constantinople.[97] But his intense interest in reviving the Greek scientific heritage in Europe would have overcome any hesitancy he may have had about bringing cutting-edge Islamic scientific thought to his young acolytes.

CONCLUSION

The possible transmission of the Ṭūsī-couple to Europe confronts us with a number of both practical and theoretical considerations. On a practical level, we need to trace the origins and development of the device and its appearance afterward over several centuries. As we have seen, it is critical that we be clear which version of the couple we are talking about and how it is being used. We also have needed to chart the various pathways by which the couple was, or could have been, transmitted.

On a theoretical level, we need to deal with several implicit issues in what has gone before by way of conclusion. The first we can call the issue of the hermetically sealed civilization. Many comments on intercultural transmission have somehow assumed that after the twelfth-century translation movement from Arabic into Latin, the gates of transmission became closed, and European Christendom and Islam were sealed off from one another until the colonial period brought them back into contact, this time with the relative civilizational – but more importantly, military – superiority reversed. This assumption has had a number of historiographical consequences. Much of premodern European history, both medieval and early modern, is written from a Eurocentric point of view. In many cases, this bias may be justified since, like politics, much of history is local.[98] However, this is not the case with all history. And here the insistence on an exclusively European-focused narrative can cause considerable distortion of the historical record. For example, discussing the development of trigonometry without bringing in the Indian introduction of the sine and, based on this innovation, the subsequent development of the other trigonometric functions and identities in Islamic mathematics leaves out an essential part of the story.[99] In the case of much postclassical (i.e., post-1200 CE) Islamic science, the assumption is made that Europeans would have had little contact because of cultural and linguistic differences. But this assumption by European intellectual historians is belied by the extensive evidence of political, economic, and cultural exchanges between various late-Islamic regimes and European realms.[100] European travellers did go to various regions of the Islamic world before the modern period, and there are certainly examples of Islamicate travellers in Europe.[101] But more to the point, it is also clear that Islamic scientific theories and objects did travel to Europe, as we have seen, through contacts such as those between Spain and Īlkhānid Iran, through Jewish intermediaries, and through Byzantine scholars and émigrés.

The above-mentioned research by Langermann and Morrison, as well as by İhsan Fazlıoğlu and other historians of the Ottoman period, points to something often overlooked, namely the important role of the Ottoman courts of Mehmed II, who was the conqueror of Constantinople, and of his son and successor Bāyazīd II in promoting scientific and philosophical study, which included providing patronage for Christian and Jewish, as well as Muslim, scholars. Many of these Christian and Jewish scholars travelled readily between the Ottoman and Christian realms.[102] And it should not be forgotten that, at the time, the Ottomans were a European power, with vast domains in eastern and central Europe, and had been such since the fourteenth century.

But there may have been more direct contact. Here, one needs to confront the myth of a linguistically impoverished Europe; even scholars sympathetic to transmission such as Swerdlow and Neugebauer feel compelled to remark that "[a] direct transmission of the Arabic [texts containing the non-Ptolemaic models used by Copernicus] is *of course* extremely unlikely."[103] But why "of course"? Some Europeans did know Arabic (how else could the twelfth-century translation movement have taken place?), and there is research showing that knowledge of Arabic was not unknown during the Renaissance.[104] At this point in our knowledge, we can only speculate that European astronomers either learned Arabic or worked with translators who did know enough to explain the non-Ptolemaic models of Ṭūsī, Ibn al-Shāṭir, and others. But it seems to me equally speculative to assume they did not. After all, Arabic is not all that esoteric – it is closely related to Hebrew, which was certainly studied by numerous European Christian scholars – and there were dictionaries and grammars available. And perhaps most importantly, why would someone seek to start from scratch when it was certainly known in the fifteenth and sixteenth centuries that Islamic astronomers still had much to teach their European counterparts?[105] But more generally from a historiographical point of view, it seems odd that so many European historians of the medieval and early modern periods have written histories that make their subjects seem isolated, devoid of curiosity, and impervious to outside influences.[106]

The next theoretical point to pursue is the question of "how much evidence is enough." It is a commonplace in the history of science to trace intercultural transmission through the reappearance of numbers, objects, models, propositions, and even ideas that we can locate in an earlier source. In fact, one might consider it our most precise way to document intercultural transmission. The gold standard in our field is arguably Hipparchus of Nicaea's value for the mean synodic month (reported by Ptolemy), namely 29;31,50,8,20 days (sexagesimal). Once Franz Kugler demonstrated in the 1890s that this value came from what is now known as Babylonian System B, the argument for Greek knowledge and use of Babylonian astronomy (at least its parameters) became incontestable. The same is also true of the fact that Hipparchus, despite what is reported by Ptolemy, did not make a recalculation using new observations. But why can we reach these conclusions? The answer is obvious. Would anyone seriously argue that two identical values to the fourth sexagesimal place is a coincidence? According to Di Bono and Goddu, the appearance of Ṭūsī's couple, Muʾayyad al-Dīn al-ʿUrḍī's lemma, Ibn al-Shāṭir's models, and so on in the work of Copernicus is not sufficient to prove transmission. But what makes this case different from the case of Hipparchus's value for the mean synodic month? The case made by Di Bono, and echoed by Goddu, is that somehow the "internal logic" is such that anyone confronting the problem of Ptolemy's irregular motions would come up with the same solutions.[107] But Di Bono makes it clear that his criteria for accepting transmission are so high that even a "high number of coincidences between Copernican and Arab models" is insufficient since it then "becomes very difficult to explain how such a quantity of models and information, which Copernicus would derive from Arab sources, has left no trace – apart from Ṭūsī's device – in the works of the other western astronomers of the time."[108] This argument is a curious one; given the tenuous nature of transmission, an insistence on multiple examples would render many cases moot, even one as strong as the transmission of the Babylonian synodic month.

Let us now turn to the issue of "internal logic" and parallel development. In fact, what we have in Islam and in the Latin West represent two very different historical developments. The criticism of Ptolemy on various fronts, including observational ones, begins quite early in Islam;[109] and certainly by the time of Ibn al-Haytham (d. ca. 1040), we have sustained criticisms of the irregularities in Ptolemy's planetary models.[110] By the thirteenth century, we see a number of attempts to deal with these criticisms by using alternative models that employ devices consisting of uniformly rotating spheres, those of Ṭūsī, ʿUrḍī, and Shīrāzī being the most prominent; the proposal of alternative models continues for several centuries in Islam. It is important to emphasize that this historical development is sustained and traceable; Ṭūsī and his successors knew of earlier criticisms and alternative models, and they explicitly sought to build upon their predecessors. This long-term historical process is precisely what is missing in the accounts of those who advocate a "parallel development" in the Latin West. As we have seen, the Ṭūsī-couple appears there in fits and starts; we do not find a sustained discussion of the "equant problem" before Copernicus,[111] and we certainly do not see a sustained, historically coherent development of alternative models. Here, the evolution of Ṭūsī's various couples is instructive; from the initial discussion of the problem and announcement of a solution until he put forth his "final" versions, Ṭūsī took twenty-five years, during which he presented various models that he would later revise. But in the Latin case, there is no one about whom a story exists that can account for the rationale and development – indeed, the "logic" – for one or more versions of the Ṭūsī-couple. As we have seen, they just somehow appear. And no one after Ṭūsī claims to have independently discovered any of the versions of the couple, either in the Islamic world or in the Latin West.[112]

In their different scenarios, both Di Bono and Goddu have attempted to provide alternative "stories," but these are deeply flawed. Di Bono seeks to find the source for Copernicus's use of the Ṭūsī-couple in the Paduan Aristotelian-Averroist critiques of Ptolemy. But the problem here is that such critiques generally led to quite different homocentric modelling based on a variety of techniques that are quite distinct from those of Ṭūsī and his successors. In particular, Di Bono makes no attempt to explain how Copernicus could have used the epicycle-only modelling of Ibn al-Shāṭir if he had been so influenced by astronomers and natural philosophers adamantly opposed to epicycles and eccentrics.[113] In the case of an astronomer who did come out of that tradition and who did use one version of the Ṭūsī-couple, namely Amico, we have an astronomy quite different from that of Copernicus. As for Goddu's attempt to locate Copernicus's discovery and use of the Ṭūsī-couple in the Aristotelian environment of Cracow, here we have what amounts to a misunderstanding. As we have seen, Brudzewo, whom Goddu wishes to make the immediate predecessor for Copernicus's use of the couple, is in fact using Ibn al-Haytham's Eudoxan-couple. It is true that Brudzewo does mention it in the context of the motion of the epicyclic apogee due to the Moon's prosneusis point, which, interestingly enough, is one of the examples Ṭūsī uses to explain the need for the curvilinear version of his couple.[114] But again, neither Brudzewo nor anyone else adduced by Goddu proposes a Ṭūsī-couple device for dealing with the problem.[115] In sum, both Di Bono and Goddu depend on tenuous connections that would have us believe that their actors can move from model to model without clear agency or plausible historical context. And it is this stark contrast – between, on the one hand, Islamic astronomy's well-developed historical context for dealing with the irregular motions of Ptolemaic astronomy and, on the other hand, the Latin West's ad-hoc, episodic, and decontextualized "parallel" attempts – that in my opinion provides us with the most compelling argument for transmission of non-Ptolemaic models such as the Ṭūsī-couple from Islam to Europe before the sixteenth century.[116]

Given what we know, it seems that one possible scenario is that Copernicus was indeed influenced by Brudzewo's comments to pursue the problem of the Moon's epicyclic apogee. And perhaps he realized at some point that what was needed was a curvilinear oscillation on the epicycle's circumference, as Ṭūsī had before him. Then, while in Italy, he somehow encountered, through one of the routes outlined above, one or more versions of the Ṭūsī-couple that he would subsequently use. But it is also clear that he was not overly interested in the complexities of the models, which would account for his use of the apocopated two-sphere (as opposed to the full three-sphere) version in the *Commentariolus*. And by the time of composing *De revolutionibus*, he was willing to make a further simplification by using Ṭūsī's two-circle version even though it did not fulfil the need either for a full-scale physical model for rectilinear motion or for a version that could produce true curvilinear oscillation.

In summary, it seems that, as put so perceptively by Dobrzycki and Kremer, "We may be looking for a means of transmission both more fragmentary and widespread than a single treatise."[117] And certainly by the time Copernicus wrote *De revolutionibus*, one version or another of the Ṭūsī-couple would have been available in the Latin West for several centuries; in other words, it had become commonplace. So perhaps Copernicus, the man from Toruń, felt no need to worry about its origins, whether in Tūn or elsewhere, and could, without qualms, cross out the redundant remark in his holograph that "some people call this the 'motion along the width of a circle.'"[118]

CHAPTER SEVEN

1 F.J. Ragep, "Copernicus." This point is made even more forcefully in my "Ibn al-Shāṭir and Copernicus: The Uppsala Notes Revisited," where I maintain that there is a stronger connection between ʿAlāʾ al-Dīn ibn al-Shāṭir (fourteenth century) and Copernicus's models and heliocentrism than has been previously claimed.

2 Here, we need to acknowledge Mario Di Bono, who, in a valuable article, insists on distinguishing the various versions of the Ṭūsī-couple. Di Bono, "Copernicus, Amico, Fracastoro." Di Bono is building on the earlier work of Noel Swerdlow, especially his "Aristotelian Planetary Theory in the Renaissance."

3 On *Risālah-i Muʿīniyya*, see F.J. Ragep, "Persian Context," and *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 65–6. See also Kennedy, "Two Persian Astronomical Treatises."

4 The relevant parts of the Persian text discussed in this paragraph, along with translation, are in F.J. Ragep, "Persian Context," 123–5.

5 F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 208.

6 The name "*Dhayl-i Muʿīniyya*" is found in the only dated manuscript of Ṭūsī's text, namely Tashkent, Uzbekistan, Al-Biruni Institute of Oriental Studies, MS 8990, fols 1a, 33a, 33b.

7 Ṭūsī, *Dhayl-i Muʿīniyya*, Tashkent, Al-Biruni Institute of Oriental Studies, MS 8990, fol. 46a (original foliation):

تمت الرسالة اتفق فراغ المصنف رفع الله مراتبه في معارج القدس من تأليفه أوائل جمادى
الآخرة سنة ٦٤٣ هجرية بمقام بلدة تون بالبستان المعروف بباغ بركه

8 On Tūn as one of the residences of the local Ismāʿīlī rulers, see Daftary, "Dāʿī," 592, col. 1.

9 Ṭūsī, *Ḥall-i mushkilāt-i Muʿīniyya*, 7:

اما استقامت حركت مركز تدوير از محيط مايل بر سمت مركزش وبعد از آن رجوع او هم بر
آن سمت تا بمحيط رسيدن بى آنكه خرقى و التيامى لازم آيد يا خللى باستدارت حركات راه
يابذ بر آن وجه تواند بود كه ياد كنيم.

"The rectilinear motion of the center of the epicycle away from the circumference of the inclined [orb] in the direction of its centre and then its return on that same line until it reaches the circumference – without there being any tearing and mending, or any rupture in the circular motions – can be in the way we shall mention."

10 See F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 208–23, vol. 2, 448–56.

11 The relevant passages from *Risālah-i Muʿīniyya*, book 2, chs 5, 6, 8, with English translation, can be found in F.J. Ragep, "Persian Context," 123–5.

12 For details and an edition and translation of the relevant chapter from the *Ḥall*, see F.J. Ragep, "Ibn al-Haytham and Eudoxus."

13 This chronology contradicts George Saliba's contention, followed by Di Bono and others, that the two-equal-circle version in *Taḥrīr al-Majisṭī* was the first occurrence of any version of the Ṭūsī-couple. But clearly the new dating of the *Ḥall* should put to rest this earlier proposal. Compare Saliba, "Role of the *Almagest* Commentaries."

14 This comment corresponds to the *Almagest*, book 13, ch. 2; Ptolemy, *Ptolemy's Almagest*, 599-601.

15 Ṭūsī, *Taḥrīr al-Majisṭī*, fols 201a–202a:

أقول هذا كلام خارج من الصناعة {201ب} غير مقنع في هذا الموضع فإنّ من الواجب على صاحب هذه الصناعة أن يضع دوائر وأجراماً ذوات حركات متشابهة على نضد وترتيب يتركّب من جميعها هذه الحركات المحسوسة المختلفة ثم إنّ كون هذه الحركات على محيط الدوائر الصغار المذكورة كما تقتضي خروج أقطار التداوير عن سطوح الخارجة المراكز في العرض شمالاً وجنوباً كذلك تقتضي خروجها عن محاذاة مركز البروج أو موازاة أقطار على سطح البروج بأعيانها في الطول إقبالاً وإدباراً بقدر تلك العروض بأعيانها وذلك مخالف للوجود ولا يمكن أن يقال إنّ ذلك التفاوت محسوس في العرض وغير محسوس في الطول لتساويهما في المقدار والبعد من مركز البروج فإن جعل قطر الدائرة الصغيرة بقدر جميع العرض في إحدى الجهتين وتوهّم أنّ مركزها يتحرّك على محيط دائرة أخرى مساوية لها مركزها في سطح الخارج المركز بقدر نصف حركة طرف قطر التدوير على محيط الدائرة الأولى وإلى خلاف جهتها حدث الانتقال إلى الشمال والجنوب بقدر العرض من غير أن يحدث في الطول تقدّم وتأخّر وليكن لبيانه اب قطعة من الخارج و جد من دائرة العرض المارّة بطرف قطر التدوير وقد تقاطعا على ه و هز هم جميع العرض في الجهتين وهح نصفه في إحديهما ونرسم على ح ببعد هح دائرة هز وعلى ه ببعد حه دائرة ح ط ك ل ونتوهّم طرف قطر التدوير على نقطة ز متحرّكاً على دائرة هز في جهة ج إلى ب ومركز ح متحرّكاً على دائرة ح ط ك ل في جهة ج إلى ا نصف تلك الحركة فظاهر أنّه إذا قطع ح ربعاً وانتهى إلى ط قطع ز نصفاً وانتهى إلى ه ثم إذا قطع ح ربعاً آخر وانتهى إلى ك قطع ز نصفاً آخر وانتهى {202آ} إلى م وإذا قطع ح ربعاً ثالثاً وانتهى إلى ل قطع ز نصفاً آخر وانتهى ثانياً إلى ه وإذا تمّ ح دورة عاد ز إلى موضعه الأول فهو دائماً يتردّد فيما بين زم على خطّ ج د غير مائل عنه إلى جهتي اب فهذا بيان هذا الوجه ولكن يلزم عليه أن يكون زمان كون القطر في الشمال مساوياً لزمان كونه في الجنوب والوجود بخلاف ذلك وأمّا القول بحركته على محيط دائرة حول نقطة غير مركزها على ما ذكر بطلميوس فمحتاج إلى نظر يحقّقه على ما مرّ ونعود إلى الكتاب

16 The unequal times in the *Almagest* occur because this motion in latitude is coordinated with the irregular motion, brought about by the equant, of the epicycle centre on the deferent. F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 2, 455.

17 Ibid., vol. 1, 216–21.

18 For a fuller account of the curvilinear version, see ibid., vol. 2, 453–6. It should be noted that the curvilinear version does not in fact produce motion on a great circle arc; there is a small discrepancy resulting in a narrow, pinched figure-eight motion. This was noticed by at least one commentator on the *Tadhkira*, Shams al-Dīn al-Khafrī (fl. 1525 CE). But the maximum deviation from a great circle arc, which occurs when using the curvilinear version to deal with the problem of the Moon's prosneusis, is only 0.214°, which is about 0.87 per cent. Ibid., vol. 2, 455n55, 455n56. For an illustration of the deviation, see figure C26, in ibid., vol. 1, 361.

19 The purpose of Ibn al-Haytham's proposal was to provide a physical basis for the circular path of the epicycle apex A in Ptolemy's latitude theory; as far as is known, he was not concerned with the resultant motion of S, which traces a "hippopede" in Eudoxus of Cnidus's theory (as shown in figure 7.9). It is interesting that Regiomontanus's version of this device resulted in a curvilinear oscillation of S along a great circle arc, something that had been proposed earlier by Joseph ibn Naḥmias. For details, see Morrison, chapter 8, this volume, especially figure 8.3. For the reason that the Eudoxan-couple should produce a hippopede, not a curvilinear oscillation, see Neugebauer, "On the 'Hippopede' of Eudoxus."

20 F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 220–3.

21 Shīrāzī, *Al-Tuḥfa al-shāhiyya*, fol. 34a:

ويمكن أن يجعل هذا دليلاً على امتناع السكون بين حركتين صاعدة وهابطة على سمت قطر من أقطار الأرض

22 Langermann, "Quies Media," provides an excellent summary of the *quies media* question and discusses a number of Islamic thinkers, including Shīrāzī, who dealt with it.

23 The restriction of the date will exclude a discussion of the translation into Sanskrit of part of ʿAbd al-ʿAlī al-Birjandī's (d. 1525–26) commentary on Ṭūsī's *Tadhkira*, the part containing the presentation of the Ṭūsī-couple. On this translation, see Kusuba and Pingree, *Arabic Astronomy in Sanskrit.*

24 On the use of *aṣl* to translate the Greek term *hypothesis*, see Morrison, chapter 8, this volume, note 10.

25 These works are currently extant in three codices, two in the Vatican and one in the Biblioteca Medicea Laurenziana in Florence.

26 Edition and translation in Paschos and Sotiroudis, *Schemata of the Stars*, 26–53.

27 This resemblance was first recognized by Otto Neugebauer, who reproduced diagrams from Vatican Gr. 211, fol. 116r, in his *History*, part 3, 1456.
28 F.J. Ragep, "New Light on Shams."
29 This use of the earlier works can most easily be established from the list of star names found in Paschos and Sotiroudis, *Schemata of the Stars*, 30–7. For a discussion and the evidence, see F.J. Ragep, "New Light on Shams," 239, 241–2.
30 It was reported that there was great reluctance by the Persians to teach astronomy to a Byzantine because of a legend that doing so would lead to the former's demise. F.J. Ragep, "New Light on Shams," 231–2.
31 Paschos and Sotiroudis, *Schemata of the Stars*, 42–5. On the *Ḥall*, see above and F.J. Ragep, "New Light on Shams," 242.
32 David Pingree states that Vatican Gr. 211 is listed in the Vatican inventory of 1475 and that Vatican Gr. 1058 is listed in the inventory made around 1510 but may well have been in the collection earlier. Pingree, *Astronomical Works*, vol. 1, 23, 25.
33 See above and F.J. Ragep, "Ibn al-Haytham and Eudoxus."
34 Langermann, "Medieval Hebrew Texts," 34.
35 Droppers, "Questiones de Spera," 462–4; Kren, "Rolling Device."
36 Goddu, *Copernicus*, 481, 484.
37 The parts of Kren's translation in "Rolling Device," 490, that have been changed are in italics; my suggested revisions are in brackets immediately following. Droppers, "Questiones de Spera," 285, 287, 289, also provides a translation, somewhat more literal than Kren's, that I have also taken into account.
38 Here is Kren's Latin version in "Rolling Device," 491n3 (compare Droppers, "Questiones de Spera," 284, 286, 288):
Circa hanc questionem, pono 3 pulcras conclusiones. Prima est quod possibile est quod aliquis planeta secundum quodlibet sui moveatur in perpetuum motu recto composito ex pluribus motibus circularibus, ita quod iste motus proveniat a pluribus intelligentiis quarum quelibet intenderet movere motu circulari nec frustratur ab intentione sua.

Pro cuius probatione, suponatur per ymaginationem, sicut faciunt astrologi, quod A sit circulus deferens alicuius planete, vel centrum eius, et sit B circulus epiciclus eiusdem planete, et C sit corpus planete vel centrum eius; hoc habeo pro eodem. Et ymaginetur linea BC, exiens de centro epicicli ad centrum planete, et CD sit linea in planeta supra quam alia cadat perpendiculariter. Moveatur etiam A circulus supra centrum ad orientem, et B ad occidentem, et C planeta supra centrum suum volvatur ad orientem. Cum ergo linea BC semper sit equalis, quia est semidyameter, ponatur quod quantum B descendit ad motum deferentis, tantum C punctus ascendat per motum epicicli. Ex quo patet intuenti quod punctus C per aliquod certum tempus movebitur super lineam rectam. Tunc ponatur ultra quod perifora qua punctus B ascenderet motu suo tantum descendat motu planete. Et patet iterum quod punctus D continue movebit in eadem linea. Ergo totum corpus planete movebitur motu recto usque ad aliquem terminum, et iterum poterit reverti in motu consimilli.

39 Figure 7.10 is from Droppers, "Questiones de Spera," 287, reproduced by Goddu, *Copernicus*, 481. Note that despite the use of *corpus* in referring to the planet, Goddu insists that "there is no indication that Oresme was directly concerned with the physical characteristics of the bodies or the mechanisms" (481). This interpretation of Oresme may be why both Droppers and Goddu seem capable of ignoring Oresme's clear statement that it is the "entire body of the planet" that moves in a straight line. We should also note here that the title of this *questio* is "Whether any heavenly body (*corpus celeste*) is moved circularly."

40 Kren, "Rolling Device," 492.

41 In contrast, Goddu, *Copernicus*, 480, finds Kren's reconstruction "implausible," but this assessment seems to be based on the grounds that Ṭūsī's construction requires two circles whereas Oresme's requires three. He apparently is unaware of Ṭūsī's physicalization of his geometrical device and his explicit use of three spheres in the *Tadhkira*. F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 200–1, 350–1, vol. 2, 435–7. Kren is able to see this use of three spheres even though she was depending, as mentioned, on an earlier French translation of this passage in which Ṭūsī describes how to physicalize his device. Kren, "Rolling Device," 493n8. Goddu had access to a new translation and discussion of this passage in the *Tadhkira*, so his claim that Ṭūsī does not have a three-sphere model is odd.

42 What follows is a modified version of what is described in the *Tadhkira*, book 2, ch. 11, para. 4. F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 200–1; see also fig. C13, in ibid., vol. 1, 351. For a discussion of this passage, see ibid., vol. 2, 435–8.

43 Droppers, "Questiones de Spera," 291.

44 See Morrison, chapter 8, this volume.

45 Dobrzycki and Kremer, "Peurbach and Marāgha."

46 Aiton, "Peurbach's *Theoricae novae planetarum*," 36, 36n118.

47 Dobrzycki and Kremer, "Peurbach and Marāgha," 233n53.

48 Dobrzycki, "Theory of Precession," 51.

49 In addition to the previous reference, see also Dobrzycki, "Astronomical Aspects," 122; and Räumer, "Johannes Werners Abhandlung."

50 On Amico, see Swerdlow, "Aristotelian Planetary Theory"; and Di Bono, *Le sfere omocentriche*.

51 Ṭūsī refers to this third as "the enclosing sphere" (*al-kura al-muḥīṭa*). F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 220–1. Amico calls it a "withstanding (*obsistens*) sphere." Swerdlow, "Aristotelian Planetary Theory," 41.

52 Di Bono, "Copernicus, Amico, Fracastoro," 141. Ṭūsī does not mention this problem, but it is mentioned by at least one commentator on the *Tadhkira*. See F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 2, 455; and note 18 above.
53 Di Bono, "Copernicus, Amico, Fracastoro," 143–4.
54 Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 47.
55 Here, we follow the lead of Di Bono, "Copernicus, Amico, Fracastoro," esp. 138–41.
56 Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 136.
57 Swerdlow, "Derivation and First Draft," 483, 497.
58 Ibid., 503.
59 See Di Bono, "Copernicus, Amico, Fracastoro," 140–1.
60 I do not deal here with all the "transmission skeptics" but focus only on the ones who have dealt specifically, using original ideas, with the transmission of the Ṭūsī-couple to medieval and early modern Europe. In particular, I do not consider here the derivative arguments of Viktor Blåsjö in "A Critique of the Arguments for Maragha Influence," 185–6, or those of Michel-Pierre Lerner and Alain-Philippe Segonds in their translation of Copernicus, *De revolutionibus (Des révolutions)*, vol. 1, 551–7. Likewise, I do not deal with André Goddu's response to criticisms by Peter Barker and Matjaž Vesel of his handling of the issue of transmission of Islamic astronomy to Copernicus since it is not germane to my own criticisms contained here. Goddu, "Response to Peter Barker," 251–4.
61 Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 47. The emphatic way that this acceptance of late-Islamic influence is stated is most likely due more to Swerdlow than to Neugebauer, for see the latter's earlier remark that "[t]he mathematical logic of these methods is such that the purely historical problem of contact or transmission, as opposed to independent discovery, becomes a rather minor one." Neugebauer, "On the Planetary Theory," 90. Nonetheless, in a personal communication, Swerdlow assured me that Neugebauer completely endorsed the phrasing in their *Mathematical Astronomy in Copernicus's De Revolutionibus*. Edward S. Kennedy and Willy Hartner also entertain little doubt that Copernicus's work was heavily influenced by his Islamic predecessors. Kennedy, "Late Medieval Planetary Theory"; Hartner, "Copernicus, the Man, the Work." A recent rejoinder to André Goddu's skepticism regarding an Islamic influence on Copernicus has been made by Barker and Vesel, "Goddu's Copernicus," 327–32. Goddu's answer, in which he distances himself from an outright rejection of Islamic influence, can be found in his "Response to Peter Barker," 251–4.
62 Veselovsky, "Copernicus and Naṣīr al-Dīn al-Ṭūsī."
63 Copernicus, *De revolutionibus*, book 5, ch. 25.
64 F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 2, 430.

65 Copernicus, *On the Revolutions*, 369, 429 (commentary by Rosen); Swerdlow, "Copernicus's Four Models," 146n5, 155n8; Prowe, *Nicolaus Coppernicus*, vol. 1, part 2, 407, cited by Rosen in Copernicus, *On the Revolutions*, 369.
66 Di Bono, "Copernicus, Amico, Fracastoro," 146.
67 Copernicus, *On the Revolutions*, 279.
68 Ibid., 126 (in book 3, ch. 4, where it was crossed out in the autograph, and in book 3, ch. 5, where it was left in).
69 This idea is also the main thrust of Blåsjö, "Critique of the Arguments."
70 Di Bono, "Copernicus, Amico, Fracastoro," 149.
71 Ibid., 133.
72 Ibid., 149 (referring to Neugebauer's statement quoted in note 61 above).
73 Note again that Goddu dismisses out of hand Kren's mostly correct reconstruction.
74 Grażyna Rosińska claims that Brudzewo owes his two-sphere model for the Moon to Sandivogius, but this is far from clear. Rosińska, "Naṣīr al-Dīn al-Ṭūsī?" Sandivogius seems to be proposing one additional orb (not two) for the Moon and for an entirely different purpose, namely to keep its single face oriented toward the observer.
75 Mancha, "Ibn al-Haytham's Homocentric Epicycles."
76 This conclusion, as part of a longer study on Brudzewo, is also reached by Barker, "Albert of Brudzewo's *Little Commentary*," 137–9. Peter Barker seems unaware of José Luis Mancha's earlier work.
77 Goddu, *Copernicus*, 157: "Experts have exaggerated the supposed identity between Copernicus's and al-Shatir's models and the Tusi couple. Di Bono explains the similarities plausibly as matters of notation and convention. Di Bono also shows that Copernicus's use of the models required an adaptation, and, we may add, if he was capable of adapting geometrical solutions, then why not the solution in Albert's [i.e., Brudzewo's] treatise? The question should be reconsidered." One hardly knows where to begin. First, Di Bono does not deal with Ibn al-Shāṭir's models. Second, the adaptation about which Di Bono is speaking (i.e., the two-equal-sphere model) already occurred with Ṭūsī, as we have seen. Third, for Goddu to think that Copernicus could have simply adapted Brudzewo's cryptic and ultimately unrelated remarks to come up with Ibn al-Shāṭir's models in the *Commentariolus*, one must assume that Goddu has never examined those models.
78 It should be noted that some of this evidence would have been available to Di Bono and even more to Goddu, whose book was published in 2010. It is unfortunate that the presumed lack of transmission that Di Bono and Goddu point to does seem to be at work in the present when we consider how slowly the work of scholars working on Islamic science seems to get transmitted to their colleagues working on the Latin West. For example, Goddu, who is mainly concerned with Copernicus's relation to the Aristotelian tradition, completely ignores the possible transmission from

Islamic sources of a number of Copernican ideas related to natural philosophy, such as the motion of the Earth, the assertion of a non-Aristotelian astronomical physics, and the heliocentric transformation itself. Summarized in F.J. Ragep, "Copernicus."

79 F.J. Ragep, "New Light on Shams," 243–5.

80 Pingree, *Astronomical Works*, 18. But there are certainly examples of Arabic works going into Greek. See Mavroudi, *Byzantine Book*; Touwaide, "Arabic Urology in Byzantium"; and Touwaide, "Arabic Medicine." Joseph Leichter believes that Chioniades may have learned or improved his Arabic at some point. Leichter, "Zīj as-Sanjarī," 11–12.

81 Mercier, "Greek 'Persian Syntaxis,'" 35–6, reproduced in Leichter, "Zīj as-Sanjarī," 3.

82 Information on the manuscripts is from Pingree, *Astronomical Works*, 23–8.

83 Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 48n9.

84 Pingree, *Astronomical Works*, 25.

85 An excellent summary of what is known of Copernicus's life can be found in Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 3–32.

86 Comes, "Possible Scientific Exchange." Note also that Tzvi Langermann alludes to the possibility of a link between Alfonso's court and Muḥyī al-Dīn al-Maghribī, who was of Andalusian origin but spent most of his career in Syria and Iran. Langermann, "Medieval Hebrew Texts," 35.

87 Lévy, "Gersonide, commentateur d'Euclide," 90–1, 100–15.

88 Heath, trans., *Thirteen Books*, vol. 1, 208–12. See also Jones, "Medici Oriental Press."

89 Langermann, "Medieval Hebrew Texts," 34–5. Finzi's notebook into which he copied the construction is currently preserved at the Bodleian Library in Oxford.

90 For example, Finzi states that he made a "translation with the help of a non-Jew here in the city of Mantua," and in another context he states, "I saw them in the Toledan Tables in the possession of a certain Christian." Langermann, "Scientific Writings," 26, 41.

91 These numbers are based upon the Islamic Scientific Manuscripts Initiative (ISMI) database, which is being developed collaboratively by the Institute of Islamic Studies at McGill University and the Max Planck Institute for the History of Science in Berlin. This is most definitely a conservative estimate of witnesses since the number of extant manuscripts in this database will surely increase as other libraries and private collections come to be catalogued and examined.

92 For example, in the encyclopaedic work entitled *Unmūdhaj al-ʿulūm* by Muḥammad Shāh al-Fanārī (d. 1435–36 CE), the author includes a discussion of the latitude problems of Mercury and Venus, as well as Ṭūsī's solution for them. F.J. Ragep, "Astronomy in the Fanārī-Circle," 168–9, 176.

93 George Saliba has done some interesting work on Islamic scientific manuscripts in Europe, but his examples are after 1500. Saliba, "Arabic Science," 154, 159. He points to an early copy of the *Tadhkira* (Vatican MS ar. 319), which was brought to Rome in 1623 as part of the Palatine collection, one of the spoils of the Thirty Years' War that was offered by Maximilian I of Bavaria to Pope Gregory XV. Ibid., 159–62. But it was certainly in central Europe by the mid-sixteenth century, where it was used and perhaps annotated by Jakob Christmann (1554–1613), professor of Hebrew and Arabic at the University of Heidelberg. Levi Della Vida, *Ricerche Sulla Formazione*, 329ff., esp. 332. See also Swerdlow, "Recovery of the Exact Sciences."

94 An example would be the treatise by ʿAlī Qushjī discussed in the next paragraph. Other possibilities include manuscripts held by the *Biblioteca* Medicea *Laurenziana* in Florence, such as a copy of Quṭb al-Dīn al-Shīrāzī's *Nihāyat al-idrāk* (MS Orientali 110) and two copies of his *Al-Tuḥfa al-shāhiyya fī al-hayʾa* (MS Orientali 116c; and MS Orientali 215). In addition to Ṭūsī's models, Shīrāzī in these two works deals with models of Muʾayyad al-Dīn al-ʿUrḍī as well as his own contributions to planetary theory. Unfortunately, we do not know at present when these manuscripts first appeared in Italy.

95 F.J. Ragep, "ʿAlī Qushjī and Regiomontanus." The diagrams found in the 1496 Venice printing of Regiomontanus's *Epitome* and in the manuscripts of Qushjī's treatise are quite similar.

96 Independent rediscovery now seems even less likely, given that Regiomontanus not only does not claim ownership of the proposition but also incorrectly attributes it to Ptolemy. See Shank, chapter 4, this volume.

97 Bisaha, chapter 2, this volume, discusses Bessarion's attitudes and his relationship to European humanist scholars.

98 This point is emphasized in Sabra, "Situating Arabic Science."

99 For an elaboration, see F.J. Ragep, "Review of *The Beginnings*." A more global approach is taken by Van Brummelen, *Mathematics of the Heavens*.

100 See, for example, Dursteler, *Venetians in Constantinople*. Bisaha, chapter 2, this volume, also discusses some of the complex issues involving cross-cultural transmission during this period.

101 Leo Africanus comes to mind.

102 Such travel has been noted in the case of Moses ben Judah Galeano.

103 Swerdlow and Neugebauer, *Mathematical Astronomy*, part 1, 48, emphasis added.

104 See, for example, Dannenfeldt, "Renaissance Humanists"; and Saliba, "Arabic Science."

105 This was even the case in the early seventeenth century. Feingold, "Decline and Fall."

106 Although things are changing, it is disheartening to note that Robert Westman in his recent book *The Copernican Question*, a tome of 681

double-columned pages, devotes precisely one short, off-handed endnote to the "Maragha school" (531n136). Ṭūsī and the Ṭūsī-couple are completely absent; Jews and Byzantines fare little better.

107 Di Bono, "Copernicus, Amico, Fracastoro," 149: "In conclusion, we note that this same question of transmission may be reduced in significance, in that from a mathematical point of view – as Neugebauer has already noted – it is the internal logic of the methods used that leads the Arabs and Copernicus to such similar results."

108 Ibid., 153–4n77.

109 F.J. Ragep, "Islamic Reactions."

110 These criticisms include, but certainly are not limited to, the equant. F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 48–51.

111 This is not to say that the equant as an issue was unknown in the Latin West; but perhaps with the limited exception of Henry of Hesse, one does not find the sustained criticism of Ptolemy's irregularities that is comparable to Ibn al-Haytham's *Al-Shukūk ʿalā Baṭlamyūs* (Doubts about Ptolemy). This criticism is of course different from criticisms of Ptolemy based upon an Aristotelian-Averroist insistence on a homocentric cosmology. The lack of sustained criticism is surprisingly still the case even in the generation before Copernicus; as Dobrzycki and Kremer put it, "We know of no extant text by Peurbach or Regiomontanus in which the Ptolemaic models are criticized explicitly on the grounds that they violate uniform, circular motion." Dobrzycki and Kremer, "Peurbach and Marāgha," 211n27.

112 Celenza, chapter 1, this volume, emphasizes the very different kind of referencing practice that was followed in the premodern world, where the need to document the source of one's ideas or scientific models was less strongly felt. However, it would be quite unusual for someone who invented as significant a device as the Ṭūsī-couple not to claim it as his own. Bisaha, chapter 2, this volume, provides another reason that early modern European thinkers may have hesitated to credit postclassical Islamic scholars with innovative ideas.

113 In my forthcoming "Ibn al-Shāṭir and Copernicus: The Uppsala Notes Revisited," I speculate that Copernicus's incorrect adaptation of Ibn al-Shāṭir's models in the *Commentariolus* may indicate some influence of an Aristotelian-Averroist insistence on a single centre – in this case, the Sun.

114 F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir*, vol. 1, 208–13.

115 Barker, "Albert of Brudzewo's *Little Commentary*," 137–9, comes to a similar conclusion.

116 This is to repeat a point that I make more generally in F.J. Ragep, "Copernicus."

117 Dobrzycki and Kremer, "Peurbach and Marāgha," 211.

118 See note 68 above.

The Two Versions of the Ṭūsī Couple

F. JAMIL RAGEP

INTRODUCTION

THE ATTEMPT by Naṣīr al-Dīn al-Ṭūsī (1201-1274)[a] to reform the Ptolemaic system has been known in the West (at least in the Modern Period) since the appearance in 1893 of Carra de Vaux' translation of Book II, Chapter Eleven of *Al-Tadhkira fī ʿilm al-hayʾa* (Memoir on the science of astronomy). That Ṭūsī's proposal was not an isolated event but rather one in a series of alternative cosmologies, ones bearing a striking resemblance to that of Copernicus, was first clearly enunciated in an article published by E. S. Kennedy in *Isis* in 1966.[1] Willy Hartner in several articles also pointed to the significance of Ṭūsī's models and their possible connection with Copernicus.

Hartner correctly recognized that the translation and analysis presented by Carra de Vaux suffered from serious defects and sought to remedy these in his study of Ṭūsī's lunar model that appeared in *Physis* in 1969. But in spite of Hartner's customary precision in construing the mathematics of the models, the details of the physical cosmography eluded him. This led him to make the unfortunate remark that "Naṣīr (*sic*) must have considered his model primarily a geometrical construction without caring much about its physical reality,"[2] a statement very wide of the mark. In fact, the aim of virtually every theoretical astronomer in the Arab/Islamic tradition was to provide a physical structure, or *hayʾa*,[3] for the universe in which each of Ptolemy's motions in the *Almagest* would be the result of a uniformly rotating solid body called an orb (*falak*). This process, of course, had been initiated by Ptolemy himself in Book II of his *Planetary hypotheses*. But it had become clear, at least by the time of Ibn al-Haytham (*ca.* 956–*ca.* 1040), that a coherent, physically unobjectionable system could not be obtained simply by assigning a physical mover to each motion inasmuch as Ptolemy had felt compelled, owing in general to the phenomena itself, to resort to motions that violated the principles of uniformity and circularity.[4] By the late medieval period, these violations, sixteen in number and commonly referred to as *ishkālāt* (difficulties), could be found enumerated as follows: (1–6) the irregular motions of the deferents of the moon and planets; (7–11) the irregular mo-

[a] All dates are A.D.

tions, each equivalent to the corresponding motion of the deferent, of the apices of the epicycle diameters of the superior and inferior planets along small circles that produce one component of Ptolemy's latitude theory and (12–13) the analogous motions of the endpoints of the mean epicyclic diameters in the case of the two inferior planets; (14–15) the oscillation of the equators of the deferent orbs of the inferior planets; and (16) the oscillation of the lunar epicycle as a result of the alignment of its diameter with the prosneusis point.[5]

It has become commonplace to refer to these objections to the Ptolemaic system as somehow "philosophical,"[6] a term which is meant to imply, I am afraid, something that is scientifically, or more to the point mathematically, insubstantial. In at least one case, however, that of the motion of the epicyclic apices on small circles, the objection does involve the disruption of Ptolemy's longitude models by his latitude theory, certainly by any criterion a serious flaw in the ability of these models, when taken as a complete system, to give accurate planetary positions. But leaving this aside for the moment, I would maintain that regarding these objections as "philosophical" or "metaphysical" seriously distorts the actual intentions of the medieval astronomers who made them. Their purpose was to build a coherent cosmology in which the results of Ptolemy's mathematical models could be obtained from a physically acceptable cosmology. They themselves would thus see these objections as physical in the sense that they referred to violations of the physical principles that formed the basis for such a cosmology and that were accepted by virtually all astronomers – including Ptolemy. In a work such as *Al-Tadhkira*, one finds these principles explicitly stated to be the absence of a void, the finitude of the heavens, and the doctrine that the celestial bodies or orbs move with a simple motion, namely uniform rotation.[7] That the celestial bodies do not experience change in the manner of bodies in the sublunar region thereby becomes not a metaphysical tenet but rather a consequence of the physical laws, for generation, corruption, expansion, contraction, changes in speed and so forth are a result of rectilinear natural motion. Since celestial orbs are simple bodies rotating uniformly, these changes of the sublunar region are precluded in the heavens.[8]

Of course, one may maintain that the physical principles themselves are based upon metaphysical (or nonphysical) foundations, but this line of reasoning misses the point in a very real sense. Besides the fact that such an argument can be used against any physical system, one should recognize that the medieval Islamic astronomers were themselves moving away, though not always explicitly, from a physics that would necessarily be subsumed under a totalitarian philosophical umbrella. Though the uniform circular motion of the heavens might ultimately have to do with souls and a grand design, it was also something that could be taken as observational fact.[9] It is for this reason, I believe, that Ṭūsī distinguishes the four elements and the celestial bodies, which as simple bodies move with motions acting in a single way, from plants and animals, which do not.[10] That the celestial bodies, plants and animals share the common feature of soul is irrelevant in such a scheme since one is here considering the manner in which the bodies move and not the ultimate cause of that motion. It is on this physical and seemingly unobjectionable basis that one should understand the attempts to reform the Ptolemaic system. For a belief in the self-evident nature of the physical principles goes far in explaining the intensity with which Muslim astronomers sought to reconcile the physical and mathematical aspects of their discipline.

THE LOCATION OF ṬŪSĪ'S NON-PTOLEMAIC MODELS

As one might expect, Naṣīr al-Dīn's new models occur in his *hay'a* works whose purpose, as we have stated, is to set forth a physical structure of the universe in which each of the individual motions of Ptolemy's *Almagest* is brought about by a simple body (in the Aristotelian sense). The best known of these writings is *Al-Tadhkira*, highly regarded in the Middle Ages and subject to no less than 15 commentaries, supercommentaries, and glosses. This work, composed in Arabic, was completed in 1261 at Marāgha during the time Ṭūsī was director of the famous observatory commissioned by the Mongol conquerors of Iran. Although the most extensive and best developed exposition of his alternative system occurs in *Al-Tadhkira*, it was not the first place in which he presented his new models. In a short treatise written in Persian for his Ismāʿīlī patrons, Ṭūsī introduced a geometrical lemma designed to produce a rectilinear oscillation that would serve as the basis for resolving the first six difficulties enumerated above (*i.e.* those dealing with the irregular motion of the epicycle centers of the moon and planets resulting from the nonuniform rotation of the deferents). This work, the *Ḥall-i mushkilāt-i Muʿīniyya* (Solution of the difficulties in the *Muʿīniyya*), was written as a short sequel to his *Risālah-i Muʿīniyya dar hay'a* (The *Muʿīniyya* treatise on astronomy) (also in Persian) upon which *Al-Tadhkira*, composed some 25 years later, was based. Although a precise date cannot at present be assigned to the *Ḥall*, we do know that it must have been written after 1235 (the date of the *Risālah-i Muʿīniyya*) and before 1256 (the year in which the last Ismā ʿīlī stronghold fell to the Mongols). In all likelihood the *Ḥall* was written shortly after the *R.-i Muʿīniyya* in the late 1230s or early 1240s. Thus Ṭūsī, long before his association with the Marāgha observatory, had managed to resolve a number of the *ishkālāt* (difficulties) identified by Ibn al-Haytham.[11]

Left unresolved, however, were those numbered 7–16. Although he reported in the *Ḥall* an attempt by Ibn al-Haytham to deal with the revolution on small circles of the epicycle diameters (*ishkālāt* nos. 7–13), he had no solution of his own to offer.[12] By the time *Al-Tadhkira* was completed, however, he had at least partially resolved the remaining *ishkālāt* by means of a second lemma, one that was again intended to produce a linear oscillation but this time on the surface of a sphere.

We may, by somewhat extending Kennedy's terminology, refer to the two lemmas as the rectilinear and curvilinear versions of the Ṭūsī couple. It is to these and the models based upon them that we now turn our attention.

THE RECTILINEAR VERSION OF THE ṬŪSĪ COUPLE

As Ṭūsī states explicitly in the *Ḥall*, the purpose of the rectilinear version of the Ṭūsī couple is to have on hand a method of varying the distance of the epicycle center from a given point by simply having it oscillate on a straight line.[13] The device itself consists of two circles, one having a diameter half that of the other, with the smaller being internally tangent to the larger (see Figure 1). In addition to these geometrical givens, there are several physical conditions. The two circles move in opposite directions, each with a simple, uniform rotation, and the smaller circle has a rotation twice that of the larger. The result of such a configuration is that a given point will oscillate on a straight line between extrema A and B.[14]

Actually, Ṭūsī does not need two motions to achieve the oscillation of his given point along the diameter of the larger circle; he merely needs to allow the smaller circle to "roll" inside the larger one, which would remain stationary. To see this we will again refer to Figure 1; now, however, instead of both circles rotating in place, circle Z will roll inside circle D. At the starting point, A and E coincide; after the smaller circle has rolled along arc AG, point E will be at a distance GE from the point of tangency G. It is clear that GE = AG; therefore, $\angle GZE = 2\angle GDA$ since the radius of the smaller circle is half that of the larger. Thus mathematically, this rolling is equivalent to having the smaller circle rotate twice as fast as the larger one in the opposite direction. We may also, being anachronistic, find the locus of the point E by noting that the parametric equations of $\vec{DZ} + \vec{ZE}$ are

$$x = s \cos \alpha + s \cos (-\alpha) = 2 s \cos \alpha$$
$$y = s \sin \alpha + s \sin (-\alpha) = 0,$$

which indicate that point E will oscillate on a straight line. (Note that it is unnecessary to make any assumptions about the whereabouts of point E or that $\angle GDE = \angle GDA$.)

This sort of analysis seems to be the basis for calling the Ṭūsī couple a "rolling device."[15] Unfortunately, such an appelation seriously misrepresents

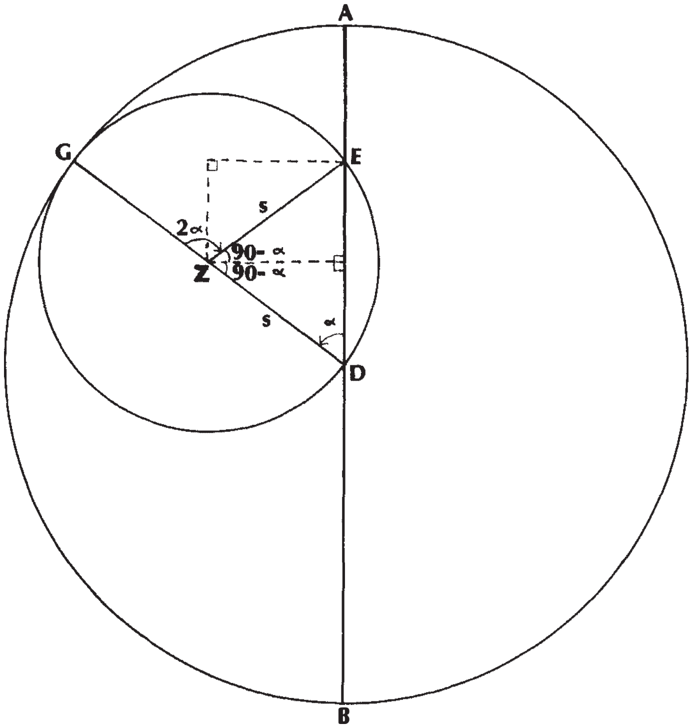


FIGURE 1

Ṭūsī's intentions, not to mention the entire thrust of medieval Islamic theoretical astronomy. The Ṭūsī couple is not simply a mathematical formulation; it is meant to possess a physical reality. Therefore each of the two circles must rotate in place since the rolling of one celestial body inside another is precluded by the absence of any void in the heavens and the stricture against any "tearing or mending."[16]

That Ṭūsī means the couple to have a physical reality becomes strikingly clear in the presentation of his lunar and planetary models.[17] Instead of circles, we now have two spheres, the larger of which rotates with half the speed of the smaller; the given point E has been replaced by a spherical epicycle (see FIGURE 2). The couple itself is composed of the two circles, shown with dotted lines, that would be the resultant paths of the epicycle center if the large and small spheres were to rotate independently of one another.[18] These two paths are referred to by Ṭūsī as equators (*minṭaqa*s) though it is obvious that they do not fit the standard definition.[19] Nevertheless as they are concentric and coplanar with their corresponding equators, we shall refer to them as "inner

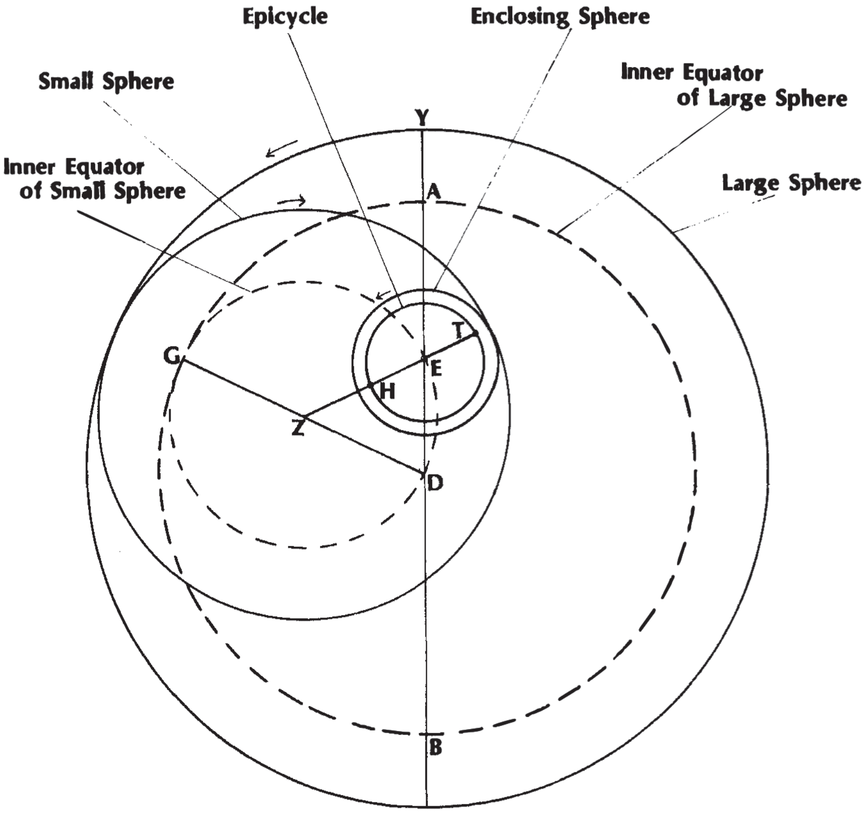


FIGURE 2

equators" which will, one hopes, reduce any potential confusion. We should note here that since the ratio of the radii of the two inner equators is 1:2, that of the outer equators of the two spheres cannot be so but is instead $(s + r) : (2s + r)$, where s is the radius of the smaller equator and r is the radius of the epicycle.[20] (For the moment, we ignore the enclosing sphere surrounding the epicycle.)

As the two spheres rotate, the smaller with twice the speed of the larger, the epicycle center E will descend in a straight line. But as we see in FIGURE 2, the line joining the apex (*dhirwa*) T and the perigee (*ḥaḍīḍ*) H of the epicycle will no longer be coincident with the line of oscillation of the epicycle center E. In order to bring his models into line with Ptolemaic theory, which requires that the apex T be a reference point from which to measure the motion of the planet on the epicycle, Ṭūsī introduces an orb concentric and

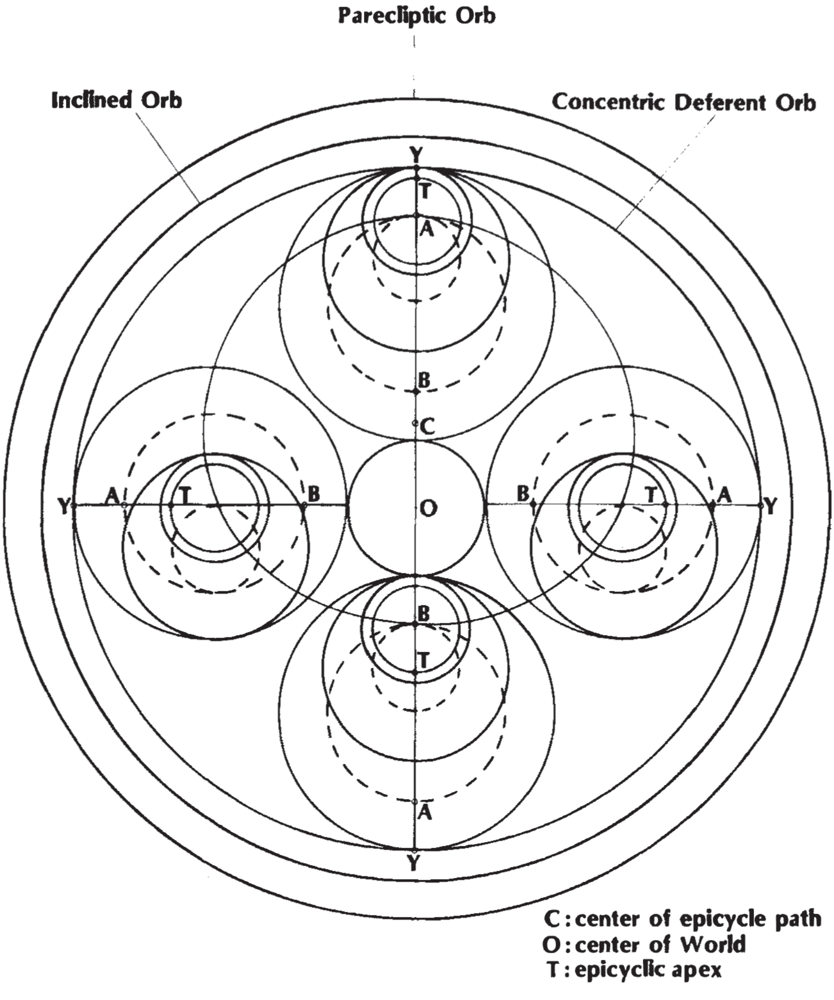


Figure 3. Ṭūsī's lunar model.

coaxial with the epicycle called the enclosing sphere (*al-kurat al-muḥīṭa*). This orb moves with the same speed and in the same direction as the large sphere, which has the effect of keeping diameter TH of the epicycle coincident with line AB at all times.[21]

In the case of the moon, the large sphere (containing the small sphere, the epicycle, and the enclosing sphere) is now embedded within a deferent orb whose center is the Earth.[22] Because the deferent and the large sphere rotate with the same speed (and incidentally in the same direction),[23] the epicycle

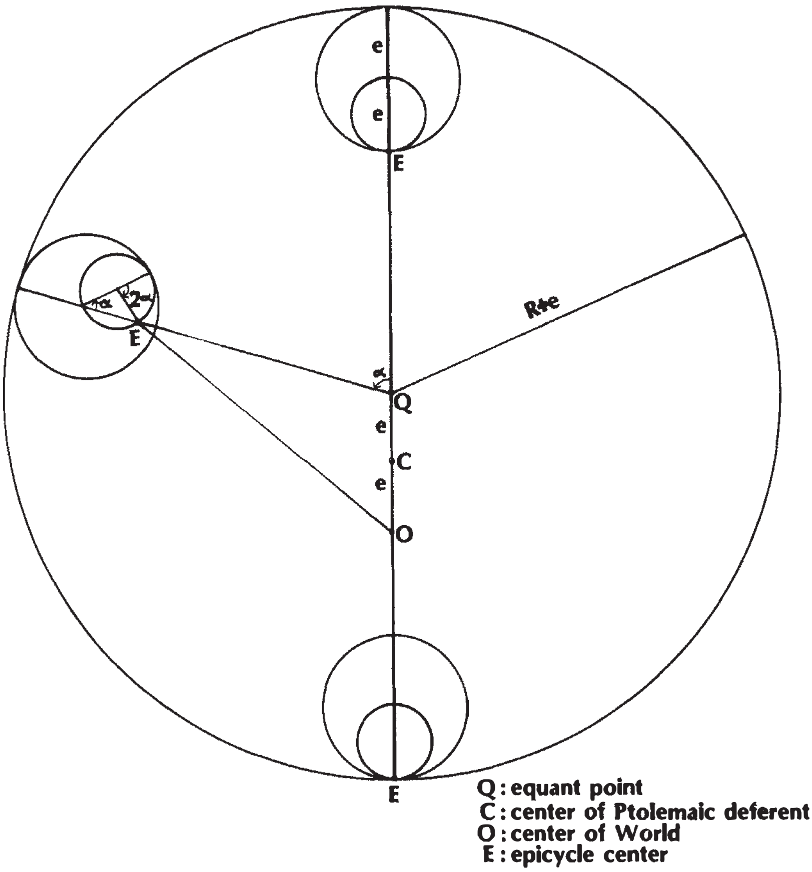


FIGURE 4. Ṭūsī's planetary model.

center will be at perigee after half a rotation of the deferent (see FIGURE 3). But since this should occur at quadrature according to Ptolemaic theory, Ṭūsī must now rotate the deferent in the opposite direction. This is accomplished by enclosing the deferent with the inclined orb (*al-falak al-mā'il*), which shares with the former the same poles and center. The inclined orb is thus the source of the "motion of apogee" of the *Almagest*. Finally the inclined orb is enclosed by the parecliptic orb, which has the same center but poles that are at a distance of 5° from those of the deferent and inclined orbs. Its equator, which is in the plane of the ecliptic, intersects the equator of the inclined orb at two points called the nodes (*ʿuqdatān* or *jawzahar*). The rather slow retrograde motion of the parecliptic accounts for the motion of the nodes (approximately 19-year cycle).

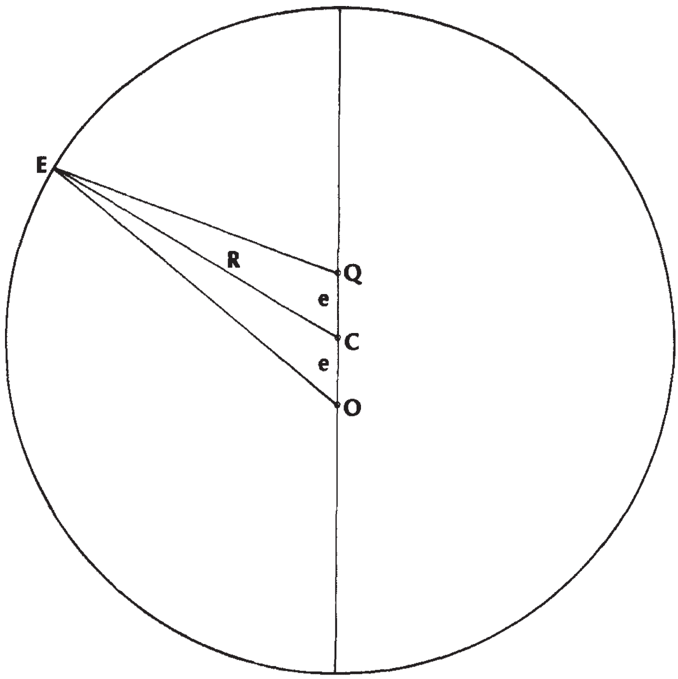


FIGURE 5.

For the planets, excluding Mercury, one has a somewhat different arrangement.[24] The deferent in this case is an eccentric orb, embedded within the parecliptic, whose center Q is that of the equant in the Ptolemaic model, *i.e.* it is at a distance $2e$ from the center of the World O and at a distance e from the center C of the Ptolemaic deferent, where e is the eccentricity (see FIGURE 4; only the "inner equators" are shown in order to simplify the illustration). Embedded within the deferent is the large sphere of the Ṭūsī couple, whose inner equator has a diameter of $2e$ while the inner equator of the small sphere has a diameter e. Now in order for the distance OE from the center of the World to the epicycle center to be $R + e$ at apogee and $R - e$ at perigee so as to conform with the requirements of the Ptolemaic model where R is the radius of the deferent (see FIGURE 5), the epicycle center E at apogee must be at its closest position to Q while at perigee it must be at its farthest distance. It then easily follows that the inner equator of the deferent in this model has a radius of $R + e$ and that the starting position of E must be, in contrast to that of the lunar epicycle center, at the extremum on the line of oscillation that is nearest Q. Thus Ṭūsī will need, as he states, three additional spheres for his model over what is used in the Ptolemaic planetary configuration: the

TABLE 1. Parameters of Ṭūsī's Lunar and Planetary Models[a]

	Moon	Venus	Mars	Jupiter	Saturn
Radius (r) and Motion (γ) of Epicycle	$5^P; 15$ $13^{\circ}4'$/day in the counter-sequence of the signs	$43^P; 10$ 37'/day in the sequence of the signs	$39^P; 30$ 28'/day in the sequence of the signs	$11^P; 30$ 54'/day in the sequence of the signs	$6^P; 30$ 57'/day in the sequence of the signs
Radius (r + ε) of Enclosing Sphere	unspecified	unspecified	unspecified	unspecified	unspecified
Diameter (e = eccentricity) of Inner Equator of Small Sphere	$10^P; 19$	$1^P; 15$	$6^P; 00$	$2^P; 45$	$3^P; 25$
Diameter (2e) of Inner Equator of Large Sphere	$20^P; 38$	$2^P; 30$	$12^P; 00$	$5^P; 30$	$6^P; 50$
Radius (½e + r + ε) and Motion (2α) of Small Sphere	$10^P; 24½ + \varepsilon$ $48^{\circ}46'$/day in the counter-sequence of the signs	$43^P; 47½ + \varepsilon$ $1^{\circ}58'$/day in the counter-sequence of the signs	$42^P; 30 + \varepsilon$ $1^{\circ}2'$/day in the counter-sequence of the signs	$12^P; 52½ + \varepsilon$ 10'/day in the counter-sequence of the signs	$8^P; 12½ + \varepsilon$ 4'/day in the counter-sequence of the signs
Radius (e + r + ε) of Large Sphere	$15^P; 34 + \varepsilon$	$44^P; 25 + \varepsilon$	$45^P; 30 + \varepsilon$	$14^P; 15 + \varepsilon$	$9^P; 55 + \varepsilon$

Radius of Inner Equator of Deferent (R for moon; R + e for planets)	60^P	61^P; 15	66^P	62^P; 45	63^P; 25
Radius of Deferent Orb (R + r + ε for moon; R + e + r + ε for planets)	65^P; 15 + ε	104^P; 25 + ε	105^P; 30 + ε	74^P; 15 + ε	69^P; 55 + ε
Motion (α) of Enclosing Sphere, Large Sphere, and Deferent Orb	24°23'/day in the sequence of the signs	59'/day in the sequence of the signs	31'/day in the sequence of the signs	5'/day in the sequence of the signs	2'/day in the sequence of the signs
Radius (R + r + ε + κ) and Motion of Inclined Orb	65^P; 15 + ε + κ 11°9'/day in the counter-sequence of the signs	NA	NA	NA	NA
Radius (R + r + ε + κ + μ for moon; R + e + r + ε + μ for planets) and Motion of Parecliptic	65^P; 15 + ε + κ + μ 3'+/day in the counter-sequence of the signs	104^P; 25 + ε + μ 1°/70 years in the sequence of the signs	105^P; 30 + ε + μ 1°/70 years in the sequence of the signs	74^P; 15 + ε + μ 1°/70 years in the sequence of the signs	69^P; 55 + ε + μ 1°/70 years in the sequence of the signs

[a] Motion in the sequence/counter-sequence of the signs is determined by the motion of the orb's apogee point; ε, κ, and μ are unspecified quantities.

large and small spheres of his couple and an "enclosing sphere" for the epicycle. This latter, as for the lunar epicycle, is needed to keep the epicyclic apex and perigee aligned with the point about which uniform motion occurs, in this case the equant center Q.

The following table summarizes the parameters of the various orbs and equators and their motions.

As for that most insidious planet Mercury, Naṣīr al-Dīn admits unqualified defeat: "for it is difficult to see how one can make the motion uniform about a point in which the moved object in its motion toward and away from it is composed of multiple motions,"[25] a plaint directed at Mercury's so-called "crank mechanism." He does, though, promise to append a solution to *Al-Tadhkira* if he were ever to find one; as there is no trace of such a work or reference to it in the commentaries, Mercury would seem to have eluded him to the end.[26]

THE DIFFERENCE BETWEEN THE ṬŪSĪ AND THE PTOLEMAIC MODELS

Though Ṭūsī has striven to develop models that will be mathematically identical to those of Ptolemy, he must admit that there are discrepancies. In the Ptolemaic theory, the path of the epicycle center that results solely from the motion of the eccentric deferent (*i.e.* taken in isolation from the motions of the apogee and nodes) is a circle. But as Ṭūsī tells us, the analogous path in his lunar model that is due to the rectilinear oscillation of the epicycle center in combination with the concentric deferent "resembles a circle, but we did not say that it was a circle since it is not a true circle."[27] The proof he offers is quite straightforward (see FIGURE 6). After the concentric deferent has rotated 90°, the epicycle center will have traveled a distance equal to the eccentricity e on its line of oscillation. It will then be at a distance $R - e$ from the center of the World. But its distance from the midpoint between its nearest and farthest distances, which corresponds to the eccentric center of the Ptolemaic model, will clearly be greater than $R - e$, and thus greater than the distance from the center of its path at its farthest and nearest distances. This is the well-known "bulging out" phenomenon of both late medieval Islamic and Copernican planetary theory. (Though we have dealt specifically with the lunar case, the same analysis is equally applicable to the planetary models.)

Ṭūsī is not content merely to indicate that there is a difference; he also wishes to quantify it. For the moon, he notes that the deviation does "not exceed 1/6 of a degree" and that this maximum difference will occur at the octants, *i.e.* when the doubled elongation equals 90° or 270°.[28] As Hartner

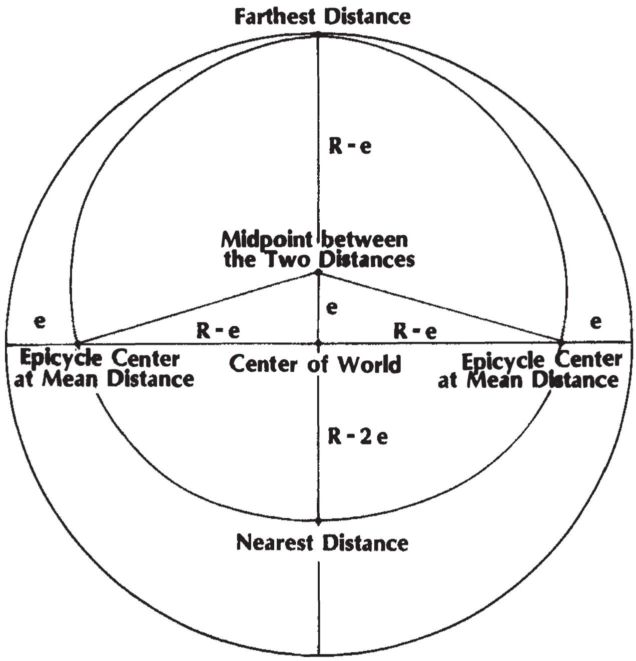


FIGURE 6.

has shown, Ṭūsī is absolutely correct; the maximum difference turns out to be approximately 8 minutes of arc, and does indeed occur at the octants.[29]

In the course of his discussion, Ṭūsī makes the very interesting remark that this maximum difference of 1/6 of a degree at the octants is "an imperceptible amount" (*ghayr maḥsūs*), which gives some idea of what the director of an observatory in the 13th century believed to be the limits of observation. Since Ṭūsī also declares, this time with regard to his planetary models, that "the distances of the epicycle center from the center of the World are the same as resulted from the [Ptolemaic] deferent without there being a difference that might disturb the situation of these planets,"[30] it would seem appropriate to test this claim as well.

From FIGURE 4, it is clear that the Earth-epicycle center distance in Ṭūsī's planetary models will be given by

$$(OE)^2_T = (QE)^2 + (2e)^2 - 2(QE)\,(2e)\cos(180 - \alpha).$$

Now since one may show without too much difficulty that QE (the distance from the equant to the epicycle center) is equivalent to R $-$ $e\cos\alpha$, we obtain

$$(OE)_T = \sqrt{R^2 + 2eR \cos \alpha - 3e^2 \cos^2\alpha + 4e^2}.$$

For the Ptolemaic model (see FIGURE 5), the following holds:[31]

$$(OE)_P = \sqrt{(\sqrt{R^2 - (e \sin \alpha)^2} + e \cos \alpha)^2 + (2e \sin \alpha)^2}.$$

The maximum difference $|(OE)_T - (OE)_P|_{Max}$ occurs near the quadratures when $\alpha \cong 90°, 270°$.[32] At those positions, $(OE)_T = \sqrt{R^2 + 4e^2}$ while $(OE)_P = \sqrt{R^2 + 3e^2}$ and the maximum prosthaphairesis for each model will be given by $\delta = \arcsin(r/OE)$, where r is the radius of the epicycle (see FIGURE 7). We will then obtain the following results:

	δ_P (Maximum prosthaphairesis for Ptolemaic model)	δ_T (Maximum prosthaphairesis for Ṭūsī model)	$\delta_P - \delta_T$
Venus	45° 58′	45° 57′	1′
Mars	40° 26′	40° 12′	14′
Jupiter	11° 1′	11°	1′
Saturn	6° 11′	6° 11′	-

As can be seen, Ṭūsī is essentially correct in his assessment of the two models except in the case of Mars. Whether 14 minutes of arc was considered negligible by Ṭūsī and later Islamic astronomers would certainly seem an interesting topic to pursue.

THE CURVILINEAR VERSION OF THE COUPLE

Part of Ṭūsī's achievement lies in his ability to deal with seemingly disparate phenomena in a unified manner. He is thus able to resolve a large class of problems, in particular *ishkālāt* nos. 7–16, by appealing to a curvilinear version of the Ṭūsī couple whose purpose is to produce an oscillation between two points on a great circle arc.

The immediate occasion for introducing this second version is in the context of the moon's prosneusis point (*nuqṭat al-muḥādhāt*).[33] In Ptolemy's lunar theory, the diameter connecting the epicyclic apex and perigee is not aligned with the center of the deferent C but with a point at a distance $2e$ from that center (see FIGURE 8). (This is, as Ptolemy tells us, in order to account for certain observations at the octants.) As can be seen, however, such a motion will result in an oscillation of the apex T about the endpoint Y of line CE. Ṭūsī states that this epicyclic apex will reach its extremum when EP is perpendicular to the line of apsides; he makes the further observation that

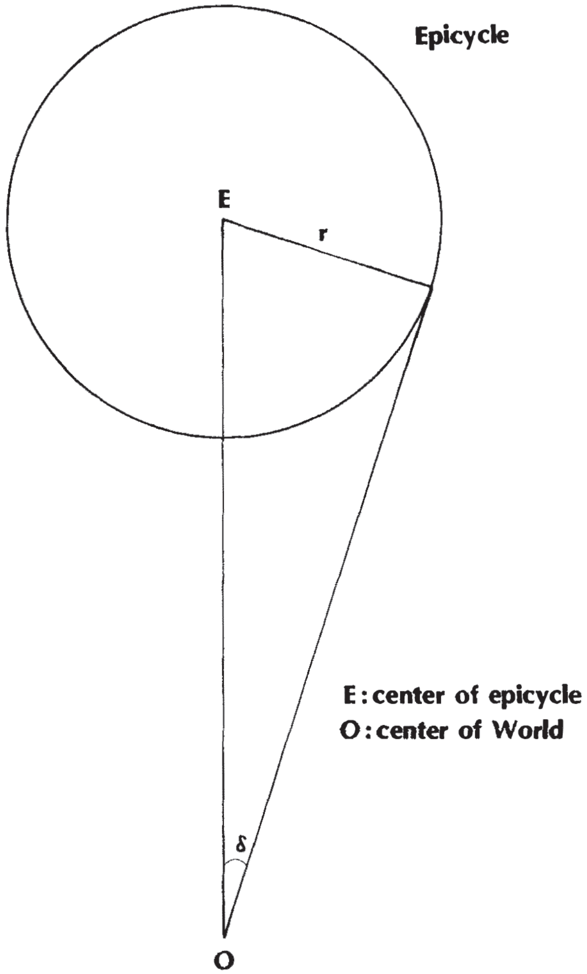


FIGURE 7.

the maximum speed (*ghāyat al-surʿa*) of point T with respect to Y will occur at the apogee and perigee. To check this, we note that the inclination of T is given by

$$\angle CEP = \arcsin\,(2e/R \cdot \sin \angle CPE),$$

which clearly reaches its maximum absolute value when $\angle CPE = 90°, 270°$. Taking the derivative of the right-hand side of the equation, we find

$$f' = (1/\sqrt{1-(2e/R \cdot \sin \angle\, CPE)^2})\ (2e/R)\ (\cos \angle\, CPE)$$

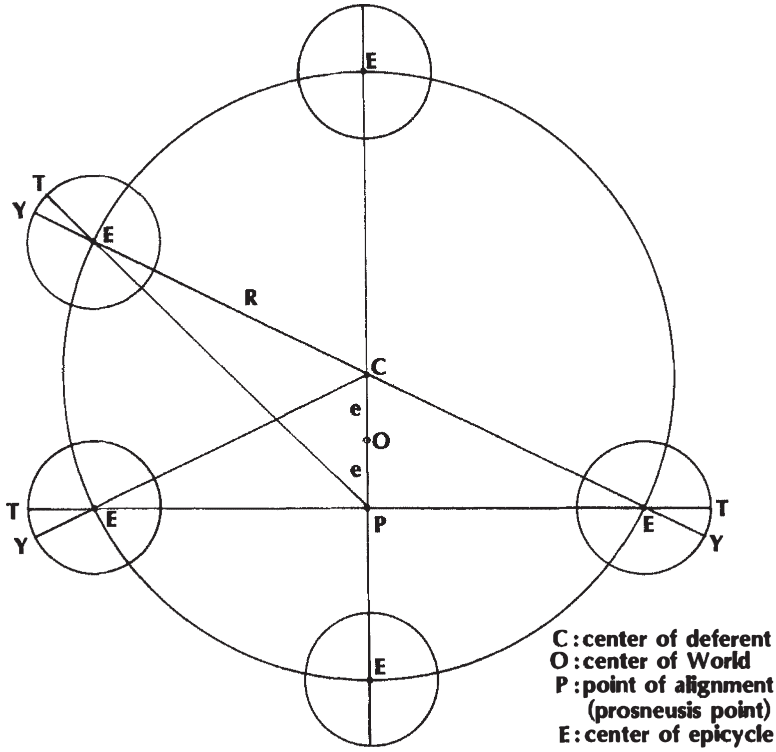


FIGURE 8.

whose maximum absolute value is reached when $\angle$ CPE = 0°, 180°, thus confirming that the maximum speed of point T will indeed take place at the apogee and perigee.

For Ṭūsī, this motion is a clear violation of the physical premises under which an astronomer should work. For the epicycle, which is a solid body, would oscillate on the diameter connecting the poles of the epicycle (*i.e.* the diameter perpendicular to the plane of the paper) and would thus not complete the required uniform rotation.[34] The emphasis on solid movers is important here. Ṭūsī would not accept, for example, a solution in which the oscillation of point T were somehow replaced by a motion on a small circle since this sort of circular motion would not have been brought about by uniformly rotating orbs. Ṭūsī makes this point explicitly when he draws our attention to the close similarity of the prosneusis point *ishkāl* and the difficulty arising from Ptolemy's latitude theory as presented in the *Almagest*.[35] In the case of all five planets, the endpoints of the diameters of the epicycles that

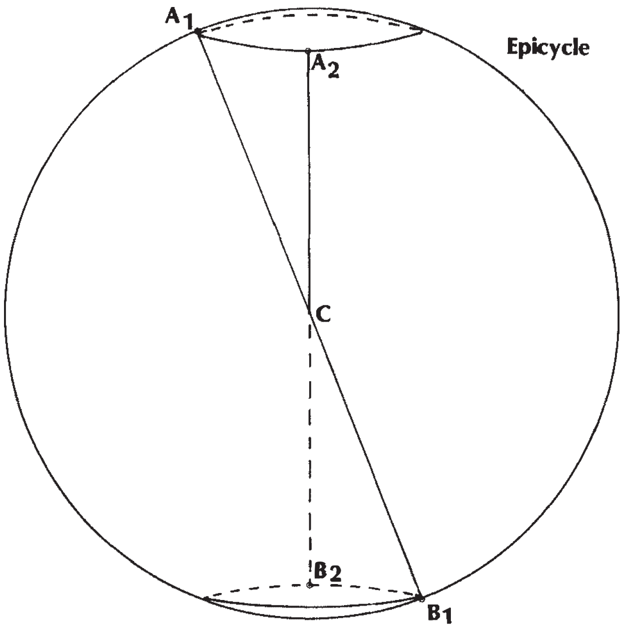


FIGURE 9.

are aligned with the equant[36] will perform a revolution upon a small circle perpendicular to the plane of the deferent. This will produce that component of latitude called the deviation (*mayl*) (see FIGURE 9).[37] Mercury and Venus are distinguished by a further latitudinal variation, called the slant (*inḥirāf*), whereby the endpoints of a second diameter at right angles to the first and in the same plane will perform a similar revolution upon small circles, again perpendicular to the deferent.

Ṭūsī raises three objections to Ptolemy's construction.[38] First, "it does not take into account the configuration (*hay'a*) of those bodies that are the principles for these motions." Since a point in a medieval cosmological system cannot simply move by itself, one must provide the appropriate uniformly rotating orbs to produce motion. Second, "it compounds the difficulty that we are expending all this effort to resolve by making the motion uniform about a point other than the center of its revolution." This is because the endpoint moves along the small circle with the same nonuniform motion as that of the epicycle center on the deferent. Finally, "just as the aforementioned small circles bring about latitudinal inclinations, they also cause inclinations to occur in longitude. . ." Unlike the first two objections, this one is not a problem of physics (or, as some would have it, "philosophy") but in the predictive ability

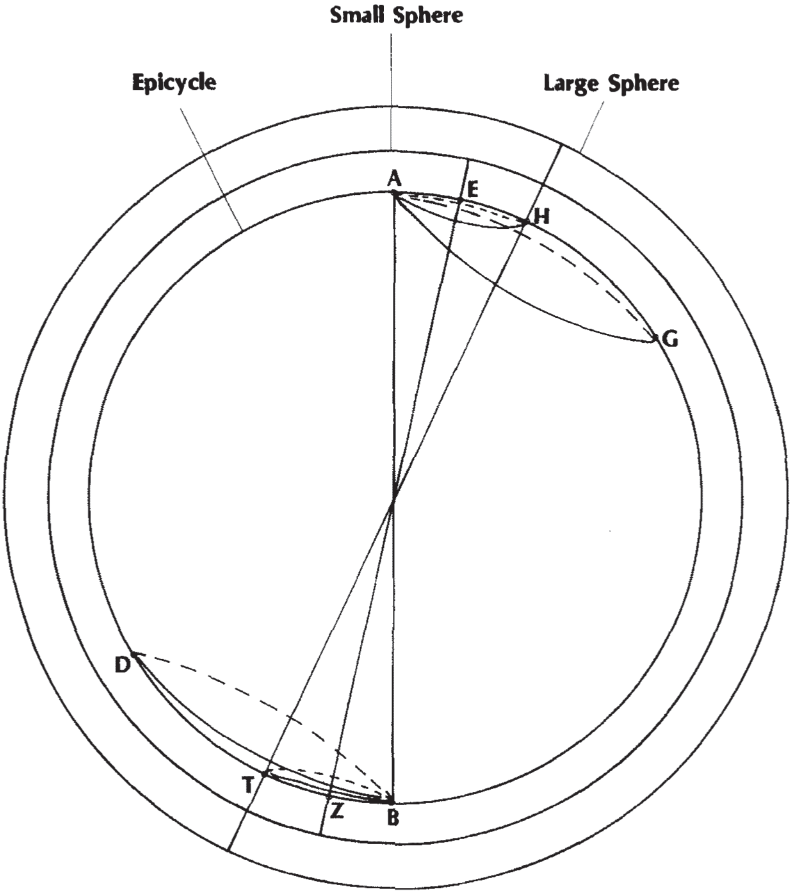


FIGURE 10.

of the model. Since the epicyclic apex revolves on a circle, motion will occur in longitude as well as latitude, thus causing the position of the planet to be removed from its proper position on the epicycle. In the extreme case, when the planet is at the epicyclic apex or perigee, this disruption will be equal to the latitudinal inclination. In order to avoid a motion in longitude, it is clear that one would need a means of causing the epicyclic apex to oscillate on an arc of a great circle perpendicular to the plane of the deferent. Thus Ṭūsī concludes that this difficulty requires the same resolution as that of the prosneusis point.

Ṭūsī's solution is based upon a series of 3 enclosing orbs surrounding the

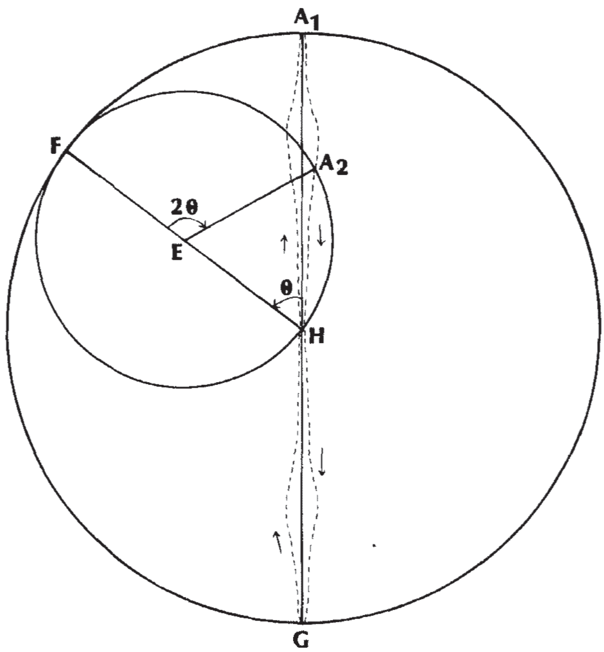


FIGURE 11.

orb to be moved.[39] For the latitudinal deviation, one has the following arrangement (see FIGURE 10). AGBD is a great circle of the epicycle that passes through the apex A and perigee B as well as the poles; it is therefore perpendicular to the deferent. Surrounding the epicycle is an orb that Ṭūsī calls the "large sphere", whose axis intersects the epicycle at points H and T, which are at a distance equal to the maximum inclination from apex A and perigee B, respectively. Between the large sphere and the epicycle is the small sphere whose axis passes through points E and Z that are the midpoints of $\widehat{AH}$ and $\widehat{BT}$. The large sphere is then given a motion equal to that of the epicycle center on its deferent while the small sphere is made to rotate with twice this motion in the opposite direction.[40] Ṭūsī concludes (incorrectly as we shall see below) that the apex will oscillate between points A and G on a great circle arc. This will account for the latitudinal deviation without a corresponding disruption of the apex in longitude (see FIGURE 11, which is drawn from a polar perspective; note that circles E and H are not in the same plane). But as a result of the motion of the two spheres, the rest of the epicycle will be moved over and above what is required by the deviation. To rectify this, Ṭūsī places a third orb (not shown in FIGURE 10) with poles that are always aligned with the oscillating apex and perigee between the small sphere and the epicycle. As should be clear from FIGURE 11, it must move in the same direction and with the same speed as the large sphere in order to bring the rest of the epicycle to its proper position. (This orb is analogous to the enclosing orb (*muḥīṭa*) of the rectilinear version.) The net result for the epicycle as a whole would then be an oscillation on an axis coincident with the mean diameter, *i.e.* the diameter of the epicycle in the plane of the deferent perpendicular to AB.[41]

Such a series of three enclosing orbs is not only useful in resolving the problem of the motion of the planetary epicycles in latitude, it can also be used whenever an oscillation between extrema on a great circle arc is needed. Thus as Ṭūsī notes, the second version of his couple may account for the oscillation of the equator of the inclined orb of the two inferior planets in latitude as well as the longitudinal inclination of the diameter of the lunar epicycle due to its alignment with the prosneusis point. Finally Ṭūsī remarks that his device could even produce a trepidation of the equinoxes as well as a cyclical change in the obliquity of the ecliptic.[42]

But there are problems with the curvilinear version, some of which Ṭūsī acknowledges, some of which he does not. One that seems to have escaped him is the failure of the couple to work as advertised. The resultant locus will not, in fact, be an arc but rather a stretched out figure 8 on the surface of a sphere (see FIGURE 11).[43] To see this we need only note that in spherical triangle EA_2H the exterior angle FEA_2 must be less than the sum of interior angles EHA_2 and EA_2H;[44] the endpoint of radius vector EA_2 must therefore always extend beyond arc A_1G except when $\theta = n90°$, n any integer, in which case A_2 will fall on it. Nevertheless, because of the small size of the arcs of oscillation, divergence will be slight.[45]

Another remaining difficulty, one that Ṭūsī must regretfully admit he is incapable of resolving, is related to objection two.[46] Because the motion of epicyclic apex A is approximately given by $A_1H - A_1H \cos \theta$,[47] it is clear that its inclination in either direction from H will be exactly equivalent in amount and duration; the Ptolemaic theory, however, requires that this inclination be of longer duration in one half than the other since the motion of point A on the small circle is coordinated with the irregular motion of the epicycle center on the deferent. Similarly, for the case of the inclination of the lunar epicycle due to the prosneusis point, Ṭūsī notes that his construction will result in a motion of inclination that is symmetrical with respect to the line joining the centers of the epicycle and the deferent whereas the Ptolemaic model results in an asymmetrical motion of inclination[48] (see above and FIGURE 8). Undoubtedly these lingering unresolved problems, as well as the lack of any model for Mercury, were important motivations for subsequent generations of astronomers.

THE DYNAMICAL PROBLEM

One aspect of the Ṭūsī models that is especially perplexing is the manner in which certain orbs may move others.[49] In particular, it is not immediately obvious how the *muḥīṭa*, or enclosing sphere, is capable of moving the epicycle. Because they are coaxial and concentric, and because of the lack of friction or violent motion in the heavens, it is difficult to see how Ṭūsī intends the *muḥīṭa* to cause a motion in the epicycle.

In order to deal with this specific issue, it will be useful to digress a bit and briefly discuss the general problem of medieval celestial dynamics. Each uniformly rotating orb, of course, is the source of a single motion, but in addition it may also be capable of simultaneously moving another orb. To understand how this is possible, it is important to be clear as to what precisely is meant by the term orb (*falak*). Ṭūsī defines it as "a spherical solid bounded by two concentric parallel surfaces," one convex, the other concave.[50] Some orbs, though, such as epicycles, have an inner surface that degenerates to a point; it is on this account that an orb may sometimes be a sphere.[51] In the case of eccentrics and epicycles, there is really no dynamical problem since these bodies are moved simply as a consequence of being contained within the thickness of another moving orb with a different center. (See FIGURE 12 in which these two possibilities are illustrated.)

On the other hand, there *was* a problem in understanding how one orb could move another orb concentric to it. Part of the reason for the difficulty arose because of a somewhat different conception of the orbs in the cosmographical tradition exemplified by *Al-Tadhkira* than that found, say, in Ptolemy's *Planetary hypotheses* and in the less specialized Arabic literature such as Ibn Rushd's *Talkhīṣ mā baʿd al-ṭabīʿa* (*Epitome of the metaphysics*) or the *Rasāʾil* (*Epistles*) of the Ikhwān al-Ṣafāʾ.[52] There the heavens are stated to be a single living being. Hence the daily motion is simply the motion of the whole, and the other orbs are considered parts of this whole. But in the *hayʾa* literature, the daily motion is caused by the ninth orb which is a discrete orb as defined above. Thus, for example, Ṭūsī does not take it for granted that the eighth orb, which contains the fixed stars, or any other orb for that matter, will partake in some automatic way of the first motion, *i.e.* the daily rotation of the heavens. The ninth orb, which shares the same center but not the poles of the eighth, is given the awesome task of transmitting to the eighth orb, as well as to all else in the heavens, its own daily rotation.[53] Ṭūsī is not very explicit in telling us how this transmission will occur. The classical solution, and the one most widely assumed in modern discussions of medieval cosmology, would somehow attach the poles of the eighth orb into the ninth. This is rejected by the commentators on *Al-Tadhkira*. Al-Sharīf ʿAlī al-Jurjānī (*d.* 1413), for example, does so on the grounds that

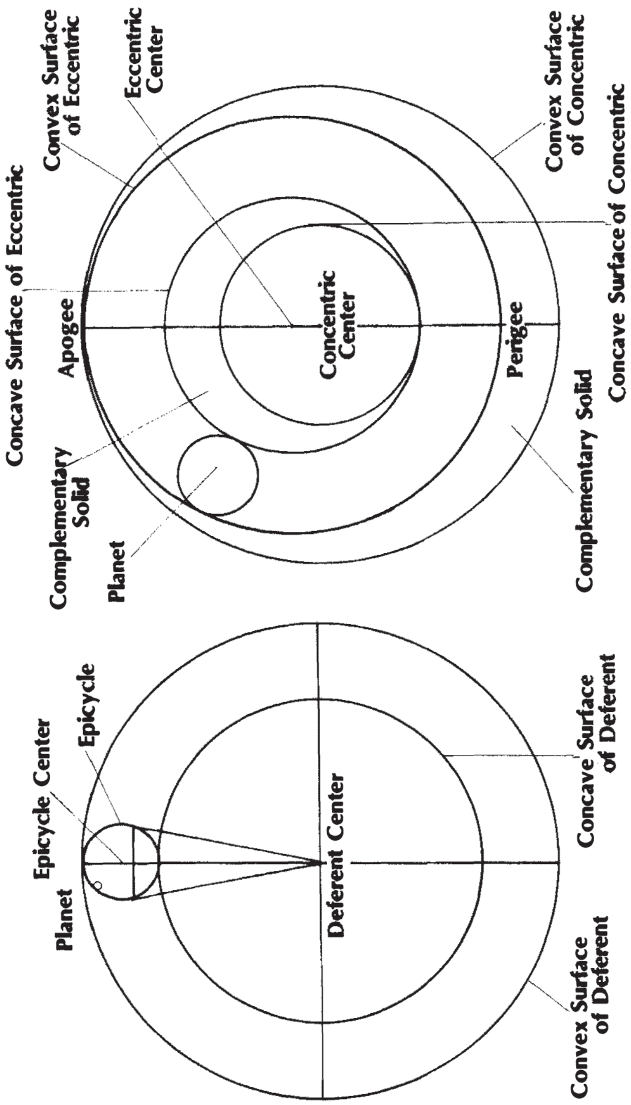


Figure 12. Epicyclic and eccentric hypotheses.

> . . . the postulated points on the concave surface of the enclosing orb are of the same substance (*māhiyya*) on account of its being simple; thus the attachment (*tashabbuth*) of the two poles of the contained orb to two designated points on the enclosing orb to the exclusion of any other points is implausible.[54]

With the cosmos as animal explanation dismissed and the attachment hypothesis rejected, the commentators resort to what one may call action at a distance. What this amounts to is that the soul of an encompassing orb (which is the efficient cause of that orb's proper motion) may have a sufficient moving faculty to cause an enclosed concentric orb to move as well. Again to quote Jurjānī:

> When the orbs are concentric (whether or not the axes are the same) and when the orbs have different centers but the axis of the enclosing orb passes through the center of the contained orb, the moving soul (*al-nafs al-muḥarrika*) of the enclosing orb may have a sufficient faculty (*quwwa*) to move the contained orb, and hence will move it, inasmuch as every action is not contingent upon a corporeal instrument (*āla jusmāniyya*), or it may not have [a sufficient faculty] whereupon it will not move [the enclosed orb].[55]

This explanation is, of course, applicable to the *muḥīṭa*. But there is another question that arises. Why does one need the *muḥīṭa* at all? Since it and the epicycle share the same center and axis, the epicycle alone would be sufficient if its motion were made the sum of the motion of the Ptolemaic epicycle and the enclosing sphere.[56] It is characteristic that Ṭūsī does not do this; after all, he is a reformer, not a revolutionary. As he tells us in his introduction, *Al-Tadhkira* is "an account of what is established in the *Almagest*."[57] It is therefore important for him to remain faithful to Ptolemy's individual elements and parameters. To subsume the Ptolemaic epicycle, the source of the first anomaly, into some new combined motion may have appeared as too radical a step to take. As we have seen, any deviation from Ptolemy, such as those noted above with regard to predicted planetary positions and the inability of the curvilinear version to be exactly coordinated with the prescribed Ptolemaic motions, are occasions for regret.[58]

CONCLUSION

Broadly speaking, there is no doubt that Ṭūsī is bound by the basic approach of Ptolemaic astronomy and by a fundamentally Aristotelian worldview. Yet given these constraints, he has managed to effect a number of new and important departures. Mathematically, we should recognize that the significant feature of both versions of the Ṭūsī couple is that it allows one to isolate the linear from the circular component of Ptolemy's rather complex motions. As we have seen, this analytical approach is explicit; it is further revealed by Ṭūsī's desire to quantify the maximum difference between the predictions of his lunar model and that of Ptolemy, and by a remarkable interest in the maximum speed achieved by the mean epicyclic apex as a result of Ptolemy's lunar prosneusis. These examples indicate an emerging concern for a variety of problems that are of great historical importance; obviously a further examination of the mathematics of the non-Ptolemaic models of Ṭūsī and his successors would be highly desirable.

But it is the physical aspect of Ṭūsī's work that I would maintain to be the most significant historically. By showing that one could indeed reform the Ptolemaic system according to the accepted physics, Naṣīr al-Dīn has given both legitimacy and immediacy to a program that had been until his time talked about but not acted upon. He no doubt saw himself as saving the Ptolemaic system by giving it consistency; ironically, the insistence upon an astronomy that was both mathematically and physically sound would eventually lead to the demise of classical cosmology.

ACKNOWLEDGMENTS

I wish to express my gratitude to Sally Palchik Ragep for drafting the diagrams and to A. I. Sabra for commenting on various stages of this article. They are, of course, not responsible for any remaining shortcomings.

NOTES

1. Dreyer, 269, n. 1, seems to have been the first to recognize that Copernicus employs Ṭūsī's construction for producing rectilinear motion.
2. Hartner 1969, 302.
3. Whence the name of the enterprise *ʿilm al-hay'a, i.e.* the "science of *hay'a*." Eventually this came to denote astronomy in a general sense though the more specialized meaning was still understood. See, for example, Ṭāshkubrīzāde, 1: 372.
4. The most important of the early criticisms of Ptolemy occurs in Ibn al-Haytham's *Al-Shukūk ʿalā Baṭlamyūs*, partially translated in Sabra 1978. George Saliba deals with two other contemporary criticisms in his Ibn Sīnā and Abū ʿUbayd al-Jūzjānī: The problem of the Ptolemaic equant. Al-Bīrūnī (973–*ca.* 1050) is also aware of the problems inherent in physicalizing Ptolemy's geometrical models; see, for example, his *Al-Qānūn al-Masʿūdī*, 2: 838.
5. *Cf.* Shams al-Dīn Muḥammad al-Khafrī (*fl.* early 16th c.), Bk. II, Ch. 11, ff. 189b–190a.
6. See, among others, Kennedy, 366–7; Hartner 1975, 9.
7. *Tadhkira*, Bk. I, Ch. 2. An edition and translation of Bk. I and Bk. II (Chapters 1–11) of *Al-Tadhkira* is included in my *Cosmography in the* Tadhkira *of Naṣīr al-Dīn al-Ṭūsī.*
8. This is explicitly stated by Niẓām al-Dīn al-Nīsābūrī (*fl.* early 14th c.), f. 10a.
9. *Cf.* Aristotle *De Caelo*, Bk. I, Ch. 3, 270b5–16, and *Metaphysica*, Bk. XII, Ch. 7, 1072a20–23.
10. *Tadhkira*, Bk. I, Ch. 2, par. 3.
11. One finds the non-Ptolemaic models of the *Ḥall* in Ch. 3. The *Ḥall* and the *R.-i Muʿīniyya*

have been published in individual facsimile editions by Muḥammad Taqī Dānish-Pizhūh. E. S. Kennedy's 1984 article in *Centaurus* describes both treatises. Wheeler Thackston is currently preparing an edition of the two works and a translation of the *Ḥall* (the latter in collaboration with myself).

12. *Ḥall*, Ch. 5.
13. *Ḥall*, 7.
14. *Ḥall*, 7–9; *Tadhkira*, Bk. II, Ch. 11, pars. 3–4.
15. *Cf.* Kennedy, 368–70; Hartner 1969, 289; and Neugebauer, vol. 1, 10, vol. 2, 1035.
16. *Tadhkira*, Bk. I, Ch. 2; this confusion about rolling has unfortunately led Moesgaard, 129 to attempt to distinguish between an alleged "mathematical" approach by Copernicus and a "physical" one by Ṭūsī.
17. *Ḥall*, 9–13; *Tadhkira*, Bk. II, Ch. 11, pars. 5–9, 11. A familiarity with the corresponding Ptolemaic models is hereafter assumed; excellent presentations of them can be found in Neugebauer and Pedersen.
18. al-Nīsābūrī, f. 65b.
19. *Cf. Tadhkira*, Bk. I, Ch. 1, par. 14: "The great circle equidistant from the two poles is the sphere's equator."
20. Both Carra de Vaux and Hartner failed to distinguish between the sphere's equator and its *minṭaqa* and consequently were led to misconstruct the models. One unfortunate result of this has been the assumption by some that medieval astronomers were as haphazard and uninterested in cosmology as their modern commentators, a wholly unwarranted conclusion.
21. The size of the *muḥīṭa* is not specified; Ṭūsī only says that it may be "of any appropriate thickness" but "it should not be large lest it occupy too big a space" (*Tadhkira*, Bk. II, Ch. 11, par. 6). Hartner believes that the *muḥīṭa* should be of zero thickness since any additional space would cause a disruption of Ptolemy's planetary sizes and distances (Hartner 1969, 292–93). But this would only be a consideration if the actual distances could be verified, which, of course, was not possible except for the sun and moon. As is well known, the accepted classical distance to the sun found by Aristarchus is considerably off, but this allowed, purely accidentally, for the complete systems of orbs of Venus and Mercury to be placed between the moon and sun. There was still some space left over between Venus and the sun, however; if anything, the additional *muḥīṭas* could have helped fill this gap.
22. *Ḥall*, 10; *Tadhkira*, Bk. II, Ch. 11, par. 6.
23. This is not a necessary condition; in the *Ḥall*, 11, Ṭūsī states that the large sphere may move in either direction.
24. *Ḥall*, 12; *Tadhkira*, Bk. II, Ch. 11, par. 11.
25. *Tadhkira*, Bk. II, Ch. 11, par. 12.
26. Hartner's claim that Ṭūsī had "invented a theory based on the same principle but too complicated to be explained here, which he hopes to bring as an appendix" (Hartner 1969, 299) is a misreading.
27. *Tadhkira*, Bk. II, Ch. 11, par. 10.
28. *Ibid*.
29. Hartner 1969, 299.
30. *Tadhkira*, Bk. II, Ch. 11, par. 11.
31. Pedersen, 280.
32. The actual values near the first quadrature, which are dependent on the eccentricity, are: Venus, 90°; Mars, 89°53′; Jupiter, 89°59′; and Saturn, 89°59′. Rounding off to 90° will have an insignificant effect on the accuracy of δ to the nearest minute.
33. *Tadhkira*, Bk. II, Ch. 11, pars. 13–14.
34. Ibn al-Haytham presents the problem in a similar fashion in his *Shukūk*, 15–20.

35. *Tadhkira*, Bk. II, Ch. 11, par. 15.
36. This alignment is with reference to the longitude theory; it will, of course, be disrupted as a result of Ptolemy's latitude constructions.
37. For accounts of Ptolemy's latitude theory, see Neugebauer, vol. 1, 206–26 and Pedersen, 355–86. Concerning the latitude theory in the Arabic tradition, see Sabra 1979, esp. 388–90.
38. *Tadhkira*, Bk. II, Ch. 11, par. 16.
39. *Tadhkira*, par. 19.
40. Because the two spheres must rotate uniformly, Ṭūsī is presumably here referring to his own longitudinal models in which the motion of the epicycle center is uniform.
41. This model bears an important relation to an earlier one of Ibn al-Haytham, a connection Ṭūsī explicitly acknowledges (*Tadhkira*, Bk. II, Ch. 11, pars. 17–18). Briefly, Ibn al-Haytham's model is a similar series of spheres except that it does not contain the small sphere. Thus it will basically reproduce Ptolemy's small circles by means of solid bodies but without resolving Ṭūsī's second and third objections. The details of Ibn al-Haytham's model and its similarity to Eudoxus' homocentric spheres are topics to which I hope to return in a future article.
42. *Tadhkira*, Bk. II, Ch. 11, pars. 20–22.
43. This was originally pointed out to me by E. S. Kennedy. Otto Neugebauer further noted that the figure could not be a hippopede since it has a vertical rather than horizontal tangent at $\theta = n90^\circ$ inasmuch as the area of triangle EA_2H will approach 0 at these points. Thus the figure will not be a smooth curve but rather one with pinched cusps. Needless to say I am indebted to both scholars for their insights.
44. This was recognized by al-Khafrī in his commentary, ff. 214b–215a, who cites Proposition 11, Book I of Menelaus' *Spherics*.
45. Even in the case of the greatest oscillation, the 24.538° in either direction resulting from the moon's prosneusis, the maximum deviation from arc AG will only be .214°, which is about .87%. This will approximately occur when $\theta = n180^\circ \pm 35^\circ$, n any integer. For smaller arcs of oscillation, the percentage deviation will be even smaller.
46. *Tadhkira*, Bk. II, Ch. 11, par. 19.
47. We here ignore the deviation from the great circle arc.
48. *Tadhkira*, Bk. II, Ch. 11, par. 21.
49. I must here acknowledge my appreciation to my teacher A. I. Sabra for having first posed to me the questions upon which this section is based; he is, of course, absolved from any shortcomings in the answers provided.
50. *Tadhkira*, Bk. I, Ch. 1, par. 16.
51. The definition of orb only becomes standardized — more or less — after the 12th century. Even then *falak* often refers, conventionally as Ṭūsī states (*Tadhkira*, Bk. II, Ch. 3, par. 3), to a circle rather than a solid body. It could even designate the heavens as a whole. It is clear, however, that Ṭūsī is attempting to restrict its usage as indicated, at least in his cosmographical works. *Cf.* Hartner, *Encyclopaedia of Islam*, 2: 761–63.
52. Ibn Rushd, 133. For Ptolemy the relevant passage occurs in his *Kitāb al-Iqtiṣāṣ* (*Planetary hypotheses*), British Museum MS Arab. 426, f. 93a, lines 22–23 (reproduced in Goldstein, 36); German translation by L. Nix in Heiberg, 112. *Cf.* Plato, *Timaeus*, 32d.
53. *Tadhkira*, Bk. II, Ch. 4, pars. 7–8.
54. al-Jurjānī, f. 36b, lines 24–25.
55. al-Jurjānī, f. 37a, lines 5–8.
56. al-Khafrī suggests this approach in his commentary, ff. 193a–b. A similar solution is applicable to Ṭūsī's lunar deferent and the concentric and coaxial inclined orb that encloses it.
57. *Tadhkira*, Bk. I, Introduction, par. 4.
58. *Cf. Tadhkira*, Bk. II, Ch. 11, par. 23.

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Subsequent to the writing of this paper, the article by E. S. Kennedy referred to in note 11 has appeared as Two Persian astronomical treatises by Naṣīr al-Dīn al-Ṭūsī, *Centaurus*, 27 (1984): 109–20.

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.

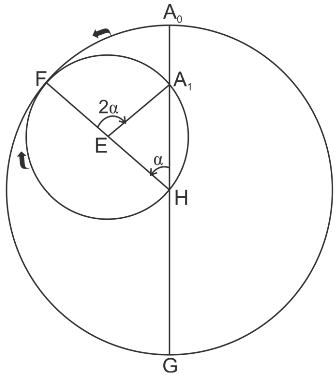

Figure 1. The Rectilinear Version of the Ṭūsī-couple.

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.

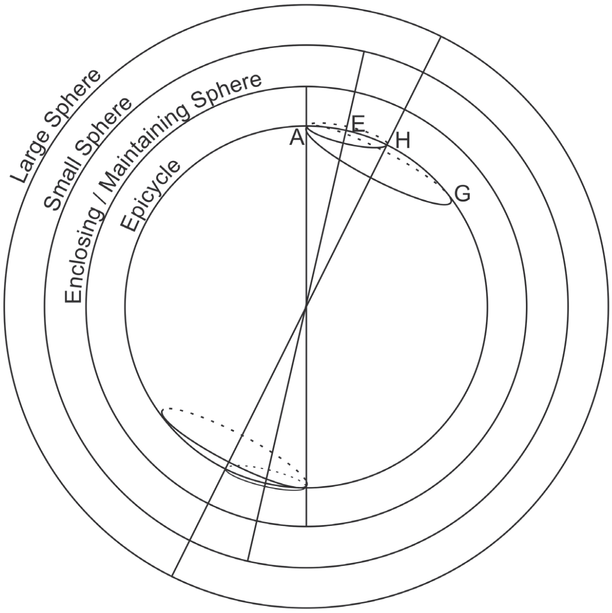

Figure 2. The Curvilinear Version of the Ṭūsī-couple.

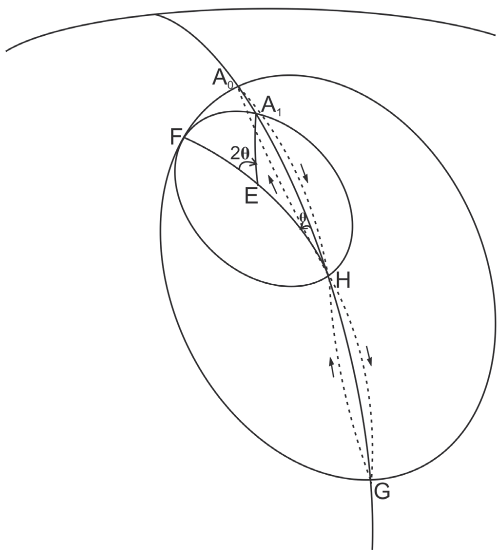

Figure 3. Polar View of the Curvilinear Ṭūsī-couple (dotted line represents actual path of pole A).

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 هيأت 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 ٨: در تصویر تعدیل الایام بلیالیها
Chapter 9: On depicting the Indian Circle, the direction of a locale and other matters	فصل ٩: در صورت دایرهٔ 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 ٦٤٣ 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

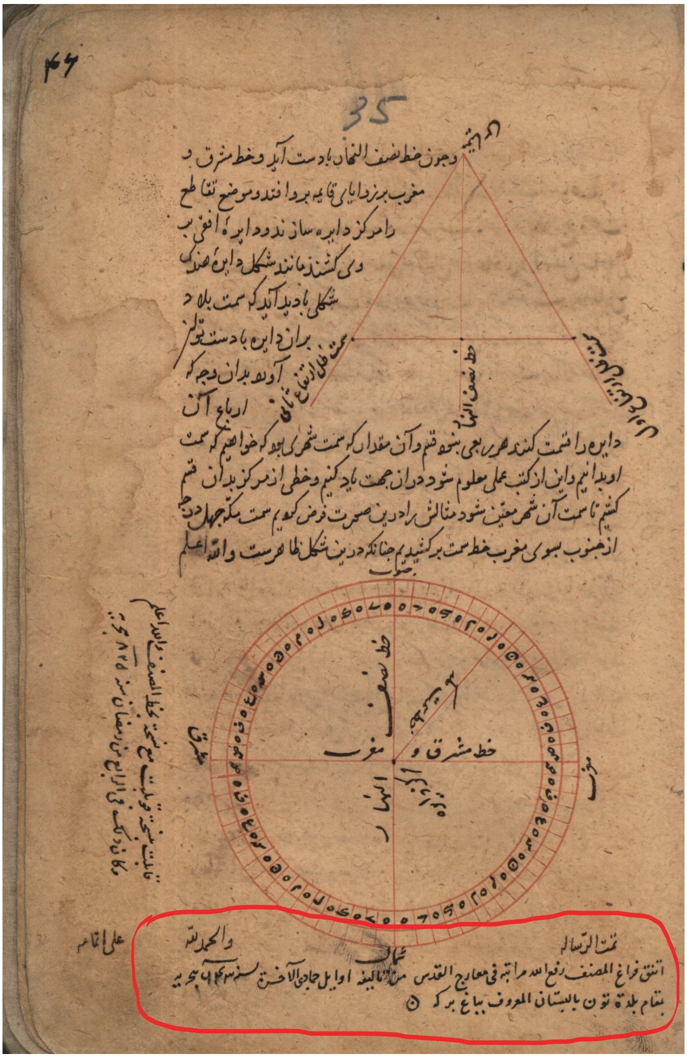

Figure 4. Colophon (boxed in red by current author) of *Ḥall-i mushkilāt-i Muʿīniyya,* Tashkent, al-Bīrūnī Institute of Oriental Studies, MS 8990, f. 46a (original foliation). Courtesy of the Institute.

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Chioniades, Gregor [George]

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(The Persian translation has been published as "Khiyūniyādis [Chioniades]," in *Dāʾirat al-Maʿārif-i Buzurg-i Islāmī* (Iran) [*The Great Islamic Encyclopaedia*], vol. 23 (Tehran: Markaz-i Dāʾirat al-Maʿārif-i Buzurg-i Islāmī, 1396/2018), 355-58.)

Chioniades, christened George, was born sometime between 1240 and 1250 CE in Constantinople and became one of the leading figures in Byzantine astronomy.[1] Little is known about his early life and education, but in 1295 he traveled to the kingdom of Trebizond, which was ruled at the time by Emperor John II Komnenos (reigned 1280–1297). There it is likely that he composed notes to John of Damascus's (d. 749 CE) *Dialectics* and a work entitled *On the Orthodox Faith*. Trebizond would serve as a way station for the ultimate aim of his journey, which was Īlkhānid Iran; in this he was supported by Komnenos, and later that year he arrived at the court of Ghāzān Khān (reigned 694-703 H/1295-1304 CE) in Tabrīz. George Chrysococces (fl. 1350) would later relate, based on the testimony of his teacher Manuel (fl. 1330s CE), that at first Chioniades found it difficult to find a teacher of astronomy, since, according to Manuel, that was a subject restricted to Persians only. But he persevered and apparently won favor with Ghāzān Khān as well as with the redoubtable Rashīd al-Dīn Ṭabīb (d. 718 H/1318 CE), the historian, physician, and sometime minister at the court of Ghāzān. Indeed, Chrysococces informs us that "Chioniades shone in Persia, and was thought to be worthy of the King's

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[1] An important source for his life is his sixteen extant letters that have been published in Jean B. Papadopoulos, ed., *Grigoríou Chioniádou tou astronómou epistolaí* [in Greek, Modern] (Thessaloníki: Panepistimio Thessaloníkis, 1929) and idem, "Une lettre de Grégoire Chioniadès, évêque de Tabriz—Rapports entre Byzance et les Mongols de Perse," in *Mélanges Charles Diehl: Études sur l'histoire et sur l'art de Byzance*, vol. 1, *Histoire* (Paris: E. Leroux, 1930), 257-62. An excellent summary of what is known of the life of Chioniades can be found in Joseph Gerard Leichter, "The Zīj as-Sanjarī of Gregory Chioniades: Text, Translation and Greek to Arabic Glossary" (PhD diss., Brown University, 2004), 2-6. Cf. L. G. Westerink, "La profession de foi de Grégoire Chioniadès," *Revue des études byzantines* 38 (1980): 233-45; and David Pingree, "Gregory Chioniades and Palaeologan Astronomy," *Dumbarton Oaks Papers* 18 (1964): 133-60; reprinted in *Pathways into the Study of Ancient Sciences: Selected Essays by David Pingree*, eds. Isabelle Pingree and John M. Steele, *Transactions of the American Philosophical Society*, n.s., 104, no. 3 (Philadelphia: American Philosophical Society, 2014), 365-91. See also Maria Mavroudi, "Exchanges with Arabic Writers During the Late Byzantine Period," in *Byzantium: Faith and Power (1261-1557): Perspectives on Late Byzantine Art and Culture*, ed. Sarah T. Brooks (New York: Metropolitan Museum of Art, 2007), 62-75.

honour."[2] This would seem partially corroborated by the existence of a short tract by Rashīd al-Dīn giving answers to questions posed by Chioniades on difficult physical and theological matters, which was later translated into Greek.[3] More importantly, Chioniades was granted what he so much desired, namely instruction in astronomy. He tells us that his teacher was someone known in Greek sources as Shams Bukharos, whom we can identify as Shams al-Dīn Muḥammad ibn ʿAlī Khwāja al-Wābkanawī al-Munajjim (b. 652 H/1254 CE), the author of a *zīj* (astronomical handbook with tables) entitled *al-Zīj al-muḥaqqaq al-sulṭānī ʿalā uṣūl al-raṣad al-Īlkhānī* (The verified *zīj* for the sultan based on the principles of the Īlkhānī observations) and a work on the astrolabe; he is also most likely the author of a commentary on Naṣīr al-Dīn al-Ṭūsī's (d. 672 H/1274 CE) astronomical work *al-Tadhkira fī ʿilm al-hayʾa* entitled *Tibyān maqāṣid al-Tadhkira* (Exposition of the intent of the *Tadhkira*).[4] From November 1295 until November 1296, Shams al-Dīn apparently dictated, in Persian, the rules for using the *Zīj al-ʿAlāʾī* of ʿAbd al-Karīm al-Fahhād (fl. 1176), which Chioniades rendered into Greek as the *Persian Astronomical Composition*.[5] During this period he also collected a number of works that he would subsequently translate into Greek.

By September 1301, Chioniades was back in Trebizond and had returned home to Constantinople in April 1302. There he taught students the astronomy and medicine he had learned while in Persia and translated, presumably from Persian into Greek, a set of recipes for antidotes as well as a number of astronomical treatises. He also wrote a confession of faith, perhaps to counter accusations of heresy accruing from his work in astrology and his years

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[2] For the full report by Chrysococces, see Raymond Mercier, "The Greek 'Persian Syntaxis' and the Zīj-i Īlkhānī," *Archives internationales d'histoire des sciences* 34 (1984): 35-60, on 35-36; reproduced with slight emendations in Leichter, "Zīj as-Sanjarī," 3.

[3] See Zeki Velidi Togan, "İlhanlı Bizans kültür münasebetlerine dair vesikalar" ("A Document Concerning Cultural Relation Between the İlkhanide and Byzantiens" [sic]), *İslâm Tetkikleri Enstitüsü Dergisi* 3 (1959–60): 315-78 (= 1-39). I owe this reference to Dimitri Gutas, "Arabic into Byzantine Greek: Introducing a Survey of the Translations," in *Knotenpunkt Byzanz: Wissensformen und kulturelle Wechselbeziehungen*, eds. Andreas Speer and Philipp Steinkrüger (Berlin: De Gruyter, 2012), 246-62, on 258.

[4] On Shams al-Dīn al-Wābkanawī and his identification with Shams Bukharos, see F. Jamil Ragep, "New Light on Shams: The Islamic Side of Σὰμψ Πουχάρης," in *Politics, Patronage and the Transmission of Knowledge in 13th - 15th Century Tabriz*, ed. Judith Pfeiffer (Leiden; Boston: Brill, 2014), 231-47 esp. 243-45.

[5] David Pingree, *The Astronomical Works of Gregory Chioniades*, vol. 1, *The Zīj al-ʿAlāʾī* (Amsterdam: J. C. Gieben, 1985), 17-18.

among the Persians.[6] Apparently sufficiently rehabilitated, he was appointed Bishop of Tabrīz in 1305 and took the name Gregory, but he may not have returned to Tabrīz until about 1310. By 1315, he was again in Trebizond, where he lived as a monk until his death around 1320.

The known astronomical works that Chioniades either translated or reworked from Islamic sources are the following:[7]

1) *al-Zīj al-ʿAlāʾī* of ʿAbd al-Karīm al-Shīrwānī al-Fahhād (ca. 1176), via a Persian version made by Shams al-Dīn (according to David Pingree).[8]

2) An abridged version of *al-Zīj al-Sanjarī* of ʿAbd al-Raḥmān al-Khāzinī (ca. 1120), a Greek freedman of a judge in Marv; made after 1) and directly from the Arabic (according to Joseph Leichter).[9]

3) The *Īlkhānī Zīj* of Naṣīr al-Dīn al-Ṭūsī.

4) A short *Syntaxis*, perhaps by Shams al-Dīn al-Bukhārī.

5) A longer *Revised Canons*, again perhaps by Shams al-Dīn al-Bukhārī. (Pingree takes this to be by Chioniades, who, he claims, was attempting to show his competence in using the tables of *al-Zīj al-ʿAlāʾī.*)[10]

6) A work called *Schemata of the Stars* (Περὶ τῶν σχημάτων τῶν ἀστέρων).[11]

7) A work on the astrolabe by Shams al-Dīn.[12]

8) *On the Genethlialogical Computation*, probably by Shams al-Dīn, which concerns the horoscope of a certain Fakhr al-Dīn born in Tabrīz on 14 Dhū al-ḥijja 666 H (25 August 1268).[13]

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[6] Westerink, "La profession de foi."

[7] All or some of these works are preserved in Vaticanus Graecus MS 211 (Rome), Vaticanus Graecus MS 1058 (Rome), and Laurentianus MS 28, 17 (Florence). Convenient listings (complete) are in Pingree, *Astronomical Works of Gregory Chioniades*, 23-28, and Leichter, "Zīj as-Sanjarī," 12-13 (partial, highlighting the works attributable to Chioniades).

[8] Edition and translation in Pingree, *Astronomical Works of Gregory Chioniades*, 36-243.

[9] Leichter, "Zīj as-Sanjarī," 19-162 (English translation), 367-567 (Greek text).

[10] Pingree, *Astronomical Works of Gregory Chioniades*, 21-22; edition and translation, 260-333. The work is a report by Chioniades, but it seems to be based on observations and calculations made by Shams al-Dīn.

[11] Edition and translation in E. A. Paschos and P. Sotiroudis, *The Schemata of the Stars: Byzantine Astronomy from A.D. 1300* (Singapore; River Edge, NJ: World Scientific, 1998), 26-53.

[12] The Greek version of the introduction has been edited and translated into English by Elizabeth A. Fisher, "Arabs, Latins and Persians Bearing Gifts: Greek Translations of Astrolabe Treatises, ca. 1300," *Byzantine and Modern Greek Studies* 36, no. 2 (2012): 161-77.

As for the first three *zīj*es, one is struck by the fact that all were considerably out of date by the 1290s. The *zīj*es of Fahhād and Khāzinī had certainly been superseded by the *Īlkhānī Zīj*, which itself had been made obsolete by the *zīj*es of Muḥyī al-Dīn al-Maghribī (d. 1283), which, unlike Ṭūsī's *Īlkhānī Zīj*, incorporated the latest observations made at Marāgha.[14] It is not clear why these *zīj*es were chosen, but they may have been more "elementary" in some sense. Pingree notes that when translating *al-Zīj al-ʿAlāʾī*, Chioniades displays a remarkable degree of ignorance, often transcribing Persian words into Greek when he did not understand the content.[15] But Leichter (the editor and translator into English of the Greek version of the *Sanjarī Zīj*) has noted an improvement in Chioniades's knowledge, this time presumably in Arabic, when translating the *Sanjarī Zīj*.[16] Of considerable importance in determining how far along Chioniades got in his apprenticeship into Islamic astronomy is whether the purported works of Shams al-Dīn (the short *Syntaxis* and the longer *Revised Canon*), which are found in Greek translation in some of the manuscripts, contain any of the newer material from the Marāgha and Tabrīz observations and whether the Persian *Syntaxis* of Chrysococces, which he says comes from the work of Chioniades, contains this new material. Raymond Mercier has claimed, somewhat unconvincingly, that the Persian *Syntaxis* of Chrysococces was mostly derived from the *Īlkhānī Zīj*, but this was disputed by Pingree, who held that there is substantial evidence that Chrysococces used the *ʿAlāʾī* and *Sanjarī zīj*es, in addition to the *Īlkhānī Zīj*, all of which were translated by Chioniades.[17] But neither seems to have considered that Chrysococces, and Chioniades himself, may have used sources and observations post-dating the *Īlkhānī Zīj,* whether from someone like Maghribī or from Shams al-Dīn. A fresh examination of the works attributed to Shams al-Dīn, along with a comparison of contemporaneous works in Arabic and Persian, is necessary in order to resolve some of these issues. The Greek translation of the astrolabe treatise purportedly by Shams al-Dīn (no. 7) still awaits comparison with the Persian astrolabe treatise

[13] Edition and translation in Pingree, *Astronomical Works of Gregory Chioniades*, 242-59.

[14] See George Saliba, *A History of Arabic Astronomy: Planetary Theories during the Golden Age of Islam* (New York: New York University Press, 1994), 163-86, 208-30.

[15] Pingree, *Astronomical Works of Gregory Chioniades*, 18-21.

[16] Leichter, "Zīj as-Sanjarī," 11-12.

[17] See Mercier, "The Greek 'Persian Syntaxisʾ." Pingree responded to Mercier in his "In Defence of Gregory Chioniades," *Archives internationales d'histoire des sciences* 35, nos. 114/115 (1985): 436-38.

contained in Istanbul, Topkapı, Ahmet III 3327 and attributed to Shams al-Dīn al-Wābkanawī. The identity of Fakhr al-Dīn in no. 8 has yet to be determined.

Treatise no. 6 has attracted considerable interest since Otto Neugebauer pointed out that it contained a diagram of the so-called Ṭūsī-couple of Naṣīr al-Dīn al-Ṭūsī, a device for producing oscillating rectilinear motion from two circular motions;[18] its various versions were used by Ṭūsī in a number of ways, in particular to deal with the irregular (and thus unacceptable) motion brought about by Ptolemy's (fl. 140 CE) equant model. Later it was used by Copernicus (d. 1543 CE) in several of his astronomical models. The existence of such a device in a "western" language that had clearly come from an Islamic source was evidence used by Noel Swerdlow and Neugebauer to advocate their position that Copernicus was indebted to Islamic astronomy for a number of his models.[19] Recently it has been shown that this work by Chioniades, the *Schemata of the Stars*, is derived from two Persian works of Ṭūsī, his *Risāla-yi Muʿīniyya* and its appendix, the *Ḥall-i mushkilāt-i Muʿīniyya.*[20] In particular, the versions of the Ṭūsī-couple and the lunar model found in the *Schemata* are the ones found in the *Ḥall* and are not in either of Ṭūsī's later Arabic works, the *Taḥrīr al-Majisṭī* or *al-Tadhkira fī ʿilm al-hayʾa*. Another interesting aspect of the *Schemata* is that Chioniades has faithfully followed the star listings in the *Muʿīniyya;* in fact, he uses corrupted forms of Greek names that had entered Arabic with the translations from Greek in the ninth century instead of their correct Greek forms. A rather striking example of this is that Chioniades names a northern constellation κακκαοῦς rather than the correct Greek name Κηφεύς, clearly indicating that he is simply copying the corrupted Arabic name *qayqāwus* (قيقاوس), which is a simple mistake for what should have been the correct transcription, namely (قيفاوس). Pingree notes other cases of transcription of Arabic/Persian terminology when Chioniades did not know the meanings or equivalents in Greek.[21]

This raises the question of how well Chioniades knew Persian or Arabic. As previously noted, it would seem, based on evidence compiled by Pingree and also the fact that he uses the Persian *Muʿīniyya* rather than its updated Arabic version, i.e., the *Tadhkira*, that Chioniades and/or Shams al-Dīn preferred using Persian over Arabic. This may well reflect the cultural interactions

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[18] Otto Neugebauer, *A History of Ancient Mathematical Astronomy*, 3 parts (Berlin; New York: Springer-Verlag, 1975), 2:1035.
[19] N. M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus,* 2 parts (New York: Springer-Verlag, 1984), 1:47-48.
[20] For further details, see Ragep, "New Light on Shams," 238-43.
[21] Pingree, *Astronomical Works of Gregory Chioniades*, 18-21.

between the Byzantines and Iranians during this period. But Leichter, as we have seen, claims that Chioniades may have competently translated the *Zīj al-Sanjarī* from Arabic, which would indicate an improvement in his language skills from his initial work on the *al-Zīj al-ʿAlāʾī*.

During his lifetime, Chioniades was evidently a significant figure in the political and religious interactions between the Byzantine and Īlkhānid realms. Though not an original or creative scholar, his translations played an important role in the transmission of Islamic astronomy to Byzantium and Latin Europe, and they were to influence not only later Byzantine scholars such as George Chrysococces and Theodore Meliteniotes (d. 1393) but scholars in Latin Europe as well.

NEW LIGHT ON SHAMS: THE ISLAMIC SIDE OF ΣΆΜΨ ΠΟΥΧΆΡΗΣ

F. Jamil Ragep

I. *Introduction*

In 1295, a certain Gregory Chioniades[1] of Constantinople traveled to the kingdom of Trebizond, ruled at that time by its emperor John II Komnenos (reigned 1280–1297), from where he would embark upon a momentous journey to the land of the Persians. Chioniades seems to have had a way with rulers, for having found favor with Komnenos, he then traveled to Persia, most likely just after the accession to the Ilkhan throne by Ghazan Khan, who had recently converted to Islam. A generation later, George Chrysococces (fl. 1350), who had also traveled to Trebizond in hopes of learning the astronomy of the Persians, was told the following story by his teacher Manuel:

> . . . in a short while he [i.e. Chioniades] was taught by the Persians, having both consorted with the King, and met with consideration from him. Then he desired to study astronomical matters, but found that they were not taught. For it was the rule with the Persians that all subjects were available to those who wished to study, except astronomy, which was for Persians only. He searched for the cause, which was that a certain ancient opinion prevailed among them, concerning the mathematical sciences, namely, that their king will be overthrown by the Romans, after consulting the practice of astronomy, whose foundation would first be taken from the Persians. He was at a loss as to how he might come to share this wonderful thing. In spite of being wearied, and having much served the Persian king, he had scarcely achieved his objective; when, by Royal command, the teachers were gathered. Soon Chioniades shone in Persia, and was thought worthy of the

[1] An excellent summary of what is known of the life of Chioniades can be found in Joseph Gerard Leichter, "The Zīj as-Sanjarī of Gregory Chioniades: Text, Translation and Greek to Arabic Glossary" (Unpublished Ph.D. Dissertation, Brown University, 2004), 2–6. Cf. L.G. Westerink, "La profession de foi de Gregoire Chioniades," *Revue des études byzantines* 38 (1980): 233–245; and David E. Pingree, "Chioniades, Gregory," in *Oxford Dictionary of Byzantium*, ed. Alexander P. Kazhdan (New York: Oxford University Press, 2002), 422–423. See also Maria Mavroudi, "Exchanges with Arabic Writers during the Late Byzantine Period," in *Byzantium: Faith and Power (1261–1557): Perspectives on Late Byzantine Art and Culture*, ed. Sarah Brooks (New York: Metropolitan Museum of Art, 2007), 62–75.

> King's honor. Having gathered many treasures, and organized many subordinates, he again reached Trebizond, with his many books on the subject of astronomy. He translated these by his own lights, making a noteworthy effort. There are in fact other books of the Persian Syntaxis which he translated, those having certain examples with the years systematically at the beginning. However, he handed on the Syntaxis alone, the best and most accurate of all, as our teacher said, who appeared to be telling the truth. He translated separately the commentary, which was taken from the Persians by word of mouth alone. In this way, the Syntaxis, called the Handy, was produced.[2]

From this account, we can gather that the *Persian Syntaxis* of Chrysococces is somehow based on the work of Chioniades and that the latter went to some city in Persia to obtain the necessary learning and materials. From letters of Chioniades, we know that the city in question was the Mongol capital, Tabriz.[3] Furthermore, in the introduction to his translation of a work that Pingree tells us is related to the *Zīj al-ʿAlāʾī* of ʿAbd al-Karīm al-Fahhād (fl. 1176), we learn that Chioniades studied with a certain Shams Bukharos,[4] about whom the author of a recent article states: "There is nothing known of him in Persian or Arabic sources, nor is there any known reference to him outside the Greek work just mentioned."[5] The purpose of this paper is to try to uncover some information about this elusive Shams, who undertook to teach the Greek Chioniades astronomy and provide him with valuable texts, despite whatever reservations Shams and others in Tabriz may have had. But first we will need to explore the intellectual context of Tabriz in which this transmission took place and the sources of some of the material Chioniades took back with him to Byzantium.

II. *The Tabriz Context*

What was the state of astronomy in and around Tabriz at the end of the thirteenth century? Tabriz was the inheritor of the Marāgha scientific tradition and observatory, which had been established in Azerbaijan after

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2 Raymond Mercier, "The Greek 'Persian Syntaxis' and the *Zīj-i Īlkhānī*," *Archives internationales d'histoire des sciences* 34 (1984): 35–36; reproduced in Leichter, "Zīj as-Sanjarī," 3.

3 Leichter, "Zīj as-Sanjarī," 3.

4 David Pingree, The *Astronomical Works of Gregory Chioniades*, vol. 1: *The Zīj al-ʿAlāʾī* (Amsterdam: J.C. Grieben, 1985), 36–37.

5 Raymond Mercier, "Shams al-Dīn al-Bukhārī," in *The Biographical Encyclopedia of Astronomers*, eds. Thomas Hockey et al. (New York: Springer, 2007), 1047.

the Mongol conquests of the 1250s. The Marāgha Observatory had been built with the active support of the Mongol ruler Hülegü Khan, who made the redoubtable Naṣīr al-Dīn al-Ṭūsī its founding director. Thanks to the work of Aydın Sayılı and excavations carried out at the site, we know quite a bit about this observatory, which, as far as we can determine, was the first large-scale observatory ever built and was to be the model for similar, big-science initiatives in the centuries to come, whether in China, in Central Asia, in India, or in Europe.[6]

It is not clear, however, when the Marāgha observatory ceased functioning as an active scientific institution (as opposed, say, to a tourist attraction that led Tīmūr Lang to take a detour during one of his expeditions in order to show his grandson Ulugh Beg the remains of the observatory).[7] This has considerable significance as we try to reconstruct the chronology of events that led Tabriz to become the major center of global science by the time Chioniades arrived there in 1295.

Now this is what we can reconstruct: From what we gather from the *zīj* (astronomical handbook) of a certain Shams al-Dīn al-Wābkanawī (about whom more later), which was mostly compiled under Öljeytü (r. 703–716/1304–1316), but not completed until sometime during the reign of Abū Saʿīd Bahadur Khan (r. 716–736/1316–1335), the Marāgha observatory seems to have ceased operations a few years (exactly how many being unclear) after the death in 1274 of Naṣīr al-Dīn al-Ṭūsī. According to Wābkanawī, the *zīj*es of Muḥyī al-Dīn ibn Abī al-Shukr al-Maghribī used the Marāgha observations, which Ṭūsī, for whatever reasons, had not been able to incorporate into the *Īlkhānī Zīj* (completed sometime in the late 1260s). Now since Maghribī died in Marāgha in June 1283, and we have no firm indications of observations or activity at the Marāgha observatory after that date, it seems likely that we can take 1283 as the *terminus ad quem*. And Wābkanawī makes it clear that the Marāgha observatory did

[6] Aydın Sayılı, *The Observatory in Islam and Its Place in the General History of the Observatory* (Ankara: Türk Tarih Kurumu Basımevi, 1960); and Parvīz Varjāvand, *Kāvish-i raṣadkhāna-i Marāgha* (Tehran: Amīr Kabīr, 1366 H.Sh [1987 CE]).

[7] This is mentioned in a letter by the eminent mathematician Jamshīd al-Kāshī, who was a member of Ulugh Beg's scientific entourage; see Edward S. Kennedy, "A Letter of Jamshīd al-Kāshī to His Father: Scientific Research and Personalities at a Fifteenth Century Court," *Orientalia* 29 (1960): 196, 208–209 (reprinted in E.S. Kennedy et al., *Studies in the Islamic Exact Sciences*, eds. David A. King and Mary Helen Kennedy (Beirut: American University of Beirut, 1983), 722–744).

not reach its goal of a 30-year observational period, which would have ended around 1289.[8]

This dating has implications for what scientific activity Chioniades may have found when he came to Azerbaijan in 1295. Given the testimony of Wābkanawī, it seems that the Marāgha observatory was no longer an ongoing concern. But we know from Rashīd al-Dīn that Ghazan Khan visited the Marāgha observatory on numerous occasions, and in particular in the spring of 1300 when returning from an expedition to Syria. He is said to have shown great interest in the observatory, asked many questions and then ordered his own observatory to be built in the extensive complex of Abwāb al-Birr in Sham, a suburb of Tabriz.[9] But let us consider the dates. If there was no functioning Marāgha observatory in 1295, and the Tabriz observatory lay in the future, what was it that brought Chioniades to Tabriz? Here, I think, we can safely guess that Tabriz, under Ghazan or before, had gained a justified reputation as a major center of scientific, and in particular astronomical, learning and research even without an observatory.

Although this period of the history of science in Islam has been somewhat downplayed (being in the shadow of the so-called Marāgha school), there is accumulating evidence that the time in which Chioniades visited Tabriz was one of intense activity. We know, for example, that Quṭb al-Dīn al-Shīrāzī arrived in Tabriz sometime in 1290 (or shortly thereafter) after serving as a Mongol emissary in Egypt and as chief judge in Malaṭya and Sivas in Anatolia, where he wrote several major works on astronomy.[10] It is in Tabriz that he most likely wrote his *Faʿalta fa-lā talum* ("You have done it so don't impugn!"), one of the most remarkable works in the entire history of Islamic science. In it he lambasts a certain al-Ḥimādhī, who had dared criticize him and, adding salt to the wound, had allegedly plagiarized large chunks of Shīrāzī's *al-Tuḥfa al-shāhiyya*, an astronomical

[8] Shams al-Dīn al-Wābkanawī, *al-Zīj al-muḥaqqaq al-sulṭānī ʿalā uṣūl al-raṣad al-Īlkhānī*, Istanbul, Süleymaniye Library, Ayasofya MS 2694, ff. 2a, 3a. On Maghribī, see Mercè Comes, "Ibn Abī al-Shukr," in *The Biographical Encyclopedia of Astronomers*, eds. Thomas Hockey et al. (New York: Springer, 2007), 548–549. On his astronomical observations, see George Saliba, *A History of Arabic Astronomy: Planetary Theories during the Golden Age of Islam* (New York: New York University Press, 1994), 163–176, 177–186, 208–230. Cf. Sayılı, *The Observatory in Islam*, 204, 211–218.

[9] Sayılı, The Observatory in Islam, 227.

[10] On Shīrāzī, see F. Jamil Ragep, "Shīrāzī," in *The Biographical Encyclopedia of Astronomers*, eds. Thomas Hockey et al. (New York: Springer, 2007), 1054–1055.

work completed in Sivas in 1285. In the introduction, Shīrāzī mentions several individuals who formed, it seems, part of an extensive network of scientists centered in Tabriz. This included Shams al-Dīn (or perhaps Jalāl al-Dīn) al-ʿUbaydī, Jamāl al-Dīn al-Turkistānī, and Kamāl al-Dīn al-Fārisī, not to mention the hapless al-Ḥimādhī.[11] And Ghazan Khan, we are told by Rashīd al-Dīn, was something of an astronomer himself.[12] We also know that others would later be attracted to Tabriz, among whom was Niẓām al-Dīn al-Nīsābūrī, who arrived sometime between 1304 and 1306.[13]

So in putting the pieces together, we come up with the following. Chioniades arrives in Tabriz in 1295, attracted both by the resurgence in Azerbaijan of the study of astronomy, which he longed to master, and the sympathetic attitude of the early Ilkhanids toward Christians. But even with Ghazan's ascension and conversion to Islam, Chioniades seems to have been well received in the court, which prided itself on its cosmopolitanism. Indeed Rashīd al-Dīn remarks: "There were gathered under the eyes of the pādishāh of Islam philosophers, astronomers, scholars, historians, of all religions, of all sects, people of Cathay, of Machin (South China), of India, of Kashmir, of Tibet, of the Uyghur, and other Turkish nations, Arabs and Franks."[14] And there is some evidence that Rashīd al-Dīn himself wrote answers to questions posed by Chioniades on difficult physical and theological matters, which were then translated into Greek.[15] And he seems to have been assigned, after some initial hesitation, to a tutor who undertook to allow Chioniades to gain the astronomy of his ancient Greek forebears, though admittedly, as we shall see, with a heavy dose of Islamic coloring.

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11 Quṭb al-Dīn al-Shīrāzī, *Faʿalta fa-lā talum*, Tehran, Majlis-i Shūrā MS 3944, ff. 5b, 7b, 9a.

12 Sayılı, The Observatory in Islam, 227–229.

13 On Nīsābūrī, see Robert G. Morrison, *Islam and Science: The Intellectual Career of Niẓām Al-Dīn Al-Nīsābūrī* (London; New York: Routledge, 2007).

14 Sayılı, The Observatory in Islam, 230.

15 Zeki Velidi Togan, "İlhanlı Bizans kültür münasebetlerine dair vesikalar" ("A Document concerning Cultural Relation between the İlkhanide and Byzantiens" [sic]), *İslâm Tetkikleri Enstitüsü Dergisi* 3 (1959–60): 315–378 (= 1–39). I owe this reference to Dimitri Gutas, "Arabic into Byzantine Greek: Introducing a Survey of the Translations," in *Knotenpunkt Byzanz: Wissensformen und kulturelle Wechselbeziehungen*, eds. Andreas Speer and Philipp Steinkrüger (Berlin: De Gruyter, 2012), 258.

III. *Chioniades as Transmitter of Islamic Astronomy*

Chioniades returned to Trebizond in the late 1290s and was in Constantinople by April 1302. There he translated, presumably from Persian into Greek, a set of recipes for antidotes as well as a number of astronomical treatises, and wrote a confession of faith, evidently to counter accusations of heresy accruing from his work in astrology and his years among the Persians. Apparently sufficiently rehabilitated, he was appointed Bishop of Tabriz in 1305 and took the name Gregory, but he may not have returned to Tabriz until about 1310. By 1315, he was again in Trebizond, where he lived as a monk until his death around 1320.[16]

What did Chioniades gain from his time in Tabriz? Thanks to the work of Otto Neugebauer, David Pingree and others, we know that Chioniades obtained access to several astronomical works and translated (or reworked them) into Greek.[17] These included:[18]

1) *al-Zīj al-ʿAlāʾī* of ʿAbd al-Karīm al-Shīrwānī al-Fahhād (ca. 1150), via a Persian version made by Shams al-Dīn (according to Pingree).[19]
2) An abridged version of *al-Zīj al-Sanjarī* of ʿAbd al-Raḥmān al-Khāzinī (ca. 1120), a Greek freedman of a judge in Marv; made after 1) and directly from the Arabic (according to Leichter).[20]
3) The *Īlkhānī Zīj* of Naṣīr al-Dīn al-Ṭūsī.
4) A short *Syntaxis*, perhaps by Shams al-Dīn al-Bukhārī.
5) A longer *Revised Canons*, again perhaps by Shams al-Dīn al-Bukhārī. (Pingree takes this to be by Chioniades, who, he claims, was attempting to show his competence in using the tables of *al-Zīj al-ʿAlāʾī*.)[21]

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16 Leichter, "Zīj as-Sanjarī," 3–6; Pingree, "Chioniades," 422–423.

17 A by now classic work on the subject is David Pingree, "Gregory Chioniades and Palaeologan Astronomy," *Dumbarton Oaks Papers* 18 (1964): 133–160. Pingree amplifies his findings in his *Astronomical Works of Gregory Chioniades* and in his "In Defence of Gregory Chioniades," *Archives internationales d'histoire des sciences* 35 (1985): 436–438.

18 All or some of these works are preserved in Vaticanus Graecus MS 211 (Rome), Vaticanus Graecus MS 1058 (Rome), and Laurentianus MS 28, 17 (Florence). Convenient listings (complete) are in Pingree, *Astronomical Works of Gregory Chioniades*, 23–28, and Leichter, "Zīj as-Sanjarī," 12–13 (partial, highlighting the works attributable to Chioniades).

19 Edition and translation in Pingree, *Astronomical Works of Gregory Chioniades*, 36–243.

20 Edition and translation in Leichter, "Zīj as-Sanjarī," 19–162, 367–567.

21 Pingree, *Astronomical Works of Gregory Chioniades*, 21–22; edition and translation, 260–333. The work is a report by Chioniades, but it seems to be based on observations and calculations made by Shams al-Dīn.

6) A work called *Schemata of the Stars* (Περὶ τῶν σχημάτων τῶν ἀστέρων).[22]
7) A work on the astrolabe by Shams al-Dīn.
8) *On the Genethlialogial Computation*, probably by Shams al-Dīn, which concerns the horoscope of a certain Fakhr al-Dīn born in Tabriz on 25 August 1268.[23]

As for the first 3 *zīj*es (astronomical handbooks with tables), one is struck by the fact that all were considerably out of date by the 1290s. The *zīj*es of Fahhād and Khāzinī had certainly been superseded by the *Īlkhānī Zīj*, which itself had been made obsolete by the *zīj*es of al-Maghribī, which, unlike Ṭūsī's *Īlkhānī Zīj*, incorporated the latest observations made at Marāgha.[24] Was this because Shams al-Dīn was withholding the latest findings from a potential Rūmī adversary (as implied by Chrysococces) or was this simply a matter of Chioniades needing to learn the more elementary material before embarking on cutting-edge research? Pingree notes that when translating *al-Zīj al-ʿAlāʾī*, Chioniades shows a remarkable degree of ignorance, often transcribing Persian words into Greek when he didn't understand the content.[25] But Joseph Leichter (the editor and translator of the Greek version of the *Sanjarī Zīj*) has noted an improvement in Chioniades's knowledge, this time presumably in Arabic, when translating the *Sanjarī zīj*.[26] Of considerable importance in determining how far along Chioniades got in his apprenticeship into Islamic astronomy is whether the purported works of Shams al-Dīn (the short *Syntaxis* and the longer *Revised Canon*), which are found in Greek translation in some of the manuscripts, contain any of the newer material from the Marāgha and Tabriz observations and whether the Persian *Syntaxis* of Chrysococces, which he says comes from the work of Chioniades, contains this new material. Raymond Mercier has claimed, somewhat unconvincingly, that the Persian *Syntaxis* of Chrysococces was mostly derived from the *Īlkhānī Zīj*, but this was disputed by Pingree, who held that there is substantial evidence that Chrysococces used the *ʿAlāʾī* and *Sanjarī zīj*es,

[22] Edition and translation in E.A. Paschos and P. Sotiroudis, *The Schemata of the Stars: Byzantine Astronomy from A.D. 1300* (Singapore; River Edge, NJ: World Scientific, 1998), 26–53.
[23] Edition and translation in Pingree, *Astronomical Works of Gregory Chioniades*, 242–259.
[24] See Saliba, *A History of Arabic Astronomy*.
[25] Pingree, *Astronomical Works of Gregory Chioniades*, 18–21.
[26] Leichter, "Zīj as-Sanjarī," 11–12.

in addition to the *Īlkhānī Zīj*, all of which were translated by Chioniades.[27] But neither seems to have considered that Chrysococces, and Chioniades himself, may have used sources and observations post-dating the *Īlkhānī Zīj*, whether from someone like Maghribī or from Shams al-Dīn himself. A fresh examination of the works attributed to Shams al-Dīn, along with a comparison of contemporaneous works in Arabic and Persian, is necessary in order to resolve some of these issues.

We can gain some additional insight into the question of what Chioniades learned in Tabriz from the examination of another of the treatises listed above, namely no. 6. This work has been dubbed "The Schemata of the Stars" and also an *ʿilm al-hayʾa* text, i.e. a work of theoretical astronomy that seeks to provide a cosmography (or *hayʾa*) of the Universe.[28] These works are well known to us in Islamic sources, and include the twelfth-century texts of al-Kharaqī, several writings by Sharaf al-Dīn Maḥmūd al-Jaghmīnī, Naṣīr al-Dīn al-Ṭūsi and Quṭb al-Dīn al-Shīrāzī from the thirteenth century, and numerous commentaries and supercommentaries on these works, as well as original compilations, in the following centuries.[29] But compared to a true *hayʾa* work, this *Schemata* is rather curious. For starters, it is quite short in comparison with Islamic works of this genre: in its extant three witnesses, it occupies about ten folios (only six in one Vatican witness). In comparison, Ṭūsī's *al-Tadhkira fī ʿilm al-hayʾa* averages about 70–80 folios, while Shīrāzī's ponderous tomes can be over two hundred!

The authors of a recent edition and translation of this work, E.A. Paschos and P. Sotiroudis, have insisted that it represents a completely independent work by a Byzantine author (they presume Chioniades) who has adapted and improved material from Islamic sources.[30] On the other hand, most other recent scholars who have discussed this work have assumed that it derives from Naṣīr al-Dīn al-Ṭūsī's *Tadhkira*.[31] Much of the material in the *Schemata* follows, more or less, material that can be found in the *Tadhkira*, and the *Schemata*'s model for the moon implicitly employs a Ṭūsī-couple,

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27 See Raymond Mercier, "The Greek 'Persian Syntaxis'," 35–60. Pingree responded to Mercier in his "In Defence of Gregory Chioniades."

28 Paschos and Sotiroudis refer to it as *The Schemata of the Stars*; Pingree and Leichter call it a *hayʾa* text in their listing of works due to Chioniades.

29 On the *hayʾa* tradition in Islam, see F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy* (New York: Springer-Verlag, 1993), 1: 24–53.

30 Paschos and Sotiroudis, *The Schemata*, 17.

31 N.M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus* (New York: Springer-Verlag, 1984), 1: 47–48.

a device invented by Naṣīr al-Dīn that produces straight-line oscillation from two interconnected rotating circles or spheres.[32] And in one manuscript (Vaticanus Graecus MS 211), there are diagrams of the Ṭūsī-couple and Ṭūsī's lunar model (ff. 116–117). But as I said, the resemblance is more or less. There are many odd differences between the *Schemata* and the *Tadhkira*: for example, the former has a complete list of constellations with the numbers of stars in each constellation, which is not given in the *Tadhkira*. Now one might think that this was an addition by Chioniades based on Ptolemy's *Almagest*, to which he presumably had access in the original. But there are a number of clues that point to a different source. For example, the constellation names are in several cases taken from Arabic, which themselves, of course, were translations and adaptations of the original Greek. A rather striking example of how a corrupt Arabic form could displace the original Greek is given by the northern constellation Cepheus (Κηφεύς). Now in most Arabic and Persian texts, one finds this mistakenly transcribed as *qayqāwus* (قيقاوس) rather than (قيفاوس), presumably reflecting some scribal error that occurred in the transmission of the translations of Ptolemy's *Almagest* from the 9th century. What is striking is that Chioniades, a native Greek, dutifully lists this as κακκαοῦς, seemingly unaware that this is actually a mistranscription of the Greek Κηφεύς. (A number of other examples could be given, e.g. Βοώτης is called ἀουάς, reflecting the Arabic [العوّاء]).[33] It is clear then that Chioniades must be using an Islamic source for his listing of constellations, since an original Greek source is obviously excluded.[34] There are other indications that the *Schemata* is based on sources other than the *Tadhkira*. In his section on the sun, Chioniades very idiosyncratically opts for a deferent and epicycle model,[35] which is contrary to the choice of eccentric model used by Ptolemy, Ṭūsī and almost everyone else. Why he did so is not clear though a discussion of such a model is given by Ṭūsī as well as by Quṭb al-Dīn al-Shīrāzī.[36]

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32 On the Ṭūsī couple, see Ragep, *Naṣīr al-Dīn*, 2: 427–457.

33 Paschos and Sotiroudis, *The Schemata*, 32. For a listing of these constellations in an Arabic *hay'a* text, see Ragep, *Naṣīr al-Dīn*, 1: 129 and 2: 411 for a brief discussion.

34 The *Schemata* also gives a different number for stars associated with some constellations from what one finds in the *Almagest*; see example 2) below dealing with Ursa Major.

35 Paschos and Sotiroudis, *The Schemata*, 38–43.

36 Ragep, *Naṣīr al-Dīn*, 1: 144–145; Shīrāzī, *Nihāyat al-idrāk fī dirāyat al-aflāk*, Istanbul, Ahmet III MS 3333, f. 68a–b. Shīrāzī indicates that some astronomers had chosen an epicycle model for the sun, but it is not clear to whom he is referring.

Finally there is the case of Ṭūsī's famous lunar model, which incorporated his Ṭūsī couple. There are significant differences in the *Schemata* with the model presented in the *Tadhkira*, most strikingly that the deferent (*ḥāmil*) of the *Tadhkira*, in which the Ṭūsī-couple device is placed, has been replaced by an inclined orb that incorporates the motions of the deferent and inclined orbs of the *Tadhkira* models. Furthermore, from the diagrams found in at least one manuscript of the *Schemata*, one can see that the couple is rotating in the opposite sense from that in diagrams found in manuscripts of the *Tadhkira*.

I was initially inclined to think that this was an adaptation by Shams al-Bukhārī, who may have been influenced by some of the new models presented by Quṭb al-Dīn al-Shīrāzī in his work. In any event, I had assumed that the *Schemata* was somehow based upon a newer, more up-to-date *hay'a* work that had been produced after Ṭūsī's death. But following up on a suggestion by S. Ragep, I discovered, much to my surprise, that the *Schemata* is mostly a translation of fragments from another work by Ṭūsī, namely the *Risāla-yi Muʿīniyya*, which he wrote in 1235, when at the Ismaʿili court in Qūhistān, long before the coming of the Ilkhanids and the writing of the *Tadhkira*.[37] A few examples should suffice to establish this, at least in a preliminary way:

1. From *Risāla-yi Muʿīniyya*, Part I, Chapter 2:[38]

> A body is either simple or composite. A simple is that which is not made up of bodies of different natures or forms. A composite is the opposite. Necessarily composites are composed of simples. Simples are of two types: celestial and elemental. The celestials are all the orbs and stars. The elementals are those fourfold substances that are the basis of the world of generation and corruption, i.e., fire, air, water and earth. The composites are of four types: (a) that whose composition is not complete, such as clouds, wind, shooting stars and the like. These are called upper phenomena;

[37] On the *Risāla-yi Muʿīniyya* and its appendix, the *Ḥall-i mushkilāt-i Muʿīniyya*, see Ragep, *Naṣīr al-Dīn*, 1: 65–70; *idem*, "The Persian Context of the Ṭūsī Couple," in *Naṣīr al-Dīn al-Ṭūsī: Philosophe et Savant du XIIIe Siècle*, eds. N. Pourjavady and Ž. Vesel (Tehran: Institut français de recherche en Iran/Presses universitaires d'Iran, 2000), 113–130 ; and *idem*, "The Origins of the Ṭūsī Couple Revisited," forthcoming in a volume of conference essays devoted to Naṣīr al-Dīn al-Ṭūsī, to be published by Mīrāth-i Maktūb (Tehran). Wheeler Thackston and I are in the process of completing an edition and translation of the *Risāla-yi Muʿīniyya* and *Ḥall-i mushkilāt-i Muʿīniyya*, which should appear in 2014.

[38] Naṣīr al-Dīn al-Ṭūsī, *Risāla-yi Muʿīniyya*, facsimile of Tehran, Malik MS 3503 with an introduction by Muḥammad Taqī Dānish-Pazhūh (Tehran: Intishārāt-i Dānishgāh-i Tihrān (no. 300 in the series), 1335 H.Sh./1956–7 A.D.), 8; translation due to Wheeler Thackston, Sergei Tourkin, and Jamil Ragep.

> (b) that whose composition is complete, i.e., it can remain for a period of time and have the capacity to retain its shape or form, but it is not subject to growth. This is called mineral; (c) that whose composition is complete but nonetheless has the capacity to grow. This is called vegetal; (d) that which has the capacity for growth and the capacity for perception and voluntary movement. This is called animal. The latter three types are called the three engendered [kingdoms]: the fourfold elements are the mothers of these engendered, and the celestial bodies are the fathers. The elements and composites are called lower bodies, and the orbs and stars are called the upper bodies.

From *The Schemata of the Stars* (introduction):[39]

> The [celestial] body is divided into two [entities], simple and composite, as is the case with the four elements, simple and composite; each of them is thus called simple element. It became evident from what we know and comprehend that the sky is circular. On the other hand, the elements are four: fire, air, water and earth; if something is composite then it is none of these.
>
> The entities beyond the elements are classified into two groups: one group where the mixing is not perfect, so that when mixing takes place the composition does not survive [for a long time]; examples are air and clouds and thunderbolts. The other group is the one in which mixing is perfect; when mixing takes place, the composition lasts for a long time. There are three such things; first the one which is produced and cannot develop any further, as is the case with metals; second the composed [substance] has the capacity for growth, as is the case with plants; and third, the one which has the capacity for both growth and movement, as is the case with animals. These three are called children of three structures, and this because the four elements are called their mother. On the other hand, the sphere and the stars are known as their father.

Although the Greek is not a perfect match for the Persian,[40] it is clear that it follows it to a great extent. And in particular, one should note the striking metaphor of the four elements being the mothers of the engendered, while the celestial bodies are the fathers. This is something I have not encountered in other *hay'a* works, including those of Ṭūsī.

2. The listing and names of the constellations, as well as the number of stars in *The Schemata of the Stars*, follows almost exactly what we find

[39] Paschos and Sotiroudis, *The Schemata*, 27.

[40] It should be noted that the translation from the Greek is problematic and needs to be revised based on a better understanding of the concepts being presented. Hopefully this will be done in a future publication.

in the *Muʿīniyya*.[41] For example, in both the *Schemata* and the *Muʿīniyya*, Ursa Major is listed as having 27 stars with 7 lying outside the constellation. On the other hand, the *Tadhkira* simply lists Ursa Major, as well as the other constellations, without providing the number of stars, while in both Shīrāzī's *Nihāya* and his *al-Tuḥfa al-shāhiyya*, Ursa Major has 27 stars with 8 lying outside.[42] This is what one also finds in the *Almagest*.[43]

3. The most decisive, and interesting, piece of evidence establishing the relation of the *Schemata* and the *Muʿīniyya* comes from the lunar model presented in the former. Chioniades lists 6 orbs, which differ both in number and content from the *Tadhkira*, where Ṭūsī lists 7 orbs for his non-Ptolemaic lunar model. Furthermore, the *Schemata* gives 13°11′/day for the motion of the second orb, while in the *Tadhkira* the equivalent motion, resulting from the combination of the inclined and deferent orbs, comes to 13°14′. On the other hand, in the Appendix (*Dhayl* or *Ḥall*) of the *Muʿīniyya*, the lunar model given has the same 6 orbs as in the *Schemata* and the second orb also moves at 13°11′/day.[44]

From these 3 examples, which could be supplemented by quite a few others, one may conclude that Chioniades learned theoretical astronomy (*ʿilm al-hayʾa*) from the *Risāla-yi Muʿīniyya* and its Appendix. What is remarkable about this is that when Chioniades was in Tabriz in the 1290s, the Persian *Muʿīniyya* and its Appendix, completed in 1235 and 1245, respectively, would have long since been superseded by the Arabic *Tadhkira*, written in 1261 and containing Ṭūsī's revisions and corrections to his earlier works. And any competent astronomer in Azerbaijan in 1295 would have known this. Why then did Chioniades's teacher, presumably Shams al-Dīn al-Bukhārī, use the *Muʿīniyya* and its Appendix to teach him theoretical astronomy? One obvious reason that presents itself is that Chioniades was more comfortable dealing with a Persian text rather than an Arabic one. And Pingree has claimed that *al-Zīj al-ʿAlāʾī*, originally

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41 Paschos and Sotiroudis, *The Schemata*, 30–37; al-Ṭūsī, *Risāla-yi Muʿīniyya*, 19–21.

42 Ragep, *Naṣīr al-Dīn*, 1: 128–129; Shīrāzī, *Nihāyat al-idrāk*, f. 58b; Shīrāzī, *al-Tuḥfa al-shāhiyya*, Istanbul, Süleymaniye Library, Turhan Valide Sultan MS 220, f. 23b.

43 Gerald J. Toomer, *Ptolemy's Almagest*, translated and annotated by G.J. Toomer (New York: Springer-Verlag, 1984), 342–343.

44 Paschos and Sotiroudis, *The Schemata*, 42–45. For a listing of the parameters for the lunar model in the *Tadhkira*, see Ragep, *Naṣīr al-Dīn*, 2: 457. The sum of the lunar inclined and deferent orbs comes to 13°14′ (24°23′/day–11°9′/day) in the *Tadhkira*; cf. the *Ḥall*, where the equivalent motion of the inclined orb is given as the mean motion of the moon (*wasaṭ-i qamar*), i.e. 13°11′ (Naṣīr al-Dīn al-Ṭūsī, *Ḥall-i mushkilāt-i Muʿīniyya*, facsimile of Tehran, Malik MS 3503 with an introduction by Muḥammad Taqī Dānish-Pazhūh [Tehran: Intishārāt-i Dānishgāh-i Tihrān (no. 304 in the series), 1335 H.Sh./1956–7 AD], 11).

written in Arabic, was translated by Shams al-Dīn into Persian, presumably for the benefit of his student, and that teaching was done in Persian.[45] The inescapable conclusion is that Chioniades felt much more comfortable in Persian than in Arabic;[46] and this may well have reflected the Byzantine predilection when dealing, in whatever field of endeavor, with their Muslim neighbors to the east. That Shams Bukharos seems to have been happy to accommodate him reveals one aspect of their relationship; but that he felt little need to provide him with the most up-to-date astronomical information is another.

IV. *The Elusive Shams*

It would certainly help in understanding this relationship if we knew more about this elusive Shams Bukharos. As recently as 6 years ago, as we have seen, a biography of Shams al-Dīn al-Bukhārī stated "There is nothing known of him in Persian or Arabic sources . . ."[47] But since then, a researcher in Iran[48] and our group at McGill, working independently, have concluded that this Shams al-Dīn al-Bukhārī is the same individual known as Shams al-Dīn Muḥammad ibn ʿAlī Khwāja al-Wābkanawī al-Munajjim, who is best known for a *zīj* entitled *al-Zīj al-muḥaqqaq al-sulṭānī ʿalā uṣūl al-raṣad al-Īlkhānī* (The verified *zīj* for the sultan based on the principles of the Īlkhānī observations), a work that, as mentioned above, was mostly completed during the reign of Sulṭān Öljeytü (r. 703–716/1304–1316) but was dedicated to his son and successor Abū Saʿīd (r. 716–736/1316–1335).[49] Now the village of Wābkana (or Wābakna), the basis for his *nisba*, is only 20 km from Bukhara, so two Shams al-Dīn's from the Bukhara region working at the Mongol court as astronomers seems unlikely. And it was not uncommon to have two *nisba*s, one from one's own village and another from the region. This Wābkanawī is also the author of a treatise on the astrolabe, *Kitāb-i Maʿrifat-i usṭurlāb-i shamālī* (On the northern astrolabe) [in Persian] that seems to be the source of the Greek work on the astrolabe

45 Pingree, *Astronomical Works of Gregory Chioniades*, 18.

46 But as we mentioned above, Leichter thinks Chioniades's Arabic had improved by the time he came to translate the Sanjarī *Zīj*.

47 Note 5 above.

48 The researcher is S.M. Muẓaffarī, whose work I have heard of informally; I am not sure whether he has published or will publish his findings.

49 Benno van Dalen, "Wābkanawī," in *The Biographical Encyclopedia of Astronomers*, eds. Thomas Hockey et al. (New York: Springer, 2007), 1187–1188.

(mentioned above) attributed to Shams al-Dīn.[50] Now if we can conclusively make this identification, we would also know that this Wābkanawī was born on 11 June 1254, based on one of the Greek sources.[51] Wābkanawī also provides evidence of continuity between the Marāgha Observatory and astronomical research in Tabriz. One of his earliest observations dates from the year 684/1285; he also uses the calendar introduced during the reign of Ghazan Khan and which was called the Khānī calendar.[52] Since as we have seen Wābkanawī himself speaks of the Marāgha Observatory as a thing of the past, this would provide evidence that the observational program in Azerbaijan resumed shortly after the death of Maghribī in 1283, but now presumably in Tabriz.

There is another possible identification we can make, this one a bit more speculative. As it turns out, al-Ḥimādhī, the author of the work that Shīrāzī lambasts, is also a Muḥammad b. ʿAlī al-Munajjim.[53] Shīrāzī refrains from mentioning his honorific, which, let us venture to say, might have been Shams al-Dīn; but given all the insults he hurls at him, it is not surprising that no honorific is given.

If this is indeed the same Muḥammad b. ʿAlī as Muḥammad ibn ʿAlī al-Wābkanawī (a.k.a. Shams Bukharos), then it adds a bit more texture to our understanding of the academic infighting that occurred in the Mongol court at this time, infighting that makes some of our contemporary scholarly battles seem quite tame in comparison. For example, Shīrāzī in *Faʿalta* became extremely upset about a claim that Ḥimādhī (allegedly our Shams) made regarding the Ṭūsī-couple. Ḥimādhī said that someone had told him that Shīrāzī's use of the couple to show that there was no resting point for an object thrown straight up was anticipated by Plato. Shīrāzī proudly tells us that he tracked this person down, a certain Shams al-Dīn al-ʿUbaydī, who may also have been Shīrāzī's student, and asked him point blank if that is what he had told Ḥimādhī. *Kidhb*! (a lie) was the inevitable reply from the no doubt cowering ʿUbaydī.[54] Perhaps this might explain why Wābkanawī tells us in *al-Zīj al-sulṭānī* that he had mostly completed it at the time of Öljeytü (r. 1304–1316) but that it was not published until the reign of Abū Saʿīd (r. 1316–1335), at which time Shīrāzī

[50] Our group is currently seeking to verify this; we have recently gained access to the witness preserved in the Topkapı Museum Library.

[51] Pingree, *Astronomical Works of Gregory Chioniades*, 16.

[52] Shams al-Dīn al-Wābkanawī, *al-Zīj al-muḥaqqaq*, Ayasofya MS 2694, ff. 2a, 2b, 3b.

[53] Shīrāzī, *Faʿalta fa-lā talum*, f. 14b.

[54] Shīrāzī, *Faʿalta fa-lā talum*, f. 5a–b.

had been safely dead for several years (since 1311). And Shams/Wābkanawī feels safe enough in his *zīj* to take a swipe at the competing *zīj* of Shīrāzī's student Niẓām al-Dīn al-Nīsābūrī, who had written what Wābkanawī considered an unusable commentary on Ṭūsī's *Īlkhānī Zīj* entitled *Kashf-i ḥaqāʾiq-i Zīj-i Īlkhānī*.[55]

It is tempting to ask at this point whether one source of the tension between Shīrāzī and his circle on the one hand and Shams/Wābkanawī on the other could have been the special treatment accorded Chioniades by Ghazan Khan and Shams's pedagogical role. This is certainly a possibility and highlighting civilizational rivalry makes a good story, especially in these times. But this question raises issues of east-west/Muslim-Christian competition, particularly in scientific matters, to a level that had not been reached, and we are in danger thereby of reading later concerns backwards in time. We can say with certainty that this period of Islamic scientific and intellectual history, during this Mongol interregnum, was a time of enormous creativity, advance and scholarly engagement and debate. No wonder Chioniades would be attracted to Tabriz. But the quest of a single scholar, and his flawed transmission of outdated texts, would not change the stark reality of the sizeable imbalance between Islamic and "western" science at the time. Chioniades had little, if anything, to offer the Persians, and they in turn took little notice of his coming—at least there is little in evidence from the historical record. Nevertheless, he had begun a process, one that would eventually result in the ancient legend coming true: for the "Romans" would indeed overthrow the "Persians," once they had consulted the practice of astronomy, whose foundation would first be taken from the Persians.

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[55] On Nīsābūrī, see Robert Morrison, "Nīsābūrī," in *The Biographical Encyclopedia of Astronomers*, eds. Thomas Hockey et al. (New York: Springer, 2007), 837. The reference to the *Kashf* occurs in Shams al-Dīn al-Wābkanawī, *al-Zīj al-muḥaqqaq*, f. 4a.

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Section III

Ibn al-Shāṭir and Copernicus

Ibn al-Shāṭir and Copernicus: The Uppsala Notes Revisited

F. Jamil Ragep

Abstract

It has long been recognized that Copernicus' models in the *Commentariolus* bear a striking resemblance to those of Ibn al-Shāṭir (14th-c. Damascus). A number of scholars have postulated some sort of transmission but have denied that Ibn al-Shāṭir's geocentric models had anything to do with the heliocentric turn. Rather, the assumption has been that they were used by Copernicus solely to resolve the irregular motions of the planetary deferents brought on by Ptolemy's equant. Based on proposals for direct transformations of Ibn al-Shāṭir's models into those of Copernicus and an alternative reading of Copernicus' so-called Uppsala notes, it is argued here that Ibn al-Shāṭir's models in fact have a "heliocentric bias" that made them particularly suitable as a basis for the heliocentric and "quasi-homocentric" models found in the *Commentariolus*.

Keywords

Ibn al-Shāṭir, Copernicus, *Commentariolus*, *De revolutionibus*, Islamic astronomy, heliocentrism, Averroism, Renaissance astronomy, homocentric astronomy

Introduction

In his classic translation of and commentary on Copernicus' *Commentariolus*,[1] Noel Swerdlow provided a plausible and coherent reconstruction of Copernicus' pathway from Ptolemaic, geocentric planetary models to Copernican, heliocentric ones.[2] Swerdlow hypothesized a conversion of Ptolemy's epicyclic models for the planets into eccentric models, based on propositions found in Regiomontanus' *Epitome of the Almagest*.[3] This, he claimed, was the crucial step in the transformation from geocentric to heliocentric models. This reconstruction was mainly based on an interpretation of the so-called Uppsala notes [U] in Copernicus' hand and the curious use of the word *eccentricitas* found therein. As Swerdlow put it,

Corresponding author:

F. Jamil Ragep, Institute of Islamic Studies, McGill University, Morrice Hall, Room 319, 3485 McTavish Street, Montreal, QC H3A 0E1 Canada.

Email: jamil.ragep[at]mcgill.ca

> The use of the word *eccentricitas* in U for the sine of the maximum equation of the anomaly shows that Copernicus was investigating the eccentric model of the second anomaly. My entire analysis hangs on this one word.[4]

In his discussion, Swerdlow bifurcated Copernicus' handling of the "first" and "second" anomalies, the former having to do with the tropical or sidereal motion, the latter with the synodic. Thus, the irregular motions arising from Ptolemy's equant (falling under the "first anomaly") were, in this view, unrelated to the critical transformations of the second anomaly that led to the eccentric models and whence to heliocentrism.[5] In what follows, I argue that there is an alternative, and simpler, way to reach Copernicus' models in the *Commentariolus* without assuming the intermediate step of eccentric models nor the presumed, bifurcated process. This depends on assuming that (1) when Copernicus uses the word *eccentricitas*, he is not referring to "eccentric models" (as found in Regiomontanus) but rather the amount the Earth is out of center ("eccentric") to the Sun, i.e., the Earth–Sun distance, and (2) Copernicus does not bifurcate the process of the geocentric-heliocentric transformation by dealing with the first and second anomalies separately but rather exploits the peculiar nature of Ibn al-Shāṭir's "heliocentrically biased" models that allows for a more direct transformation.

Relation of Ibn al-Shāṭir's models to the *Commentariolus* models

Ibn al-Shāṭir (1306–1375/6 C.E.), who was a timekeeper at the Umayyad Mosque in Damascus, dispensed with eccentrics in his *Nihāyat al-suʾl* and, more importantly, made the Earth the center of mean motion of his planetary models. This has been known for some time, since the modern examination by E.-S. Kennedy of the *Nihāyat al-suʾl* and the subsequent articles by Kennedy and his students at the American University of Beirut in the 1950s and 1960s.[6] It is almost impossible to discuss Ibn al-Shāṭir's models without mentioning the further discovery, made by Otto Neugebauer, that Ibn al-Shāṭir's models bore significant similarities with those of Copernicus in the *Commentariolus*.[7] These discoveries were used with great effect by Swerdlow and later Swerdlow/Neugebauer when analyzing Copernicus' planetary models.[8]

Among the underappreciated aspects of Ibn al-Shāṭir's models are, somewhat paradoxically, their Aristotelian *and* heliocentric biases. By Aristotelian, I mean their "quasi-homocentricity," whereby all the planetary models have their major deferent orb (the "inclined orb" (*falak māʾil*)) centered and moving uniformly about the Earth; furthermore, as noted, he removed all eccentrics from his system and depended on epicycles to replicate Ptolemy's eccentricities.[9] The "heliocentric bias" is a consequence of this, since it allows a relatively straightforward and direct transformation from Ibn al-Shāṭir's to Copernicus' *Commentariolus* models.[10] This represents a radical departure from previous systems, both Ptolemaic and non-Ptolemaic, as we can see from the following illustration comparing several models (Figure 1).

$\bar{\alpha}$ is the mean motion for each of the models; each of the lines extending from O, D, H, and E represents the main deferent orb for each model. C is Ptolemy's epicycle center, which is approximately, but not exactly, the location of the epicycle center in the other

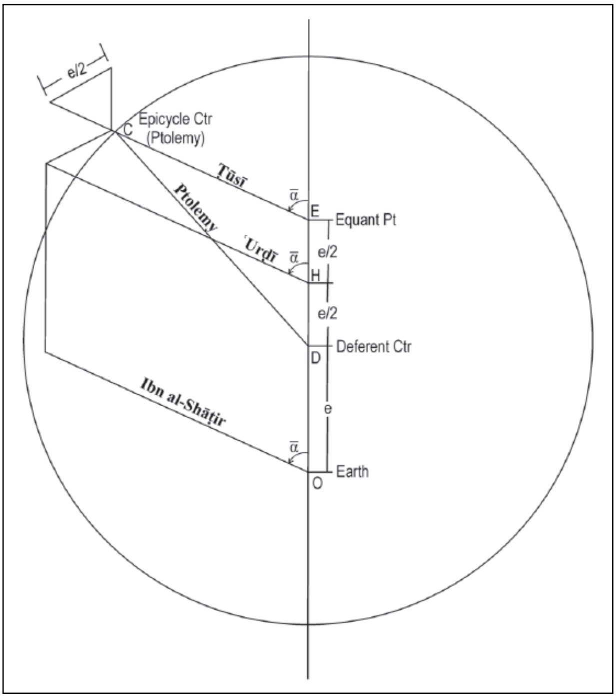


Figure 1. Several models schematically compared.[11]

models; e is the eccentricity. Note that although Ptolemy's mean motion is about the equant point, his main deferent is centered at D, not E. It is this "centering on the Earth" by Ibn al-Shāṭir, I am arguing, that is critical for the transformation to a heliocentric system, at least insofar as Copernicus presents it in the *Commentariolus*.

Let us first consider Ibn al-Shāṭir's model for the outer planets (Figure 2).[12]

The Earth O is at the center of a concentric orb with radius OF, rotating counterclockwise[13] with the mean motion $\bar{\alpha}$. F is then the center of a "large" epicycle FG, rotating clockwise with the mean motion $\bar{\alpha}$, and G is the center of a "small" epicycle GC, rotating counterclockwise with twice the mean motion. C is the center of the Ptolemaic epicycle CP, P being the planet. The two epicycles FG and GC, called by Ibn al-Shāṭir the deferent (*al-ḥāmil*) and dirigent (*al-mudīr*), respectively, rotate uniformly and serve to account for Ptolemy's "first anomaly," brought about by his eccentricities and equant E (the point about which equal "mean" motion occurs in Ptolemy's models). Ibn al-Shāṭir thereby eliminates the irregular motion of Ptolemy's deferent that moves with respect to the equant rather than its own center (see Figure 1). As with Ptolemy's model, the line joining the planet P with the center of the epicycle is coordinated with the motion $\bar{\gamma}$ of the mean Sun $\bar{\odot}$, so that CP is always parallel to the direction of the mean Sun from the Earth.[14]

There are three steps in the proposed transformation to the models of the outer planets in the *Commentariolus* (Figure 3). The first step is to transpose the Ptolemaic epicycle so that its center C is now at C′, which coincides with the center of the World O.

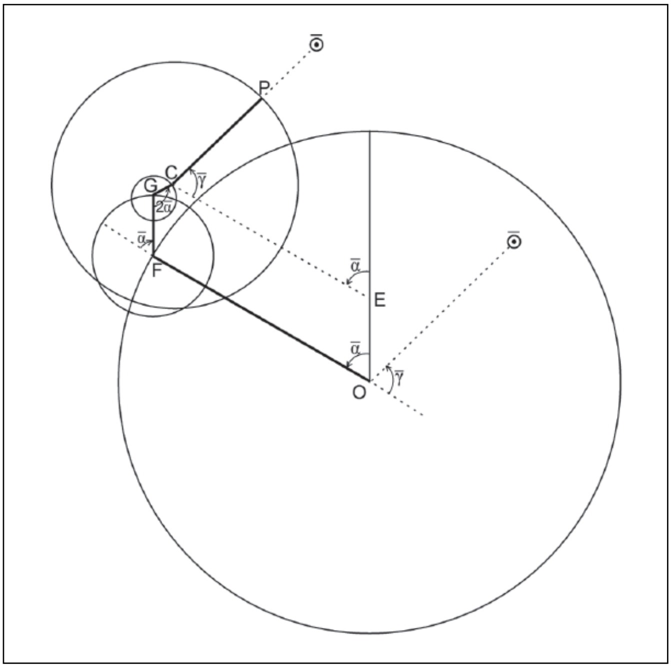


Figure 2. Ibn al-Shāṭir's model for the outer planets.

The second step is to move O and $\bar{\odot}$ along line $\bar{\odot}$O, O to O′ on the circumference of the transposed epicycle, and $\bar{\odot}$ to $\bar{\odot}'$ at the center of the World. Finally, P is moved parallel to $\bar{\odot}$O to P′, coinciding with the Ptolemaic epicycle center C. $\bar{\odot}'$F has become the radius of the new "deferent orb" of the planet, which Copernicus refers to as the "semidyameter orbis." Figure 4 represents the *Commentariolus* model.[15]

From an astronomical standpoint, this transformation is not that difficult to conceive, since, as mentioned, the motion of Ptolemy's epicycle is essentially equal to the motion of the Sun around the Earth. Mathematically, we note that $\overrightarrow{OF}+\overrightarrow{FG}+\overrightarrow{GC}+\overrightarrow{CP}$ (Figure 2) = $\overrightarrow{O'\bar{\odot}'}+\overrightarrow{\bar{\odot}'F}+\overrightarrow{F'G'}+\overrightarrow{G'P'}$ (Figure 4). The point to keep in mind is that such a simple transformation is possible because Ibn al-Shāṭir has placed the Earth at the center of the main deferent OF and dealt with the first anomaly not using eccentrics centered on the apsidal line but rather with the double epicycles external to the apsidal line. In the other models shown in Figure 1, such a direct transformation would not be possible, since one must first transform deferents centered at H and E (for ʿUrḍī and Ṭūsī, respectively) to ones centered on the Earth in order to reach the *Commentariolus* models; in other words, one would need to transform these models into Ibn al-Shāṭir's mathematically equivalent models. For Ptolemy, the situation is even more complicated, since Copernicus would, in addition to everything else, have had to deal with the irregular motion brought on by the equant and then somehow resolve that problem and come up with Ibn al-Shāṭir's models. In any event, at some point, Copernicus borrowed or came up with Ibn al-Shāṭir's models, since that is what is implied, as we shall see, by the Uppsala notes.

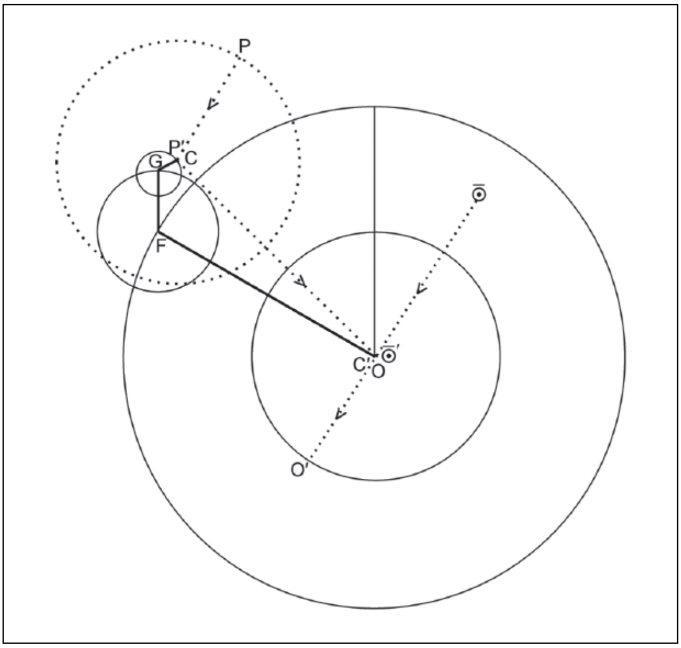


Figure 3. Transformation of Ibn al-Shāṭir's models for the outer planets into the *Commentariolus* models.

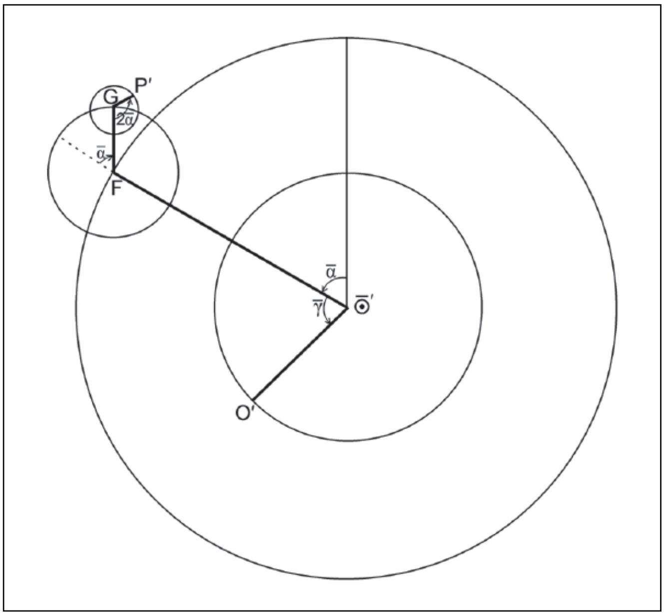


Figure 4. *Commentariolus* model for the outer planets.

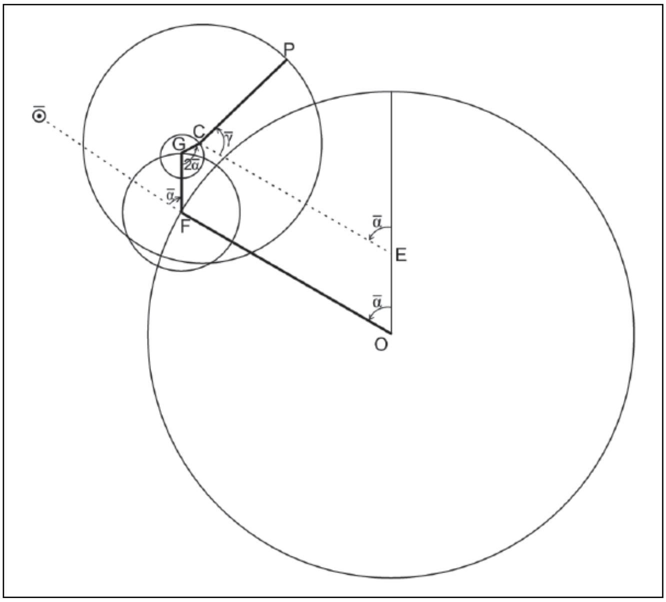


Figure 5. Ibn al-Shāṭir's Venus model.

The advantage of having Ibn al-Shāṭir's models in the transformation to a heliocentric system becomes even clearer when we examine the inner planets. Because Mercury's model is more complex, let us take Venus as our example, since the basic points related to its transformation are equally applicable to Mercury. Ibn al-Shāṭir's version is shown in Figure 5.

As can be seen, this model is essentially the same as that for the outer planets; the major difference is that for Venus (and Mercury), the mean Sun is in the direction OF rather than CP as it was for the outer planets. For the inner planets, the transformation to a heliocentric model is even simpler than for the outer planets; all we need to do is move $\bar{\odot}$ to F and have the Earth O revolve at a fixed distance around a stationary mean Sun, keeping the radii of the other orbs/vectors in the same relative positions. We then have the model in *De revolutionibus* (Figure 6).

This type of simple transformation to the *De rev* model is not possible for Mercury and Venus using the other models depicted in Figure 1. Referring to Figure 7, we note that the mean Sun is on a line from the equant through the epicycle center for the inner planets, since their mean motion is equal to that of the mean Sun and is with respect to the equant point. Moving the mean Sun to the deferent (i.e., the endpoint of the first vector, which for Ptolemy and Ṭūsī would be to the epicycle center C and for ʿUrḍī the point K) would then require a correction to the mean motion to achieve the line of sight from the Earth to the mean Sun (OC or OK). But for Ibn al-Shāṭir's models, there is no correction since the mean motion is with respect to the Earth, so the mean Sun, as we have seen, is on the line OF defined by the mean motion, making the above simple transformation possible.

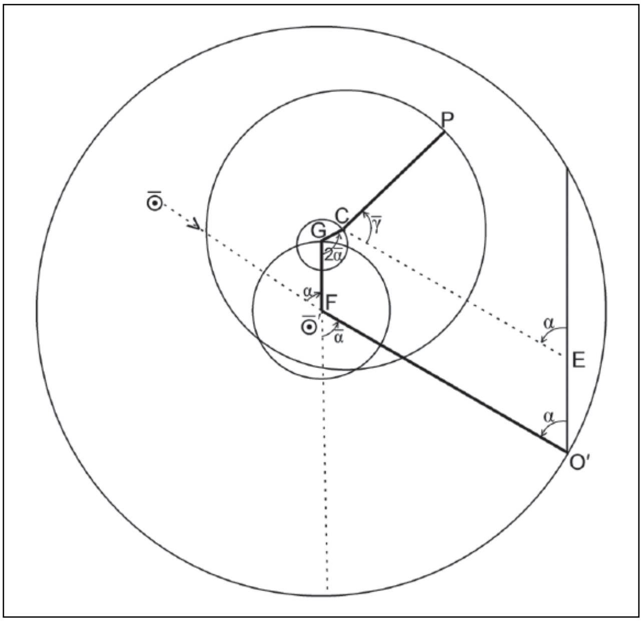


Figure 6. Copernicus' Venus model in *De revolutionibus*.[16]

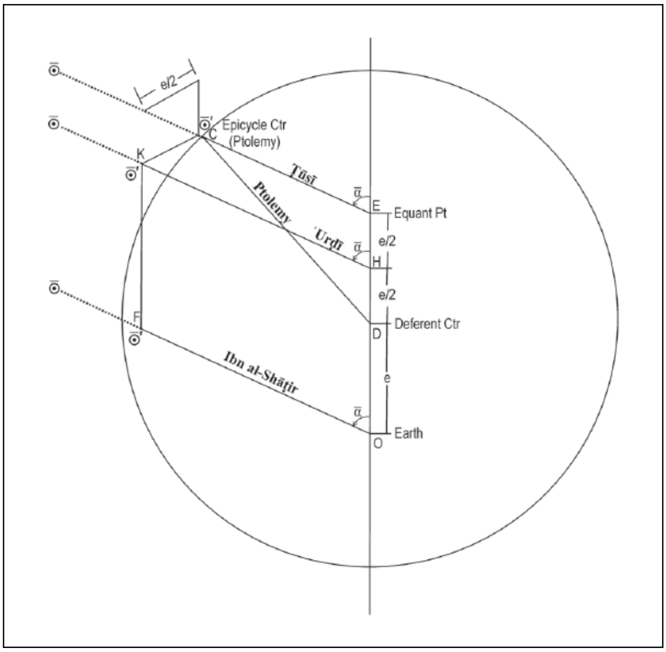


Figure 7. Comparison of Venus models with respect to the mean Sun.

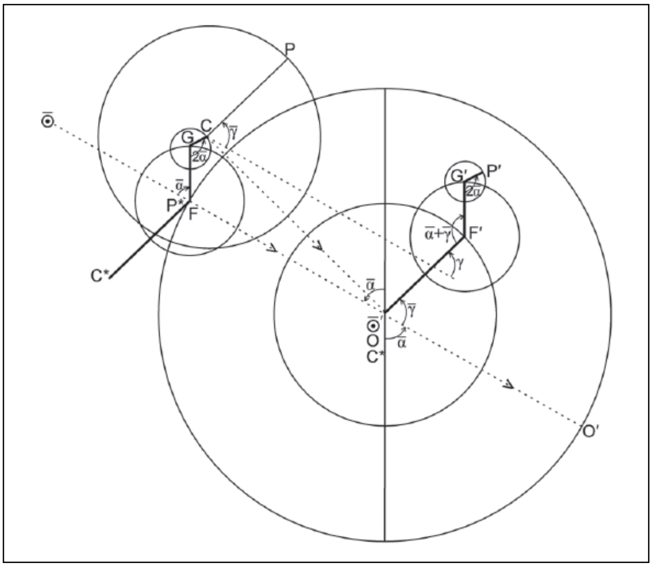


Figure 8. Transformation of Ibn al-Shāṭir's model for Venus into the *Commentariolus* model.

It would be nice if we could end the story here, but Copernicus' *Commentariolus* models for the inner planets are not the same as those in *De rev*. The main difference is that in the *De rev* models, the mean Sun and the center of the planet's orbit (C) are different, whereas they are the same in the *Commentariolus*. Consequently, the transformation from Ibn al-Shātīr's models, while still possible, is more complex as we see in Figure 8.

As with the outer planets, the planetary epicycle with center C is moved so it is now about center O. The mean Sun is also moved to the center, while the Earth is moved along the same line to the circumference of the former deferent OF so it is now at O′. One is left with the problem of where to place the bi-epicyclic device. Since CP is a radius of the epicycle, which is now the main deferent of the planet in the heliocentric model, Copernicus could have reasoned as follows. Move CP, renaming it C*P*, and maintaining size and direction, so that P* coincides with F. Now move C*F, FG, GC, again maintaining size and direction, so that C* coincides with $\bar{\odot}'$. By simple geometry, one can find that O′ moves with a mean motion of $\bar{\alpha}$ in the counterclockwise direction; the mean motion of point F′ is $\bar{\alpha}+\bar{\gamma}$ in the counterclockwise direction; epicycle F′ rotates $\bar{\alpha}+\bar{\gamma}$ clockwise and epicycle G′ moves $2\bar{\alpha}$ counterclockwise. Using vectors, we note that $\overrightarrow{OF}+\overrightarrow{FG}+\overrightarrow{GC}+\overrightarrow{CP}$ (Figure 5) $=\overrightarrow{O'\bar{\odot}'}+\overrightarrow{\bar{\odot}'F'}+\overrightarrow{F'G'}+\overrightarrow{G'P'}$ (Figure 8). A similar transformation is used for Mercury, but here one needs to add a Ṭūsī-couple, just as in Ibn al-Shāṭir's model, in order to vary the size of the planetary deferent/orbit.[17]

Admittedly, these complicated transformations for Mercury and Venus raise numerous questions. If Copernicus had Ibn al-Shāṭir's models when composing the *Commentariolus*, why didn't he make the simple transformation that he later did in *De revolutionibus*? This question becomes particularly acute when we realize that the

Commentariolus models for Mercury and Venus are quite difficult to use for computations as a result of the peculiar arrangement of the orbs resulting from this transformation; in fact, the equation of center can no longer be calculated from the Earth, and the calculation of elongations becomes quite difficult (and perhaps even impossible as far as Copernicus and his contemporaries are concerned).[18] On the other hand, as we have seen, it is indeed possible, with a bit of ingenuity, to transform Ibn al-Shāṭir's models for the inner planets into those in the *Commentariolus* without resorting to the intermediation of Regiomontanus' eccentric alternative. My argument is that when writing the *Commentariolus*, one of Copernicus' priorities was to have models whose main deferents/orbits were centered on the mean Sun even if this made the models less practical for calculation. This is not the case with the *De rev* models, where Copernicus introduced eccentric orbs for his planetary deferents.[19] I will speculate below about the reasons for this insistence in the *Commentariolus* on "homocentric" deferents.

The Uppsala notes

In addition to the fact that one can, as above, make a fairly straightforward transformation of Ibn al-Shāṭir's models to those in the *Commentariolus*, one can also interpret the Uppsala notes as providing evidence that Copernicus transformed Ibn al-Shāṭir's models without an eccentric intermediary. The first thing to note is that the parameters for Ibn al-Shāṭir's models are provided as a set in U; in other words, there is no indication that there are separate transformations for the first and second anomalies. Let us take the specific example of Mars using Ibn al-Shāṭir's values for radii OF and CP, namely, the 60 parts of the Ptolemaic deferent (which Ibn al-Shāṭir calls the "inclined orb") and the 39½ parts for Mars' epicycle. If we norm 60 to 1, then the 39½ becomes .6583. Norming the 1 to 10,000 results in 6583, which is precisely what one finds in the Uppsala notes with the label *Eccentricitas Martis*. As we have seen, Swerdlow takes this to be "the sine of the maximum equation of the anomaly," which it is, but then he makes the further assumption that *eccentricitas* has to do with the eccentric model of the second anomaly, which I question. A simpler explanation is to understand *eccentricitas* literally, and consistently, as the distance of the Earth from the new center, i.e., the mean Sun (Figure 4).[20] This would be the new "off-centeredness" in this transformation of Ibn al-Shāṭir's model.

There is additional evidence in support of this interpretation. In the Uppsala notes, after giving the eccentricities for Mars, Jupiter, Saturn, and Mercury, Copernicus writes, "proportio orbium celestium ad eccentricitatem 25 partium" (the proportion of the celestial orb to an eccentricity of 25 parts). Now what exactly does he mean by eccentricity here? If one interprets this to be the same eccentricity (but with a different norm) as in the earlier part of the notes, then all he is saying is let us find the "proportion" or amount of the celestial orb (i.e., $\bar{\odot}'F$ in Figure 4) if we assign an eccentricity (i.e., an "off-centered-ness" of the Earth) to be 25 rather than, say, 6583 for Mars.[21] And indeed this is exactly what happens in the next line, where $\bar{\odot}'F$ the "semidyameter orbis" is given as 38, which results from the following proportion: $6583/25 = 10{,}000/x \Rightarrow x = 37.98 \approx 38$.

The situation of the inner planets is a bit different and less straightforward. Taking Mercury, since Copernicus does not list Venus in the upper part of U,[22] we find that Copernicus gives the *ecce[ntricitas]* as 2256 (or less likely 2259). But this number is underlined, and in the margin, there is the number 376. Now if Copernicus were to use the same method as with the outer planets, Mercury's epicycle radius of 22.56, divided by 60, would give an *eccentricitas* of .376, which would be 376 normed to 1000.[23] However, neither 2256 nor 376 is the *eccentricitas* if we interpret it as being the Earth–Sun distance. For in order to arrive at the radius of Mercury's "orbis" $\bar{\odot}'F'$ (i.e., 9;24) in the lower part of the Uppsala notes, we must reduce the 1000 (corresponding to $OF=60$ in Ibn al-Shāṭir's model, $\bar{\odot}'O'$ in the *Commentariolus* model) to 25: $1000/25=376/x \Rightarrow x = 9;24 = \bar{\odot}'F'$. Thus, the *eccentricitas* for Mercury, which is the distance between the mean Sun and Earth before that value is normed to 25, is actually 1000, which is implied by the 376 in the margin. Thus, whether one interprets 2256 as the epicycle radius or as the eccentricity in the eccentric model of the second anomaly, in order to arrive at an "orbis" of 9;24 in the lower part of U, one needs to use 1000 as the "eccentricity" implied by "proportio orbium celestium ad eccentricitatem 25 partium." It is interesting that Copernicus chose not to provide an *eccentricitas* for Venus, perhaps because of the confusion regarding exactly what was the *eccentricitas*.

Table 1 provides derivations of all the non-crossed-out numbers in U (excluding the Moon), assuming only that Copernicus had at his disposal Ibn al-Shāṭir's models in some form and that *eccentricitas* refers to the Earth–Sun distance resulting from the above transformations of Ibn al-Shāṭir's models. As mentioned (see Note 23), Copernicus' parameters are from, or derived from, the Alphonsine tables, and, unlike Ibn al-Shāṭir and later in *De rev*, he maintains a strict 3:1 relationship between r_1 (the radius of first epicycle) and r_2 (the radius of second epicycle) for all the planets.

In discussions of the possible influence of Ibn al-Shāṭir on Copernicus, one important counterargument is that their parameters are different. Swerdlow has shown that most of the parameters in U are either directly or indirectly from the Alphonsine Tables; indeed, the "eccentricities" are the sines of the maximum equation of the second anomaly from those tables.[26] This means that not only are the parameters different from those of Ibn al-Shāṭir, it is also clear that they are not taken directly from the *Almagest*. More tellingly, Copernicus adheres to a 3:1 ratio for the bi-epicyclic device for all the planets, whereas Ibn al-Shāṭir does so only for the outer planets.[27] Among other things, this results in an exceedingly bad value for Mercury's maximum equation of center.[28]

But then how do we account for the remarkable similarity between Ibn al-Shāṭir's models and those in the *Commentariolus*? One possibility is that Copernicus does not have the text of *Nihāyat al-suʾl*, or has the text and can't read it, but does have the diagrams. In support of this, let us look a bit more closely at the Mercury model and some of its parameters in U.

Copernicus' Mercury model has been a challenge to researchers, inasmuch as he talks rather cryptically about the orbit being smaller when the Earth is at 0° and 180°, while it is larger when the Earth is at quadratures.[29] Let us examine Ibn al-Shāṭir's diagram for Mercury (Figure 9), which evidently illustrates Copernicus' meaning.

As can be seen, Ibn al-Shāṭir shows the effect of the Ṭūsī-couple (the two, small intersecting circles in one of which Mercury is embedded) by indicating a "True Epicycle Orb" and an "Apparent Epicycle Orb," the latter resulting from the couple moving the planet in a straight line toward and away from the center. At 0° and 180°, the apparent epicycle becomes smaller (Mercury "traversing a far smaller circumference" according

Table 1.

	Mercury	Venus	Mars	Jupiter	Saturn
Eccentricitas $\bar{\odot}$O	{1000 = $\bar{\odot}$O = 6000/6} { $\bar{\odot}$F=}**376** {=**2256**/6} {r = 22.56}	{1000 = $\bar{\odot}$O = 6000/6} {$\bar{\odot}$F = 720 = 4320/6} {r = 43.20}	**6583**{= $\bar{\odot}$O = (39.5/60)* 10,000} {$\bar{\odot}$F = 10,000} {r = 39.5}	**1917** {= $\bar{\odot}$O = (11.5/60)* 10,000} {$\bar{\odot}$F = 10,000} {r = 11.5}	**1083**{= $\bar{\odot}$O = (6.5/60)*10,000} {$\bar{\odot}$F = 10,000} {r = 6.5}
Radius of Planetary Orb based on $\bar{\odot}$O = 25	**9;24** {= (376*25)/1000}	**18**{= (720*25)/1000}	**38**{≈(10,000*25)/6583}	**130;25** {= (10,000*25)/1917}	**230 5/6** {≈230.84 = (10,000*25)/1083}
r_1 = 3/2 e	{405 = 1.5*2.7*100} {e = 2.7} {$\bar{\odot}$F = 376}	{180 = 1.5*1.2*100} {e = 1.2} {$\bar{\odot}$F = 720}	**14**{8}**2**{= (1.5*5.928/60)* 10,000} {e = 5.928} {$\bar{\odot}$F = 10,000}	**777**{= (1.5*3.108/60)*10,000} {e = 3.108} {$\bar{\odot}$F = 10,000}	**852**{= (1.5*3.408/60)*10,000} {e = 3.408} {$\bar{\odot}$F = 10,000}
r_2 = ½ e	{135 = 0.5*2.7*100} {e = 2.7} {$\bar{\odot}$F = 376}	{60 = 0.5*1.2*100} {e = 1.2} {$\bar{\odot}$F = 720}	**494**{= (0.5*5.928/60)* 10,000} {e = 5.928} {$\bar{\odot}$F = 10,000}	**259** {= 0.5*3.108/60)*10,000} {e = 3.108} {$\bar{\odot}$F = 10,000}	**284** {= 0.5*3.408/60)*10,000} {e = 3.408} {$\bar{\odot}$F = 10,000}
$r_1 + r_2$ = 2e	**600** {540}[24] {$\bar{\odot}$F = 376}	{240} {$\bar{\odot}$F = 720}	{1976} {$\bar{\odot}$F = 10,000}	{1036} {$\bar{\odot}$F = 10,000}	{1136} {$\bar{\odot}$F = 10,000}
r_1 = 3/2 e ($\bar{\odot}$O = 25)	**1;41¼** {= (405/6000)*25}	**¾**{= (180/6000)*25}	**5;34**{≈5;38 = 38*1482/ 10,000}	**10 1/10** {≈10.13 = 130;25*777/10,000}	**19 41/60** {≈19.667 = 230 5/6*852/10,000}
r_2 = ½ e ($\bar{\odot}$O = 25)	**0;33¾** {= (135/6000)*25}	**¼**{= (60/6000)*25}	{1};**51** {1;51 ≈ 1;53 = 38*494/10,000}	**3 11/30** {≈3.38 = 130;25*259/10,000}	**6 17/30** {≈6.56 = 230 5/6*284/10,000}
$r_1 + r_2$ = 2e ($\bar{\odot}$O = 25)	{1;41¼ + 0;33¾ = 2;15 = (540/6000)*25}	{1 = ¾ + ¼ = (240/6000)*25}	{5;38 + 1;53 = 7;31 = 38*1976/10,000}	{13 14/30 = 10 1/10 + 3 11/30}	{26 15/60 = 19 41/60 + 6 17/30}
$r_1 - r_2$ ($\bar{\odot}$O = 25)	**1·7·½** {= 1;41¼ − 0;33¾}	{½ = ¾ − ¼}	{5;38 - 1;53 = 3;45}	{6 22/30 = 10 1/10 − 3 11/30}	**13 7/60** = {19 41/60 − 6 17/30}
"diversitas diametrj" (diameter of large circle of Ṭūsī couple) ($\bar{\odot}$O = 60,000)	**1151**[25]	N/A	N/A	N/A	N/A
"diversitas diametrj" ($\bar{\odot}$O = 1000)	**19** {≈1151/60 = 19.183}	N/A	N/A	N/A	N/A
"diversitas diametrj" ($\bar{\odot}$O = 25)	**0;29**{≈19/40 = 0;28½}	N/A	N/A	N/A	N/A

r: Ptolemaic epicycle radius (as given by Copernicus); e: Ptolemaic eccentricity (as given by Copernicus); r_1: radius of first epicycle; r_2: radius of second epicycle; {…}: reconstructed or surmised parameter.
Numbers in **bold** are in U.

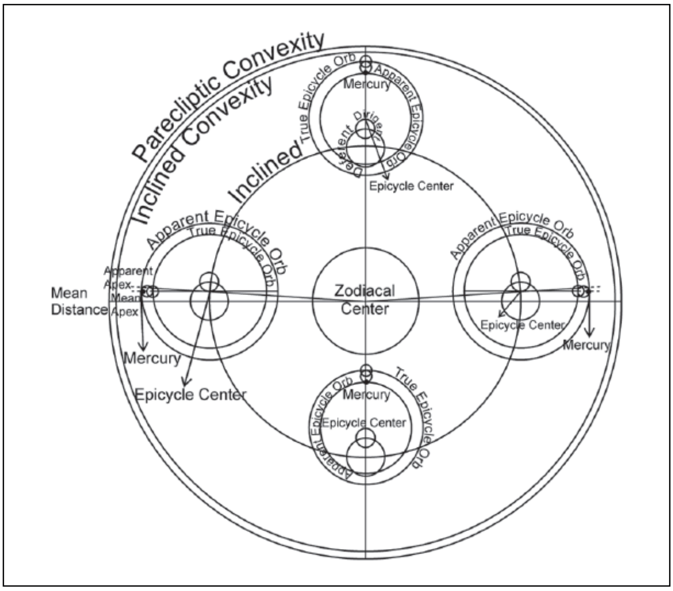


Figure 9. Ibn al-Shāṭir's schematic depiction of his Mercury model.[30]

to Copernicus), while at 90° and 270°, it becomes larger ("traversing a far larger circumference").[31] This would seem to indicate that Copernicus is following the illustration in *Nihāyat al-suʾl.*

Turning to Mercury's parameters, in the upper part of U, Copernicus writes 6 or 600 for r_1+r_2 for Mercury. However, the "ecce" of 2256 (or 376) in conjunction with the 115.1 (or 19) for the *diversitas diametrj*, the displacement resulting from the Ṭūsī couple, implies $r_1+r_2=576$.[32] But Copernicus uses 540 to derive the values in the lower part of U, i.e., $r_1=1;41\frac{1}{4}$ and $r_2=0;33\frac{3}{4}$ (see Note 24). Where does this 540 come from? Looking again at Figure 9, we can conjecture that Copernicus reasoned (incorrectly) as follows: the largest size of the epicycle ("Apparent Epicycle Orb") is $2256+115.1=2371.1$ at 90°. Its smallest size ("Apparent Epicycle Orb") is $2256-115.1=2140.9$ at 0°. But rather than taking the radius of the "True Epicycle Orb," i.e., 2256 (or 376), he adopted the "Apparent Epicycle Orb" at $\bar{\alpha}=0°$ as his reference epicycle, since it is the starting point. If we take the maximum equation to occur at 90°, then the Ptolemaic eccentricity of 6 (or 600) should be measured there with the epicycle being 2371.1. But at $\bar{\alpha}=0°$, the ratio of the two "apparent" epicycles is $2140.9/2371.1\approx.9$. So the sum of the eccentricities (r_1+r_2) should be proportionally lowered, at least according to this reasoning, i.e., $.9\times600=540$.[33] Along with Copernicus' description of a varying planetary "circumference" (epicycle in Ibn al-Shāṭir's model) and the explanation for 540 arising from the diagram, I would argue that Copernicus had at his disposal something like Figure 9. In which case, he had Ibn al-Shāṭir's model when composing the *Commentariolus*. Why then he didn't make the simple transformation of Mercury (as well as Venus) to the *De rev* model is taken up in the concluding section.

Concluding remarks

Thanks to the recent work of Tzvi Langermann and Robert Morrison, we now know that a certain Jewish scholar named Moses Galeano brought knowledge of Ibn al-Shāṭir's models to the Veneto (and environs such as Padua?) at the time Copernicus was studying in Italy.[34] And from the earlier discoveries and research of E.S. Kennedy and his students as well as Otto Neugebauer and Noel Swerdlow, the remarkable similarities between the models of Ibn al-Shāṭir (and other Islamic astronomers) with those of Copernicus have been brought to light. Although there are still skeptics who believe Copernicus could have come up with his models without this cross-cultural influence, I will assume here, without further detailed proof, that Ibn al-Shāṭir's models were available in some form to Copernicus.[35]

As noted at the beginning of this paper, Swerdlow has sought to treat the reform of the first anomaly independently of the heliocentric transformation; many (if not most) other scholars, including Neugebauer, Kennedy, and Goldstein, have agreed with this approach.[36] André Goddu, however, has recently focused on the views of two scholars who sought to link Copernicus' turn to heliocentrism with his stated objective to rid astronomy of the irregular motion of celestial orbs such as that brought on by the equant[37] – or to put it another way, to link the transformation of the second anomaly with the "Marāgha-type" reforms of the first anomaly. The two scholars, Ludwik Antoni Birkenmajer (1855–1929) and Curtis Wilson (1921–2012), proposed somewhat similar views on how the bi-epicyclic device somehow laid bare the possibility for Wilson, the necessity for Birkenmajer, to replace Ptolemy's large, unbecoming epicycles for the outer planets with the Earth's orbit around the Sun as shown in Figure 4 above. In some ways, this is similar to what is being proposed here, namely, the "heliocentric bias" of the bi-epicyclic solution to the equant problem that allows a simple, straightforward transformation to heliocentric models. Where I would differ with Birkenmajer and Wilson (and perhaps Goddu) is that they have not provided plausible pathways to the bi-epicyclic models of the *Commentariolus*, either in their presumed earlier geocentric or final heliocentric forms. Birkenmajer and Goddu invoke Albert of Brudzewo, the Cracow University schoolman who criticized Peurbach's unthinking acceptance of the equant and also proposed a model to deal with the irregular motion brought on by Ptolemy's lunar prosneusis point, as an important, perhaps critical, influence on Copernicus.[38] But these are slim pickings; it is a long way from simply stating the equant problem or proposing a vague model for epicyclic oscillation to Copernicus' *Commentariolus* models.[39]

There is another way that I would differ from Birkenmajer and Wilson as presented by Goddu. Their primary emphasis for Copernicus' path to heliocentrism is on the outer planets; in fact, Wilson states that his figure for the superior planets "cannot be easily adapted to the case of the inferior planets," which is true.[40] On the other hand, I am impressed with the utter simplicity of the transformation of Ibn al-Shāṭir's Venus model and, especially, his complex model for Mercury into the models in *De revolutionibus* (Figure 6).[41] More than the outer planets, this seems to show the "heliocentric bias" in its most obvious form, and I think Ibn al-Shāṭir's models for the inner planets may have been influential in convincing Copernicus of the possibility of heliocentric models. But this does bring up the fact, already mentioned above, that such a simple transformation of the inner planets is not what we have in the *Commentariolus* (Figure 8). Given my commitment to transmission, I would offer the following, tentative scenario. Copernicus, for reasons to be outlined below, was attempting to find some form of a *homocentric* cosmology that resolved the problem of Ptolemy's violations of uniform circular motion, in particular those brought about by the equants. Ibn al-Shāṭir's models offered a compromise, in that they dispensed with eccentrics and all his major deferents were centered on the Earth. The bi-epicyclic device was an uncomfortable but tolerable necessity. But that left the Ptolemaic epicycles, which could be dispensed with by adopting heliocentrism. Admittedly, the latter required a bold leap, but here I think Birkenmajer and Wilson have glimpsed an important part of Copernicus' thinking and motivation. I would just add that this still leaves open the possibility that Copernicus could have also been initially motivated by other factors toward heliocentrism, say Ibn al-Shāṭir's models for the inner planets (my preference) or some other, non-mathematical reason.[42]

Let me expand on the argument regarding homocentrism. As is well known, the homocentric cosmology of the Andalusian Nūr al-Dīn al-Biṭrūjī (fl. *ca*. 1190) was read and commented on in Europe from the time it became available in Latin translation in the early thirteenth century. Coupled with the views of Averroes (1126–1198), another Andalusian who had also advocated a return to Aristotelian homocentric orbs, one can detect a growing interest in homocentric astronomy in fifteenth- and sixteenth-century Europe, as well as Averroism.[43] Copernicus himself brings up Calippus and Eudoxus in his introduction to the *Commentariolus*, and as Swerdlow states, "What is of interest to note about Copernicus's remark is that he objects to the result, but not to the principle of homocentric spheres."[44] Now there has been a tendency among both historians of Islamic science and of astronomy to lump all the eastern Islamic, non-Ptolemaic models under the rubric of the "Marāgha School" and to contrast them with the homocentric proposals that came out of twelfth-century Andalusia.[45] In this scenario, the main issue motivating the former was resolving the irregularities of the equant and its siblings, while the Andalusians were driven by "philosophical" concerns and a desire to return to a pure Aristotelianism. But there is something fundamentally different about Ibn al-Shāṭir's models. They are actually centered on the Earth both mathematically and cosmologically, and they dispense with eccentrics. In a way that likely would have appealed to the Averroists in Bologna and Padua, where Copernicus studied, Ibn al-Shāṭir's models both resolve a number of irregularities of Ptolemaic astronomy and at the same time, unlike those of other members of the so-called "Marāgha School," bring the Earth back into the center of the universe.[46] Although he is certainly not a homocentrist along the lines of al-Biṭrūjī, he was able to achieve a successful "quasi-homocentric" system, whereas the Andalusian Aristotelians and their followers could only tilt at windmills.

If we accept that Copernicus was, at the time of writing the *Commentariolus*, a "quasi-homocentrist" along the lines of Ibn al-Shāṭir, then we can explain the puzzling models for the inner planets. Eschewing their simple transformations that would have led to *De rev*-type models with their eccentrics, he instead chose to make the centers of their main deferents coincide with the mean Sun, i.e., the center of the Earth's orb. However, this created numerous problems, not the least of which was making them difficult if not unusable for calculation. But in the following 30 or so years, the "homocentrism" of the

Commentariolus would give way to the extensive use of eccentrics in *De revolutionibus*. Clearly, there could be no other choice if he were to be taken seriously as a competent mathematical astronomer, someone whose work could rival that of the *Almagest*.

I have attempted to show that there is a relatively straightforward way to go from Ibn al-Shāṭir's planetary longitude models to those in the *Commentariolus* without needing to treat the first and second anomalies independently. In particular, there would have been no need for recourse to Regiomontanus' propositions and the intermediation of eccentric models.[47] I have also tried to present a compelling case that Ibn al-Shāṭir's models had a "heliocentric bias" that may have influenced Copernicus' turn to heliocentrism. What I have not shown, nor was it my intent, is that Ibn al-Shāṭir saw the heliocentric potential of his models or had any inclination in that direction. There is just no evidence I know of to support this. Furthermore, just because Ibn al-Shāṭir's models lend themselves in a certain direction doesn't mean that anyone had to be a borrower. After all, the fact that the Sun's motion about the Earth was connected in some way with each of the planets was hardly news; Ptolemy had already stated as much in the *Almagest*, and one finds this repeated throughout both the Islamic and Latin middle ages.[48] Here, I would speculate that Ibn al-Shāṭir's models, however "biased" they might be, would only influence someone toward heliocentrism who was already inclined in that direction. Ibn al-Shāṭir's models, when all is said and done, are geocentric, and they work remarkably well. Why mess with something that wasn't broken unless, of course, one was already disposed toward a new cosmology, which brings us to the recurring question of not "how" Copernicus developed his models but "why." And to that there are no lack of answers, to which I shall refrain from adding another.

Acknowledgements

This paper would not have been possible without the insights of my student and collaborator Sajjad Nikfahm-Khubravan; he first alerted me to the "heliocentric bias" of Ibn al-Shāṭir's models, and he has been my most acute and constructive critic. Robert Morrison's comments on an earlier draft were very helpful, especially regarding the connection of Ibn al-Shāṭir with homocentrism. An anonymous reviewer saved me from several major blunders and generously provided advice on how to present the transformation of Ibn al-Shāṭir's models to Copernicus'. And as always, Sally Ragep's good judgment and keen editorial eye helped make this a much better paper. All remaining shortcomings are, of course, my own.

Notes on Contributor

Jamil Ragep is a professor of Islamic Studies at McGill University and is currently working on projects involving the relation of Islamic astronomy to Copernicus, science education in Islam, and a database of Islamic scientific manuscripts.

Notes

1. N.M. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory: A Translation of the Commentariolus with Commentary," *Proceedings of the American Philosophical Society*, 117(6), 1973, pp. 423–512, <https://www.jstor.org/stable/986461?seq=1#page_scan_tab_contents>. A facsimile of the Uppsala notes and Swerdlow's transcription, referred to throughout this paper, are on pp. 428–9.

2. As is well-known, heliocentric in this context means “centered on the mean Sun,” not the “true Sun.”
3. Swerdlow, “The Derivation and First Draft of Copernicus’s Planetary Theory” (see Note 1), pp. 471–8. See also N.M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus’s De Revolutionibus*, 2 parts (New York: Springer-Verlag, 1984), part 1, pp. 54–64, esp. 55–8. Dennis Duke provides animations showing this transformation from Ptolemaic epicyclic models to eccentric models to Copernican models at <https://people.sc.fsu.edu/~dduke/models> (25 September 2016). For the treatise by ʿAlī Qushjī that may well have provided the basis for Regiomontanus’ propositions, see F. Jamil Ragep, “ʿAlī Qushjī and Regiomontanus: Eccentric Transformations and Copernican Revolutions,” *Journal for the History of Astronomy*, 36(4), 2005, pp. 359–71.
4. Swerdlow, “The Derivation and First Draft of Copernicus’s Planetary Theory” (see Note 1), p. 478.
5.

   This [introduction in the Commentariolus of the heliocentric theory] really has nothing to do with the principle of uniform circular motion that started Copernicus’s investigations in the first place, but it seems likely that in the course of the intensive study of planetary theory undertaken to solve the problem of the first anomaly, he carried out an analysis of the second anomaly leading to his remarkable discovery.

   (Swerdlow, “The Derivation and First Draft of Copernicus’s Planetary Theory” (see Note 1), p. 425). See also Swerdlow, “The Derivation and First Draft of Copernicus’s Planetary Theory,” p. 430: “… the Maragha theory is, in any case, relevant only to the first anomaly, not to the heliocentric theory.”
6. For the purposes of this paper, the most important is E.S. Kennedy and V. Roberts, “The Planetary Theory of Ibn al-Shāṭir,” *Isis*, 50(3), 1959, pp. 227–35, reprinted E.S. Kennedy, “Colleagues and Former Students,” in D.A. King and M.H. Kennedy (eds), *Studies in the Islamic Exact Sciences* (Beirut: American University of Beirut, 1983), pp. 55–63.
7. V. Roberts, “The Solar and Lunar Theory of Ibn Ash-Shāṭir: A Pre-Copernican Copernican Model,” *Isis*, 48(4), 1957, pp. 428–32, n. 2 on p. 428.
8. Although neither Swerdlow nor Neugebauer thought there was a connection between Copernicus’ heliocentrism and his Islamic predecessors, it should be noted that both consistently maintained the importance of Islamic astronomy, and in particular Ibn al-Shāṭir’s models, for Copernicus:

   The planetary models for longitude in the *Commentariolus* are all based upon the models of Ibn ash-Shāṭir – although the arrangement for the inferior planets is incorrect – while those for the superior planets in *De revolutionibus* use the same arrangement as ʿUrdi’s and Shīrāzī’s model, and for the inferior planets the smaller epicycle is converted into an equivalent rotating eccentricity that constitutes a correct adaptation of Ibn ash-Shāṭir’s model. In both the *Commentariolus* and *De revolutionibus* the lunar model is identical to Ibn ash-Shāṭir’s and finally in both works Copernicus makes it clear that he was addressing the same physical problems of Ptolemy’s models as his predecessors. It is obvious that with regard to these problems, his solutions were the same.

   The question therefore is not whether, but when, where, and in what form he learned of Marāgha theory. (Swerdlow and Neugebauer, *Mathematical Astronomy in Copernicus’s De Revolutionibus* (see Note 3), part 1, p. 47)

9. George Saliba has perceptively discussed the reasons for Ibn al-Shāṭir’s dismissal of eccentrics and justification of epicycles in several of his writings; see G. Saliba, “Critiques of

Ptolemaic Astronomy in Islamic Spain," *Al-Qantara: revista de estudios arabes*, 20(1), 1999, pp. 3–25, on pp. 15–17; G. Saliba, *Islamic Science and the Making of the European Renaissance* (Cambridge: MIT Press, 2007), pp. 162–3.

10. Saliba has already pointed to this in Saliba, *Islamic Science and the Making of the European Renaissance* (see Note 9), p. 164:

    One additional advantage [of Ibn al-Shāṭir's models] resulted from this systematic use of geocentricity, which was to come in handy later on during the European Renaissance: the unification of all the Ptolemaic geocentric models under one structure that lent itself to the simple shift of the centrality of the universe from the Earth to the sun, thus producing heliocentrism, without having to make any changes in the rest of the models that accounted well for the Ptolemaic observations resulting from the equant.

    See also Saliba, *Islamic Science and the Making of the European Renaissance*, pp. 193–4, where Saliba notes their "strict Aristotelian cosmological requirements of abolishing eccentrics," and "the unintended consequences of the unified models [that] produced the 'strange' development that allowed them to be transferred into heliocentric models …"
11. Adapted from E.S. Kennedy, "Late Medieval Planetary Theory," *Isis*, 57(3), 1966, pp. 365–78, Figure 1 on p. 367.
12. Cf. Kennedy and Roberts, "The Planetary Theory of Ibn al-Shāṭir" (see Note 6), p. 229, reprinted p. 57 and Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 468. See also G. Saliba, "Arabic Astronomy and Copernicus," *Zeitschrift für Geschichte der Arabisch-Islamischen Wissenschaften*, 1, 1984, pp. 73–87, on pp. 81–4, reprinted in G. Saliba, *A History of Arabic Astronomy: Planetary Theories during the Golden Age of Islam* (New York: New York University Press, 1994), pp. 291–305, on pp. 299–302.
13. Counterclockwise is in the positive (sequential) direction of the zodiacal signs; clockwise is in the negative (counter-sequential) direction.
14. For simplicity, γ is being measured in Figure 2 from the epicyclic perigee rather than the "true" apex, which is the point from which the motion of the epicycle would normally be measured. Thus, for both Ptolemy and Ibn al-Shāṭir, the depicted position of the planet on the epicycle would be $180° + \gamma$.
15. For an elaborated version, see Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), Figure 26, p. 481. Note that Curtis Wilson also suggested a similar transformation; however, since he does not take into account the possibility of Copernicus having Ibn al-Shāṭir's models, his transformation required the additional steps of first coming up with the bi-epicyclic device to deal with the first anomaly. See C. Wilson, "Rheticus, Ravetz, and the 'Necessity' of Copernicus' Innovation," in R.S. Westman (ed.), *The Copernican Achievement* (Berkeley: University of California Press, 1975), pp. 17–39, esp. Figure 5, p. 35. Wilson's analysis has recently been re-examined by A. Goddu in his "Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism," *Isis*, 107(2), 2016, pp. 225–53. I thank an anonymous reviewer for bringing these references to my attention.
16. Cf. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), Figure 34, p. 492.
17. For details on Mercury, see Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), pp. 499–509; the model is illustrated in Figure 39, p. 501. See also S. Nikfahm-Khubravan and F.J. Ragep, "Ibn al-Shāṭir and Copernicus on Mercury" (in press).

18. Swerdlow and Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus* (see Note 3), part 1, pp. 62, 372–3, where the claim is made that "the difficulty was probably due to Copernicus's originally using the eccentric model for the second anomaly." In what follows, I provide an alternative explanation for the peculiarities of these models. See also Nikfahm-Khubravan and Ragep, "Ibn al-Shāṭir and Copernicus on Mercury" (see Note 17).
19. See Swerdlow and Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus* (see Note 3), part 1, pp. 299–300, 356 ff., 384 ff., where they discuss why Copernicus may have decided to introduce eccentrics in his *De rev* models.
20. Swerdlow recognizes this possible interpretation of *eccentricitas*:

    In holding the eccentricity constant, Copernicus has, of course, done something of enormous importance, for although he did not mention it in U, we know that he also assumed the eccentricity to be the distance between the earth and the mean sun.

    ("The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 474).
21. Could this be why Copernicus refers to the Earth's orbit around the Sun as the Great Sphere (*orbis magnus*), since all the "eccentricities" are its radius? Cf. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 442 for a different interpretation.
22. Although the models for Mercury and Venus are somewhat different, for this exercise, we can refer to Venus's model in Figure 8.
23. Of course, another way to look at this is that Copernicus has extracted this number from the Alphonsine tables as Swerdlow has shown. This would account for the rather odd 2256 instead of Ptolemy's 2250 (epicycle radius = 22.5). Swerdlow interprets 2256, unnecessarily in my opinion, as the eccentricity in the eccentric model of the second anomaly. See "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 505.
24. The "a cum b" for Mercury (upper notes) was written first as 10, apparently because Copernicus had forgotten he wasn't norming to 1000 (100/60 = x/6; x = 10); so 10 is crossed out and 2e = 6 is substituted; the 100 apparently means this number should be multiplied by 100 to be compatible with the 2256, i.e., it should be 600. However, the lower notes imply 540, i.e., 2e = 5.4 [(540/6000)*25 = 2;15 and 1;41¼ + 33¾ = 2;15]; for a possible explanation of this number, see infra.
25. For the derivation of this number, see Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), pp. 507–8.
26. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 425.
27. Kennedy and Roberts, "The Planetary Theory of Ibn al-Shāṭir" (see Note 6), p. 230, reprinted in p. 58.
28. The resultant value from Copernicus' parameters is 2;34,4, whereas one may derive a much more accurate value of 3;1,7 from the *De rev* parameters. See Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 509, where he calls the *Commentariolus* value "absurd." Ibn al-Shāṭir's parameters result in 3;1,53, which is close to Ptolemy's 3;1,45; see Nikfahm-Khubravan and Ragep, "Ibn al-Shāṭir and Copernicus on Mercury" (see Note 17).
29. See Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 504, where he states that "This misunderstanding must mean that Copernicus did not know the relation of the model to Mercury's apparent motion." This interpretation has been challenged by V. Blåsjö, "A Critique of the Arguments for Maragha Influence on Copernicus," *Journal for the History of Astronomy*, 45(2), 2014, pp. 183–95, on pp. 189–93. For an extended discussion of this issue and a critique of Blåsjö's approach, see Nikfahm-Khubravan and Ragep, "Ibn al-Shāṭir and Copernicus on Mercury" (see Note 17).

30. Figures vary greatly in the manuscripts of Ibn al-Shāṭir's *Nihāyat al-suʾl*; what is represented here is close to what one finds in Oxford, Bodleian, Marsh MS 139, f. 29a.
31. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 503 for the quotations from Copernicus.
32. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 507, where he derives 576(0). As he notes (pp. 508–9), Copernicus seems to have had considerable problems in converting from the upper value in U for $r_1 + r_2$ to the values for the two epicycles in the lower part.
33. This also works, of course, if one uses 376 and 19 instead of 2256 and 115.1.
34. Y.T. Langermann, "A Compendium of Renaissance Science: *Taʿalumot ḥokma* by Moshe Galeano," *Aleph: Historical Studies in Science and Judaism*, 7, 2007, pp. 283–318 on pp. 290–6; R. Morrison, "A Scholarly Intermediary between the Ottoman Empire and Renaissance Europe," *Isis*, 105(1), 2014, pp. 32–57.
35. A detailed argument is in preparation, which will supplement F.J. Ragep, "Copernicus and His Islamic Predecessors: Some Historical Remarks," *History of Science*, 45, 2007, pp. 65–81.
36. Swerdlow, though, does note that dealing with the irregularities related to the first anomaly may have led Copernicus to investigate the second anomaly, which led to the heliocentric models; Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 425. See also, Swerdlow and Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus* (see Note 3), part 1, p. 56:

    Copernicus probably undertook an investigation of the second anomaly, and of the eccentric model, because even with the Marāgha solution to the first anomaly, the uniform motion of the planet on the epicycle must still be measured from the mean apogee lying on a line directed to the equant …

    For B. Goldstein's views, see "Copernicus and the Origin of His Heliocentric System," *Journal for the History of Astronomy*, 33(3), 2002, pp. 219–35, on pp. 219–20. Goddu provides a summary of the views of Swerdlow and Goldstein ("Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism" (see Note 15), pp. 227–8).
37. Goddu, "Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism" (see Note 15).
38. Recently, there has been something of an explosion of interest in Brudzewo. Goddu provides a nice summary of him and the possible relation to Copernicus' astronomy in "Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism" (see Note 15), pp. 230–2, 236–43; for references, see n. 26 on p. 232 and passim. M. Malpangotto makes an extended argument for the importance of Brudzewo in "The Original Motivation for Copernicus's Research: Albert of Brudzewo's *Commentariolum super Theoricas novas Georgii Purbachii*," *Archive for History of Exact Sciences*, 70, 2016, pp. 361–411. Goddu also extensively discussed Brudzewo in A. Goddu, *Copernicus and the Aristotelian Tradition: Education, Reading, and Philosophy in Copernicus's Path to Heliocentrism* (Leiden: Brill, 2010), for which see P. Barker and M. Vesel, "Goddu's Copernicus: An Essay Review of André Goddu's *Copernicus and the Aristotelian Tradition*," *Aestimatio*, 9, 2012, pp. 304–36 and A. Goddu, "A Response to Peter Barker and Matjaž Vesel, 'Goddu's Copernicus'," *Aestimatio*, 10, 2013, pp. 248–76, esp. pp. 260–7.
39. I discuss Brudzewo's model, and differentiate it from Copernicus' bi-epicyclic device, in F.J. Ragep, "From Tūn to Toruń: The Twists and Turns of the Ṭūsī-Couple," in R. Feldhay and F.J. Ragep (eds), *Before Copernicus: The Cultures and Contexts of Scientific Learning in the Fifteenth Century* (Montreal: McGill-Queen's University Press, 2017 [exp.]).

40. Wilson, "Rheticus, Ravetz, and the 'Necessity' of Copernicus' Innovation" (see Note 15), p. 34, n. 25. This is cited by Goddu, "Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism" (see Note 15), p. 248, who credits Robert Westman for bringing Wilson's views to his attention (p. 226, n. 3).
41. For Mercury, see Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 502, Figure 40 and Swerdlow and Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus* (see Note 3), part 1, p. 410; the transformation from Ibn al-Shāṭir's model to the *De rev* model can be seen from Figure 70 (part 2, p. 657) to Figure 73(a) (part 2, p. 658). This is discussed in detail in Nikfahm-Khubravan and Ragep, "Ibn al-Shāṭir and Copernicus on Mercury" (see Note 17).
42. One of the factors could be the ordering of the planets, which Bernard Goldstein and Robert Westman have both claimed as the major motivation for Copernicus; Goldstein, "Copernicus and the Origin of His Heliocentric System" (see Note 36) and R. Westman, *The Copernican Question* (Berkeley: University of California Press, 2011), esp. pp. 76–105. Another factor could have been the question of the Earth's possible rotation, which had been extensively discussed in both the Latin and Islamic worlds; see F.J. Ragep, "Ṭūsī and Copernicus: The Earth's Motion in Context," *Science in Context*, 14(1–2), 2001, pp. 145–63. On the need for a multifaceted approach to Copernicus, see Feldhay and Ragep (eds), *Before Copernicus* (see Note 39), Introduction.
43. There is in fact an extensive amount of work on the subject. The importance of homocentric astronomy, especially for Regiomontanus, has been emphasized by Michael Shank in several articles: M.H. Shank, "The 'Notes on al-Biṭrūjī' Attributed to Regiomontanus: Second Thoughts," *Journal for the History of Astronomy*, 23(1), 1992, pp. 15–30; M.H. Shank, "Regiomontanus and Homocentric Astronomy," *Journal for the History of Astronomy*, 29(2), 1998, pp. 157–66. Robert Morrison has also drawn our attention to the Jewish role in disseminating homocentric astronomy in Europe: R.G. Morrison, *The Light of the World: Astronomy in al-Andalus* (Berkeley; Los Angeles: University of California Press, 2016). See also the important article by N.M. Swerdlow, "Regiomontanus's Concentric-Sphere Models for the Sun and the Moon," *Journal for the History of Astronomy*, 30(1), 1999, pp. 1–23. Swerdlow had earlier noted the interest in homocentric astronomy in N.M. Swerdlow, "Aristotelian Planetary Theory in the Renaissance: Giovanni Battista Amico's Homocentric Spheres," *Journal for the History of Astronomy*, 3(1), 1972, pp. 36–48. Homocentric astronomy in early modern Europe is also dealt with by M. Di Bono, *Le sfere omocentriche di Giovan Battista Amico …* (Genoa: Centro di Studio sulla Storia della Tecnica, 1990); E. Peruzzi, *La nave di Ermete: la cosmologia di Girolamo Fracastoro* (Florence: Olschki, 1995). Goldstein has connected the ordering of the planets, which he sees as crucial for Copernicus, to the Averroists ("Copernicus and the Origin of His Heliocentric System" (see Note 36), p. 225). On Averroism in early modern Europe, see A. Akasoy and G. Giglioni, *Renaissance Averroism and Its Aftermath: Arabic Philosophy in Early Modern Europe* (Dordrecht: Springer, 2013). I was intrigued to discover that "Birkenmajer concluded that Copernicus knew the Averroist critique of Ptolemaic models, and he believed that the critique motivated Copernicus to adopt concentric models initially." (Goddu, "Ludwik Antoni Birkenmajer and Curtis Wilson on the Origin of Nicholas Copernicus's Heliocentrism" (see Note 15), p. 242).
44. Swerdlow, "The Derivation and First Draft of Copernicus's Planetary Theory" (see Note 1), p. 434.
45. A.I. Sabra, "The Andalusian Revolt against Ptolemaic Astronomy: Averroes and al-Biṭrūjī," in E. Mendelsohn (ed.), *Transformation and Tradition in the Sciences: Essays in Honor of I. Bernard Cohen* (Cambridge: Cambridge University Press, 1984), pp. 133–53, reprinted in A.I. Sabra, *Optics, Astronomy and Logic: Studies in Arabic Science and Philosophy*, XV (Aldershot: Ashgate Variorum Reprints, 1994). Saliba has argued against such a dichotomization in his "Critiques of Ptolemaic Astronomy in Islamic Spain" (see Note 9).

46. Morrison, *The Light of the World* (see Note 43), p. 44, n. 165, also associates the astronomy of Ibn al-Shāṭir with the homocentric astronomy of Ibn Naḥmias (fl. *ca*. 1400 C.E.), someone who may well have been known in Renaissance Italy. Morrison, following Saliba (see Note 9 supra), notes that Ibn al-Shāṭir made a strict distinction between eccentrics, which were unacceptable, and epicycles, which were possible, likening them to stars or planets that were also embedded in the cosmos; this could well have opened the way for an Aristotelian or Averroist to accept Ibn al-Shāṭir's "quasi-homocentrism." Morrison also points to Profiat Duran (d. *ca*. 1415) as someone who interpreted Maimonides' doctrine of homocentricity as allowing for epicycles (Morrison, *The Light of the World*, p. 16).
47. The fact that the transformations are mathematically consistent with Regiomontanus' propositions does not entail that they were actually used by Copernicus.
48. For example, Naṣīr al-Dīn al-Ṭūsī in his *Tadhkira* states,

    They placed the sun in the medial orb between the former and the latter … deeming this the most elegant arrangement and the most excellent structure inasmuch as the six were connected to it – the upper [planets] in a certain way, the lower in another and the moon in yet another.

    (F.J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's Memoir on Astronomy (al-Tadhkira fī ʿilm al-hayʾa)*, 2 vols. (New York: Springer-Verlag, 1993), vol. 1, p. 110). Cf. G.J. Toomer (trans.), *Ptolemy's Almagest* (London: Duckworth, 1984), pp. 419–20 [H207]. For the Latin West, see E. Grant, *Planets, Stars, and Orbs: The Medieval Cosmos, 1200–1687* (Cambridge: Cambridge University Press, 1994), p. 233.

THE MERCURY MODELS OF IBN AL-ŠĀṬIR AND COPERNICUS

SAJJAD NIKFAHM-KHUBRAVAN
Institute of Islamic Studies, McGill University

F. JAMIL RAGEP
Institute of Islamic Studies, McGill University

Abstract. Copernicus' complex Mercury model in *De revolutionibus* is virtually identical, geometrically, to Ibn al-Šāṭir's (ca. 1305 – ca. 1375). However, the model in his earlier *Commentariolus* is different and in many ways unworkable. This has led some to claim that the younger Copernicus did not understand his predecessor's model; others have maintained that Copernicus was working totally independently of Ibn al-Šāṭir. We argue that Copernicus did have Ibn al-Šāṭir's models but needed to modify them to conform to a "quasi-homocentricity" in the *Commentariolus*. This modification, and the move from a geocentric to heliocentric cosmology, was facilitated by the "heliocentric bias" of Ibn al-Šāṭir's models, in which the Earth was the actual center of mean motion, in contrast to Ptolemy and most Islamicate astronomers. We show that: 1) Ibn al-Šāṭir sought to reproduce Ptolemy's critical elongation at the trines (±120°), but changed the Ptolemaic values at 0, ±90, and 180°; 2) in the *Commentariolus*, Copernicus does not try to produce viable elongations for Mercury; and 3) by the time of writing *De revolutionibus*, Copernicus is in full control of the Mercury model and is able to faithfully reproduce Ptolemy's elongations at all critical points. We also argue that claims regarding "natural" solutions undermining transmission are belied by historical evidence.

Résumé. Le modèle complexe de Mercure dans le *De revolutionibus* de Copernic est virtuellement identique, géométriquement, à celui d'Ibn al-Šāṭir (ca. 1305 – ca. 1375). Cependant, le modèle, antérieur, du *Commentariolus* est différent et il fonctionne mal. Certains en ont déduit que le jeune Copernic n'avait pas compris le modèle de son prédécesseur ; d'autres ont affirmé que l'œuvre de Copernic était totallement indépendante d'Ibn al-Šāṭir. Nous soutenons que Copernic avait les modèles d'Ibn al-Šāṭir mais qu'il a dû les modifier pour les rendre "quasi-homocentriques" dans le *Commentariolus*. Cette modification et le passage d'une cosmologie géocentrique à une cosmologie héliocentrique étaient rendus aisés par le "biais héliocentrique" des modèles d'Ibn al-Šāṭir, pour qui la Terre était le centre effectif du mouvement moyen, contrairement à Ptolémée et à la plupart des astronomes islamiques. Nous montrons que : 1) Ibn al-Šāṭir a cherché à reproduire les élongations critiques à ±120° de l'apogée, mais il a changé les valeurs ptoléméennes à 0, ±90 et 180° ; 2) dans le *Commentariolus*, Copernic n'essaie pas de reproduire des élongations viables pour Mercure ; et 3) au moment de la rédaction du *De*

revolutionibus, Copernic contrôle pleinement le modèle de Mercure et il est capable de reproduire les élongations de Ptolémée aux points critiques. Nous soutenons aussi que les arguments concernant des solutions " naturelles " qui excluent la transmission sont niés par l'évidence historique.

1. INTRODUCTION

We begin with a remarkable but little-remarked fact: Copernicus' most complex planetary model in *De revolutionibus*, that for Mercury, is for all intents and purposes virtually identical, geometrically, to Ibn al-Šāṭir's (ca. 1305 – ca. 1375). But even more significant, it is simple to transform Ibn al-Šāṭir's *geocentric* model into Copernicus' final, *heliocentric* model. (See figures 1 and 2; a fuller analysis will be given below.)

One would have expected this virtual equivalence to be something that would have elicited considerable interest and provoked numerous explanations among scholars, especially since it was stated clearly by E. S. Kennedy and Victor Roberts in their seminal paper on Ibn al-Šāṭir's planetary theory, published in *Isis* in 1959 [1]. Curiously, this has received scant attention in much of the recent writings on Copernicus or else has been dismissed. Michel-Pierre Lerner and Alain-Philippe Segonds, in their notes on the Mercury model in *De revolutionibus,* do not mention Ibn al-Šāṭir or his model [2], nor does Michela Malpangotto in her article on Peurbach's Mercury model that is audaciously entitled " L'Univers auquel s'est confronté Copernic " [3]. Robert Westman, in his massive tome on Copernicus, mentions Ibn al-Šāṭir only once, and that in a minor footnote related to the lunar model [4]. André Goddu even denies the similarity of the models, opining that " Experts have exaggerated the supposed identity between

[1] E. S. Kennedy and V. Roberts, " The Planetary theory of Ibn al-Shāṭir ", *Isis,* 50/3 (1959): 227-35 at 232-3, reprinted E. S. Kennedy, " Colleagues and former students ", in D. A. King and M. H. Kennedy (ed.), *Studies in the Islamic exact sciences* (Beirut, 1983), p. 55-63 at 60-1.

[2] Nicolas Copernic, *De revolutionibus orbium coelestium (Des révolutions des orbes célestes)*, 3 vol., transl. M.-P. Lerner and A.-P. Segonds with the collaboration of C. Luna, I. Pantin, and D. Savoie (Paris, 2015), vol. III, p. 394-409. Elsewhere they at least mention the similarity of the lunar models of Ibn al-Šāṭir and Copernicus but immediately cast doubt on its significance (III, 307; see also I, 311, 354, n. 1, 553-4).

[3] M. Malpangotto, " L'Univers auquel s'est confronté Copernic: La sphère de Mercure dans les *Theoricae novae planetarum* de Georg Peurbach ", *Historia mathematica*, 40/3 (2013): 262-308.

[4] R. S. Westman, *The Copernican question: Prognostication, skepticism, and celestial order* (Berkeley, 2011), p. 531, n. 136.

Copernicus' and al-Shatir's models and the Tusi couple… The question should be reconsidered[5]". A different tack is taken by Viktor Blåsjö, who insists that similarities between models can be explained by there being "natural" solutions that would lead Copernicus and Ibn al-Šāṭir to come to similar conclusions without the necessity of assuming influence[6]. (More on this later.)

On the other hand, Noel Swerdlow, throughout his career, has insisted that the similarities between Copernicus' models and those of his Islamic predecessors "is so close that independent invention by Copernicus is all but impossible[7]". But for Mercury (as well as for Venus) this creates something of an unacknowledged conundrum for Swerdlow. Since Ibn al-Šāṭir's Mercury model and Copernicus' in *De revolutionibus* are *virtually* the same, one must then explain why the *Commentariolus* model (from some 30 years earlier) is different, not to say flawed, if, as Swerdlow has maintained, Copernicus did have Ibn al-Šāṭir's one and only Mercury model when composing the *Commentariolus*. Swerdlow has provided a complex scenario, most recently repeated in an article, that culminates with the *Commentariolus* model[8]. But it has seemed odd to us that Copernicus substituted a flawed model when, according to Swerdlow, he had a much better one immediately at hand. We are also uncomfortable with the numerous ad hoc assumptions Swerdlow needs to make in order for Copernicus to reach, over a 30-year period, essentially what he had all along. Thus part of the purpose of this paper is to suggest an alternative account that we believe provides a more straightforward explanation[9]. Inasmuch as Swerdlow has already offered a critique of some of the central points in this paper, we will need to respond to his criticisms[10].

[5] A. Goddu, *Copernicus and the Aristotelian tradition: Education, reading, and philosophy in Copernicus's path to heliocentrism* (Leiden, 2010), p. 157.

[6] V. Blåsjö, "A critique of the arguments for Maragha influence on Copernicus", *Journal for the history of astronomy*, 45/2 (2014): 183-95.

[7] N. Swerdlow, "Copernicus, Nicolaus (1473-1543)", in W. Applebaum (ed.), *Encyclopedia of the scientific revolution from Copernicus to Newton* (New York, 2000), p. 165.

[8] N. M. Swerdlow, "The Derivation and first draft of Copernicus's planetary theory: A translation of the Commentariolus with commentary", *Proceedings of the American Philosophical Society*, 117/6 (1973): 423-512, esp. 471-8, 499-509. Swerdlow usefully summarizes his position in "Copernicus's derivation of the heliocentric theory from Regiomontanus's eccentric models of the second inequality of the superior and inferior planets", *Journal for the history of astronomy*, 48/1 (2017): 33-61, esp. 33-44.

[9] A preliminary attempt to deal with Copernicus' Mercury models and their connection to that of Ibn al-Šāṭir is in F. J. Ragep, "Ibn al-Shāṭir and Copernicus: The Uppsala notes revisited", *Journal for the history of astronomy*, 47/4 (2016): 395-415 at 400-6.

[10] Swerdlow, "Copernicus's derivation of the heliocentric theory", p. 45-61.

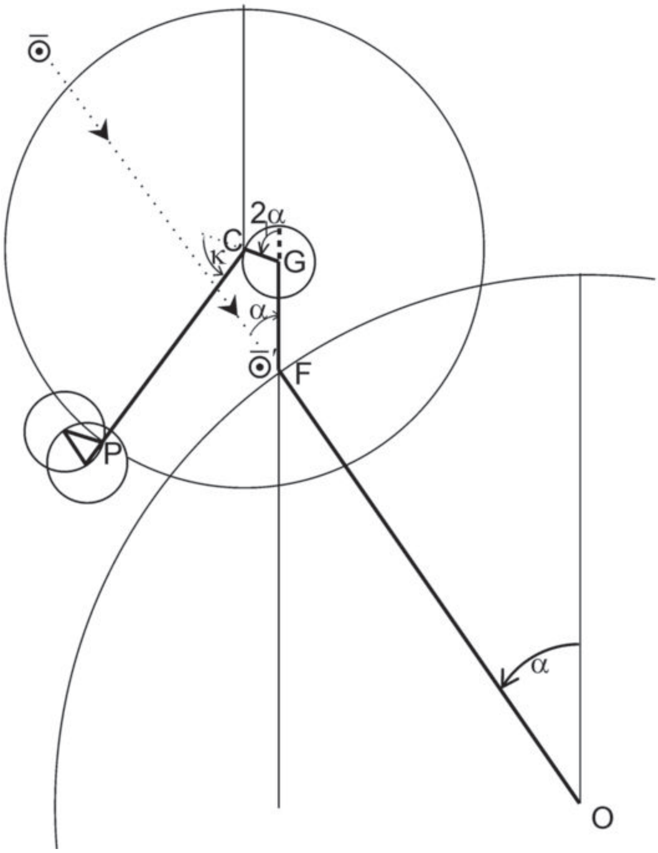


Fig. 1. Ibn al-Šāṭir's Mercury model in *Nihāyat al-su'l*. Moving the mean Sun to F results in the "Tychonic" version of the *De rev.* model. (Not to scale*.)

* For reasons of visualization, our figures are not to scale; in general, we use a mean motion (α) of 35°, which entails an epicycle motion (κ) of ca. 75°. In drafting the figures, we assume that the deferent apogee and epicycle apex are on the apsidal line when $\alpha = 0°$. Darker lines indicate the sequence of the radii of the orbs from the Earth to the planet due to the various motions. Animations illustrating the transformation of Ibn al-Šāṭir's models into those of Copernicus may be found at `https://islamsci.mcgill.ca/MercuryAnimations/`.

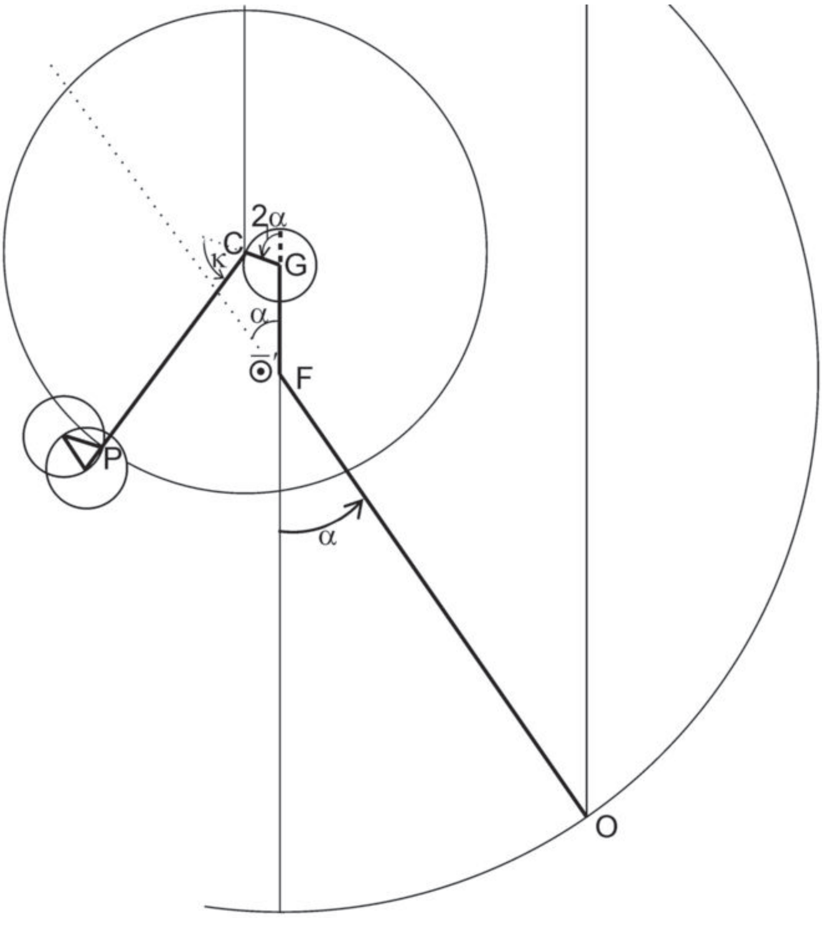


Fig. 2. Copernicus' Mercury model in *De revolutionibus*. (Not to scale.)

Another aim of this paper is to deal with Blåsjö's claims regarding what he calls the "equivalence" of the Mercury models in the *Almagest* and the *Commentariolus*, as well as his insistence that there is a "natural" route that goes from Ptolemy to the more correct models in *De revolutionibus* that undermines transmission. To do this, we need to provide detailed discussions of the Mercury models of Ptolemy, in addition to those of Ibn al-Šāṭir and Copernicus. The former has been discussed competently and in detail by a number of historians [11], but it will be useful to summarize a few salient points for our analysis. For Copernicus, we have Swerdlow's translation and study of the *Commentariolus* as well as Swerdlow and Neugebauer's lengthy study of *De revolutionibus* [12], both being indispensable for this paper. As for Ibn al-Šāṭir's model, there are good presentations by E. S. Kennedy and Victor Roberts [13], as well as by Willy Hartner [14]; however, their work did not delve deeply enough for the kind of comparisons that will allow us to see how Copernicus appropriated the work of his predecessors. Another problem is that up until recently, there have been no published editions or translations of Ibn al-Šāṭir's *Nihāyat al-su'l* where he presents his Mercury model [15]. So in appendices 2 and 3, we provide a translation and critical edition of chapter 21 of part 1 of his work that deal with Mercury, based on ten manuscripts.

[11] See, for example, O. Pedersen, *A survey of the Almagest*, reprint of the 1974 orig. ed. with annotation and new commentary by A. Jones (New York, 2011), p. 309-28; O. Neugebauer, *A history of ancient mathematical astronomy*, 3 parts (Berlin / New York, 1975), I, 158-69; and esp. N. Swerdlow, "Ptolemy's theory of the inferior planets", *Journal for the history of astronomy*, 20/1 (1989): 29-60 at 43-59.

[12] Swerdlow, "The Derivation and first draft", p. 499-509; N. M. Swerdlow and O. Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, 2 parts (New York, 1984), I, 403-43.

[13] Kennedy and Roberts, "The Planetary theory of Ibn al-Shāṭir", p. 231-2.

[14] W. Hartner, "Ptolemy, Azarquiel, Ibn al-Shāṭir, and Copernicus on Mercury: A study of parameters", *Archives internationales d'histoire des sciences*, 24/4 (1974): 5-25, reprinted in W. Hartner, *Oriens-Occidens: Ausgewählte Schriften zur Wissenschafts- und Kulturgeschichte: Festschrift zum 60. Geburtstag*, ed. Y. Maeyama, 2 vol. (Hildesheim: Olms, 1968-1984), vol. II p. 292-312.

[15] G. Saliba does give an English translation of the Saturn chapter in his "Arabic astronomy and Copernicus", *Zeitschrift für Geschichte der Arabisch-Islamischen Wissenschaften*, 1 (1984): 73-87 at 81-2, reprinted in G. Saliba, *A history of Arabic astronomy: Planetary theories during the golden age of Islam* (New York, 1994), p. 291-305 at 299-300. E. Penchèvre has recently published an edition and French translation of part 1, ch. 25 of *Nihāyat al-su'l*, which deals with the latitude theory for Venus and Mercury ("Vénus selon Ibn al-Šāṭir", *Arabic sciences and philosophy*, 26/2 (2016): 185-214 at 202-14). Penchèvre has also put online an edition, French translation, and commentary of the *Nihāyat al-su'l* at arXiv.org (`https://arxiv.org/abs/1709.04965`: "La *Nihāya al-sūl fī taṣḥīḥ al-'uṣūl* d'Ibn al-Šāṭir: Édition, traduction et commentaire"; accessed 27 February 2018).

As an aside before we begin: because this paper deals with a controversial topic, and the ideas underlying it have generated a fair amount of criticism, we thought we should provide a summary of what we are claiming as well as not claiming.

1) We are not claiming that Ibn al-Šāṭir ever entertained, or even thought about, a heliocentric cosmology. At least we have no evidence to support such a contention. He has developed a quite coherent geocentric cosmological system, which is what we assume he intended.

2) When we say Ibn al-Šāṭir's models have a " heliocentric bias ", we mean that Ibn al-Šāṭir has made the Earth the center of mean motion (α). This gives his system a certain " bias " that makes the transformation from a geocentric to heliocentric system much easier. For details, see Ragep, " Ibn al-Šāṭir and Copernicus ".

3) Whether one believes that Copernicus appropriated Ibn al-Šāṭir's models, or reinvented them on their own, it is incontrovertible that one cannot get to Copernicus' models, either in the *Commentariolus* or *De rev.*, without models that are virtually identical to Ibn al-Šāṭir's.

4) We claim that Copernicus in all likelihood did not develop his models on his own; the similarities with those of Ibn al-Šāṭir are just too many to make a plausible case for independent discovery. As we will show below, this is especially true for Mercury.

5) Our proposal for the transformation from Ibn al-Šāṭir's geocentric models to Copernicus' heliocentric ones is, we claim, much simpler than any of the alternatives. In particular, the proposal by Noel Swerdlow (discussed below) does lead to simple heliocentric models, but these are not the actual, computationally viable models we find in the *Commentariolus* or *De rev.*

6) We make no claims about why Copernicus decided to introduce heliocentric models. In particular, we are not claiming that the " heliocentric bias " of Ibn al-Šāṭir's models was the reason behind Copernicus' choice. What we are claiming is that Ibn al-Šāṭir's models were easier to transform into the heliocentric models of the *Commentariolus* and *De rev.* than the other possibilities available to Copernicus.

7) When we say that Ibn al-Šāṭir's models and those in the *Commentariolus* are " quasi-homocentric ", we mean that they eschew eccentrics and depend solely on concentric and epicyclic orbs. Though speculative, we think it is plausible that both Ibn al-Šāṭir and Copernicus in the *Commentariolus* were trying to find a system that had elements of homocentrism while at the same time being more astronomically viable than a purer form of homocentric astronomy.

2. PTOLEMY'S MERCURY MODEL

Ptolemy found Mercury to be the most problematic planet he had to deal with, in part because of the difficulties involved in viewing a planet whose maximum elongation from the Sun is about 28°, in part because of several unfortunate assumptions [16]. Our purpose here, however, is not to critique Ptolemy's methodology or observations but simply to present his model, both as it appears in the mathematical-schematic version in the *Almagest* and in the physical, solid-sphere versions of the Islamic *hay'a* and Latin *theorica* traditions. As is well known, the origins of the latter are to be found in Ptolemy's *Planetary hypotheses*, which was the basis of the *hay'a* tradition and from it the Latin *theoricae* (the *Planetary hypotheses* not being available in Europe in the medieval period).

The model for Mercury as presented in the *Almagest* is represented in figure 3 [17].

There are several things that make Mercury distinctive:

1) Unlike the case of the other four " vacillating " planets (*i. e.*, the ones that exhibit retrogradation), the center of equal motion E is placed closer to the world center O, while the deferent center N, which maintains equal distance R to the epicycle center C, is usually (except once in a cycle) farther away. For the other planets, the order of centers (toward the apogee) is O-N-E.

2) Distinctive among the vacillating planets, but similar to what was done for the Moon, the deferent center is not fixed but moves on a small circle, coinciding with E once every cycle.

3) The result of this configuration is that the epicycle center is not closest to the Earth at 180°, as it is for the other vacillating planets, but at two places, $\pm 120°$, which fulfills his empirical conditions (figure 4 [18]).

So far, we have only discussed the model as presented in the *Almagest*. But in a *hay'a* work, as later in Peurbach's *Theoricae*, the plane geometrical models of the *Almagest* are transformed into full-fledged spherical models in which circles were made into uniformly rotating orbs – fully spherical epicycles that do not surround the Earth or concentric and eccentric hollowed-out spheres that do surround the Earth. Thus a typical *hay'a* illustration for Mercury would appear as figure 5 [19].

[16] For some of these assumptions, see Swerdlow, " Ptolemy's theory of the inferior planets ", p. 43-59.

[17] Figure 3 is a modified version of fig. 11 in Swerdlow, " Ptolemy's theory of the inferior planets ", p. 50.

[18] Figure 4 is a modified version of fig. 13 in Swerdlow, " Ptolemy's theory of the inferior planets ", p. 52.

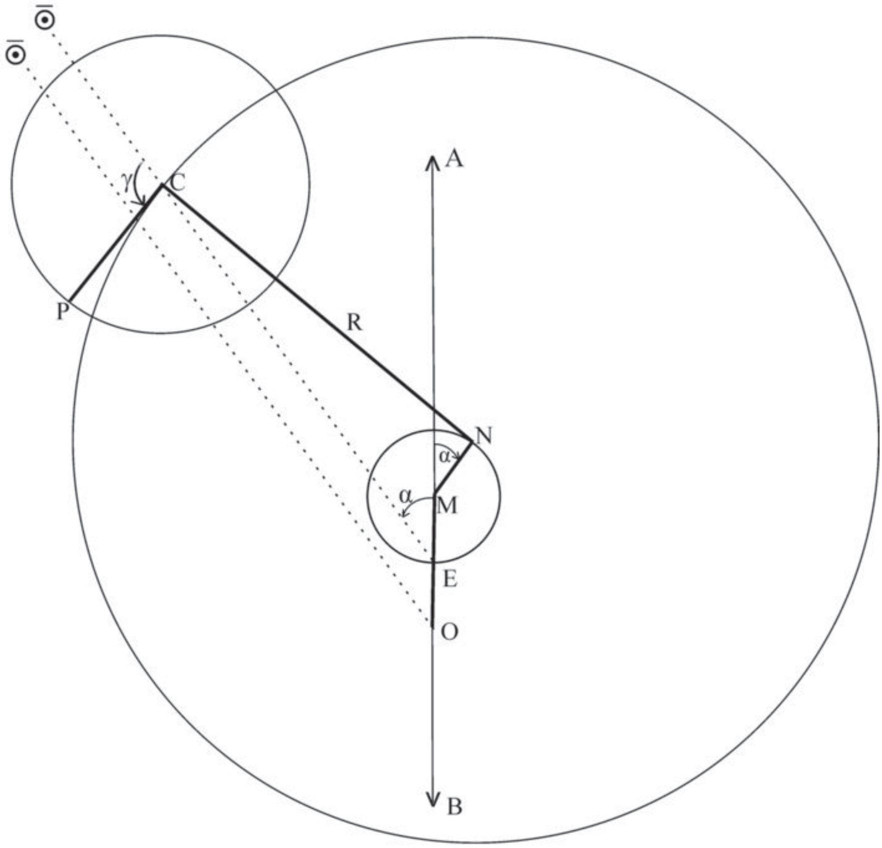
[19] Figure 5 is adapted from S. P. Ragep, *Jaghmīnī's* Mulakhkhaṣ*: An Islamic introduction to Ptolemaic astronomy* (New York, 2016), figure 4, p. 96.

Circle	Radius	Motion
Parecliptic (OE+EM+ MN + NC + CP)	91; 30 parts	$+1°/100$ years
Dirigent (MN)	3 parts	$-0;59,8,17,13,12,31°/\text{day}$ $(-\alpha)$
Deferent (NC $= R$)	60 parts	$+1;58,16,34,26,25,2°/\text{day}$ * (2α)
Epicycle (CP)	22; 30 parts	$+3;6,24,6,59,35,50°/\text{day}$ (γ)

* This is an "average" speed since point N is not the center of the deferent's uniform motion.

Chart 1. Ptolemy's *Almagest* parameters for Mercury (see fig. 3) (plus / minus indicates sequential / counter-sequential zodiacal motion).

There are several points that should be noted. First, Mercury, unlike the models for the upper planets and Venus, has four rather than three orbs (not counting the planet itself). The three orbs in common are the parecliptic (responsible for the motion of the apogee), the deferent (the basis for the mean zodiacal motion), and the epicycle (the source for the synodic motion). But in addition, Mercury has a dirigent (*mudīr*) that causes the deferent center to move on a circle that brings it closer and farther away from the world center. Another feature of the model is that a point on the deferent, and in particular the epicycle center (which is located in the deferent), cuts equal angles in equal times not with respect to the deferent center (as one would expect based on the principle of uniform circular motion in the celestial region), but with respect to the equant point, which is located mid-way between the world center and dirigent center on the apsidal line. There are thus 4 critical points on the apsidal line (this at the initial position, *i. e.*, when the epicycle center is at apogee): the world center, the equant point, the dirigent center, and the deferent center. For Ptolemy in the *Almagest*, the distance between the world center and equant point is 3 parts where the distance from the deferent center to epicycle center is 60 parts; likewise, the distance from the equant point to the dirigent center is 3 parts and the distance from the dirigent center to the deferent center is 3 parts. The upshot of this arrangement and the stipulated motions for the orbs is that the epicycle center will trace an oval-shaped figure in which the nearest approach to the world center (and the Earth) occurs at about 120° and 240°, whereas the farthest distance is, as one would expect, at 0°. (Figure 6 shows how this was illustrated in Ṭūsī's *Taḏkira*; note in particular the explicit designation of the nearest distances at the trines. For the parameters see chart 1.)



α: mean motion
γ: motion of epicycle (Ptolemaic)
A: apogee
B: point opposite apogee
C: epicycle center
E: equant point (about which equal motion of the epicycle center occurs)
M: center of circle about which the deferent center moves
N: deferent / eccentric center
O: world center
P: planet
R: radius of deferent
$\bar{\odot}$: mean Sun

FIG. 3. Ptolemy's Mercury model.

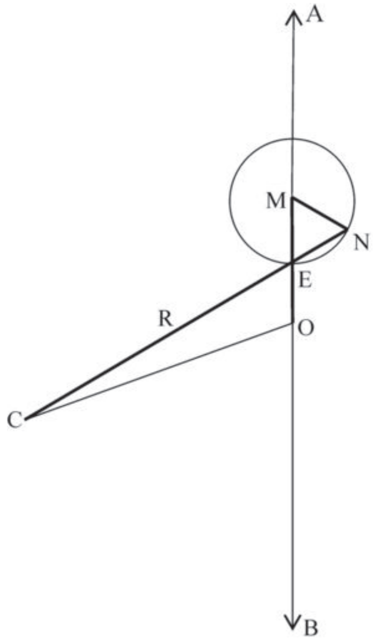


Fig. 4. Epicycle center at first trine.

3. THE MERCURY MODEL OF IBN AL-ŠĀṬIR

As is well known, Islamic astronomers criticized Ptolemy's models from an early period, at least as early as the first half of the eleventh century [20]. The particular form that these criticisms took, leading to such devices as the Eudoxan-couple, the Ṭūsī-couple, the 'Urḍī lemma, Ibn al-Šāṭir's double epicycle device, and their associated models, would seem to have been a particularly Islamicate phenomenon associated mainly with the eastern Islamic world [21]. The main idea was to reform the Ptolemaic system by making it conform to the accepted physics

[20] From this period we have Ibn al-Haytham's remarkable work *Al-Šukūk 'alā Baṭlamyūs* ("Doubts about Ptolemy"), A. I. Sabra and N. Shehaby (ed.) (Cairo, 1971; 2nd ed., Cairo, 1996) as well as the treatise by Abū 'Ubayd al-Jūzjānī, an associate of Ibn Sīnā (for which see G. Saliba, "Ibn Sīnā and Abū 'Ubayd al-Jūzjānī: The problem of the Ptolemaic equant", *Journal for the history of Arabic science*, 4 (1980): 376-403, reprinted in G. Saliba, *A history of Arabic astronomy*, p. 85-112).

[21] For a summary, see G. Saliba, "Arabic planetary theories after the eleventh century AD", in R. Rashed (ed.), *Encyclopedia of the history of Arabic science*, 3 vol. (London, 1996), I,

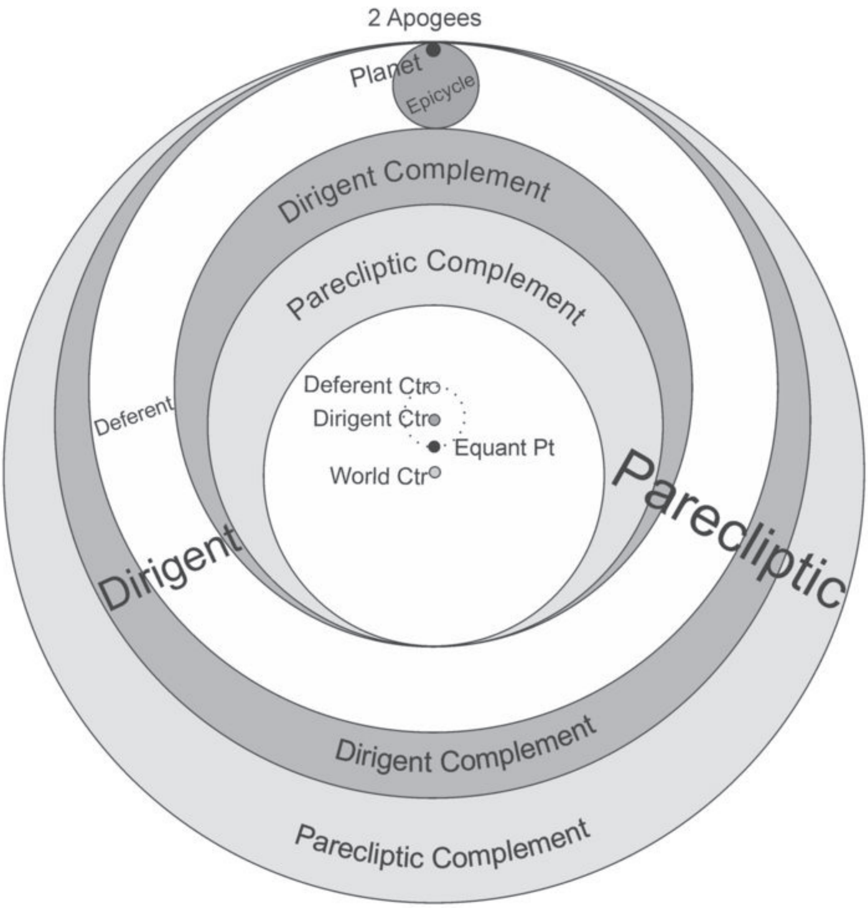


FIG. 5. A *hay'a* model for Mercury.

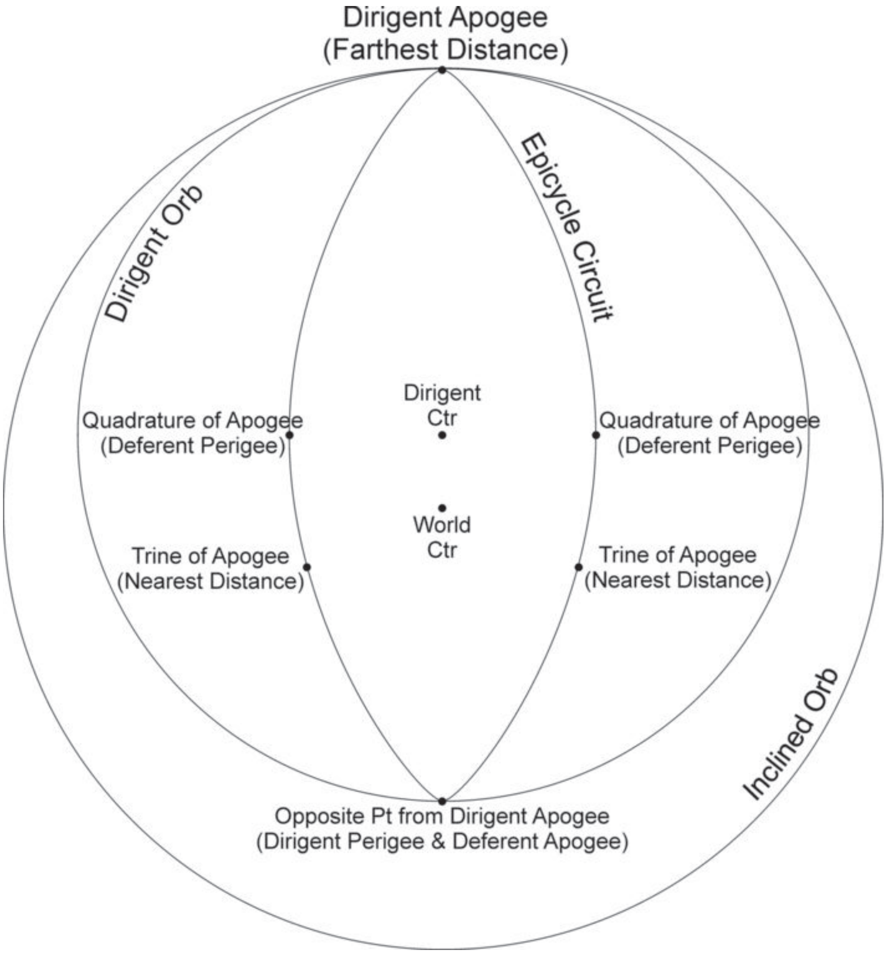


Fig. 6. From Ṭūsī's *Memoir on astronomy*.
[F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's* Memoir on astronomy *(Al-Tadhkira fī 'ilm al-hay'a)*, 2 vol. (New York, 1993), I, 176.]

that required uniform circular motion in the heavens. As such, devices such as the equant were replaced by combinations of uniformly rotating orbs. Now the reason we say that it is an eastern Islamicate phenomenon, contingent on certain intellectual and possibly social and religious trends, is that other examples we have of criticisms of Ptolemy took different approaches. For example, Proclus in his *Hypotyposis* is highly critical of Ptolemy's eccentric and epicyclic models but offers no criticism of the equant and has nothing to offer in the way of alternatives [22]. In the western Islamic world, in particular in twelfth-century al-Andalus, one has a quite dissimilar set of criticisms leading to the homocentric alternative of al-Biṭrūjī that is mostly rejected in the Islamic East [23]. Pre-Copernican alternatives in Europe are either of a far different sort (*e. g.*, those of the fourteenth-century Jewish scholar Levi ben Gerson [24]) or are based on alternatives that clearly can be traced to Islamic precedents (such as those of Ibn Naḥmias, Regiomontanus, and Giovanni Battista Amico [25]). For this reason, Copernicus' criticism of the equant in the introduction to the *Commentariolus*, and his models meant to rectify it, are strikingly innovative within a European context [26].

[22] Proclus, *Procli Diadochi Hypotyposis astronomicarum positionum,* ed. C. Manitius (Leipzig, 1909; reprint, Stuttgart, 1974). For a well-informed analysis of Proclus' attitude toward astronomy (and an important corrective to Pierre Duhem's discussion in his *Σώζειν τὰ φαινόμενα*), see G. E. R. Lloyd, "Saving the appearances", *Classical quarterly*, 28/1 (1978): 202-22, esp. 204-11 (reprinted with new introduction in G. E. R. Lloyd, *Methods and problems in Greek science* [Cambridge, 1991], p. 248-77).

[23] A. I. Sabra, "The Andalusian revolt against Ptolemaic astronomy: Averroes and al-Biṭrūjī", in E. Mendelsohn (ed.), *Transformation and tradition in the sciences: Essays in honor of I. Bernard Cohen* (Cambridge, 1984), p. 133-53, reprinted in A. I. Sabra, *Optics, astronomy and logic: Studies in Arabic science and philosophy*, XV (Aldershot, 1994).

[24] B. R. Goldstein, *The Astronomy of Levi Ben Gerson (1288-1344): A critical edition of chapters 1-20 with translation and commentary* (New York, 1985).

[25] On Ibn Naḥmias, see R. G. Morrison, *The Light of the world: Astronomy in al-Andalus* (Berkeley, 2016). On Regiomontanus, see N. Swerdlow, "Regiomontanus's concentric-sphere models for the Sun and Moon", *Journal for the history of astronomy*, 30/1 (1999): 1-23; M. H. Shank, "The 'Notes on al-Biṭrūjī' attributed to Regiomontanus: Second thoughts", *Journal for the history of astronomy*, 23/1 (1992): 15-30; and M. H. Shank, "Regiomontanus and homocentric astronomy", *Journal for the history of astronomy*, 29/2 (1998): 157-66. For Amico, see N. Swerdlow, "Aristotelian planetary theory in the Renaissance: Giovanni Battista Amico's homocentric spheres", *Journal for the history of astronomy*, 3/1 (1972): 36-48; and M. di Bono, "Copernicus, Amico, Fracastoro, and Ṭūsī's device: Observations on the use and transmission of a model", *Journal for the history of astronomy*, 26/2 (1995): 133-54. See also R. Morrison, "A scholarly intermediary between the Ottoman Empire and Renaissance Europe", *Isis*, 105/1 (2014): 32-57.

[26] Alternatively, Michela Malpangotto has argued that the original motivation for Copernicus' criticism of the equant and his research leading to heliocentrism came from Albert of Brudzewo (d. ca. 1497); "The Original motivation for Copernicus's research: Albert of

On the other hand, Ibn al-Šāṭir is the inheritor of a long tradition of Islamic criticisms of Ptolemy and of the alternatives these gave rise to. Unfortunately, these alternatives are still referred to by the generic term " Marāgha " even though there are few if any models that can be attributed to the years of operation of the Mongol-sponsored Marāgha Observatory (roughly 1260-83); most of the theoretical work of Mu'ayyid al-Dīn al-'Urḍī (d. ca. 1266) and Naṣīr al-Dīn al-Ṭūsī (1201-74) predates their time at the Observatory, and the major astronomical works of Quṭb al-Dīn al-Šīrāzī (d. 1311) were written after he left Marāgha. And we know that there were alternative models long before Marāgha gained prominence as a Mongol capital, and these models continued to be proposed centuries afterwards[27]. So we need to see Ibn al-Šāṭir in the fourteenth century as one of a series of astronomers, spanning six or more centuries, who worked to find models that provided results comparable to those of Ptolemy while adhering to the accepted celestial physics.

Ibn al-Šāṭir, the long-time chief muezzin (*ra'īs al-mu'aḏḏinīn*) and timekeeper (*muwaqqit*) at the Umayyad Mosque in Damascus, was distinctive for a number of reasons[28]. Unlike his " Marāgha " predecessors, he rejected eccentrics, so attempted to base his alternatives on concentrics (orbs whose center was the Earth) and epicycles. He also made strong claims that his work was based on new observations. Unfortunately, his major work in which he claims to have explained the observational basis for his new models, *Ta'līq al-arṣād* (" Explanation of the observations "), is lost to us. What we have is a kind of summary account of his models, contained in a *hay'a* work entitled *Nihāyat al-su'l fī taṣḥīḥ al-uṣūl* (" The culmination of inquiry into correcting the hypotheses "). The Mercury model is presented in chapter 21 of part 1; our translation and edition are in appendices 2 and 3.

Brudzewo's *Commentariolum super Theoricas novas Georgii Purbachii* ", *Archive for history of exact sciences*, 70/4 (2016): 361-411. Though Brudzewo, and earlier Henry of Hesse, do indeed point out the problems related to the equant, it is not entirely clear that this is done with the same motivation of Islamic astronomers who put in place a program for reforming the Ptolemaic system. As Edith Sylla has put it, in response to Malpangotto's contentions regarding the equant: " Contrary to Malpangotto, I think that Peurbach and Brudzewo both accept the idea that there are some physical orbs uniformly rotating and other, purely mathematical methods that do not correspond to bodies. Brudzewo is not disappointed with Peurbach but is elucidating positions with which Peurbach would have agreed ". E. Sylla, " The Status of astronomy as a science in fifteenth-century Cracow: Ibn al-Haytham, Peurbach, and Copernicus ", in Feldhay and Ragep, *Before Copernicus*, p. 45-78 at 78.

[27] For a critique of " Marāgha " as a shorthand for this long tradition, see S. P. Ragep and F. J. Ragep, " The ' Marāgha school ': The myth and its prequel ", forthcoming.

[28] For an overview of his life and works, see S. Nikfahm-Khubravan and F. J. Ragep, " Ibn al-Shāṭir ", *Encyclopaedia of Islam*, 3rd ed., forthcoming.

The model itself consists of seven solid orbs[29] (see figure 7): 1) the parecliptic [r_0]; 2) the inclined [r_1]; 3) the deferent [r_2]; 4) the dirigent [r_3]; 5) the epicycle [r_4]; 6) the enclosing [r_5]; 7) the maintaining [r_6]. $r_0, r_1, \ldots$ designate the radii of the orbs in the schematic version (figure 10); their values are given in chart 2. This allows radii of the "schematic" equators to be considered, somewhat anachronistically, as linked, uniformly rotating vectors[30]. These radii, determined by the planetary parameters, are of the "inner equators" (indicated in dashed lines in figure 8) of the solid orbs, which are parallel to the "outer equators" on the surface of the orb. (For a further explanation of these "inner equators", see Ragep, *Ṭūsī's Memoir on astronomy*, II, 435-6, as well as I, 350-3, fig. C11-C15 for examples.)

Since the parecliptic only causes the slow motion of the apogee (one degree per 60 years[31]), we will ignore it in the subsequent analysis. The combination of the inclined, deferent, and dirigent will result in the apex of the epicycle being displaced by 2α; thus in figure 9, which is a schematic version of figure 7, we note that the epicyclic apex A, which for Ptolemy is on the line from the equant through the epicycle center, has shifted from A_0 to A_1 when $\alpha = 90°$. The enclosing and maintaining orbs will therefore also be 90° from the Ptolemaic "reference apex" A_0 of the epicycle. Practically, this means that the epicycle's daily motion for Ibn al-Šāṭir (as also for Copernicus in *De revolutionibus*) is $\approx 2;7°$ (κ) rather than $\approx 3;6°$ (γ) as it was for Ptolemy. Ibn al-Šāṭir refers to the sum of his epicycle's motion (κ) plus the mean motion of center (α) as the "proper" [*khāṣṣa*] motion of the epicycle, which is the motion of anomaly ($\gamma = \kappa + \alpha$) in the Ptolemaic model.

The final two orbs, the enclosing and maintaining, form a Ṭūsī-couple: in the schematic model (figure 9) they are the same size but in the full, solid-sphere

29 Solid here refers to the substance of the orbs, whereby other bodies are precluded from moving through them. Of course, another solid body can be embedded within a solid orb; *e. g.*, an epicycle is embedded within a deferent orb, each rotating with its own motion. As one can see in figure 7, all of Ibn al-Šāṭir's orbs (except for the planet itself) contain one or more orbs embedded within them.

30 Kennedy and Roberts, "The Planetary theory of Ibn al-Shāṭir", p. 231, fig. 2.

31 Earlier in the *Nihāya*, Ibn al-Šāṭir cites $1°/100$ years (Ptolemy) as well as $1°/\ 66\,{}^2/_3$ years and $1°/70$ years (the "Moderns") as possible values for precession (part I, ch. 3 and ch. 5), which one would expect to be equivalent to the motion of apogee. In fact, in *Al-Zīj al-jadīd*, Ibn al-Šāṭir tells us that the motion of the apogees for all the planets is $1°/60$ years, whereas the precessional motion is $1°/70$ years. He claims the proof can be found in his *Taʿlīq al-arṣād,* which unfortunately is not extant. See Leiden ms. Or. 65, f. 49b.

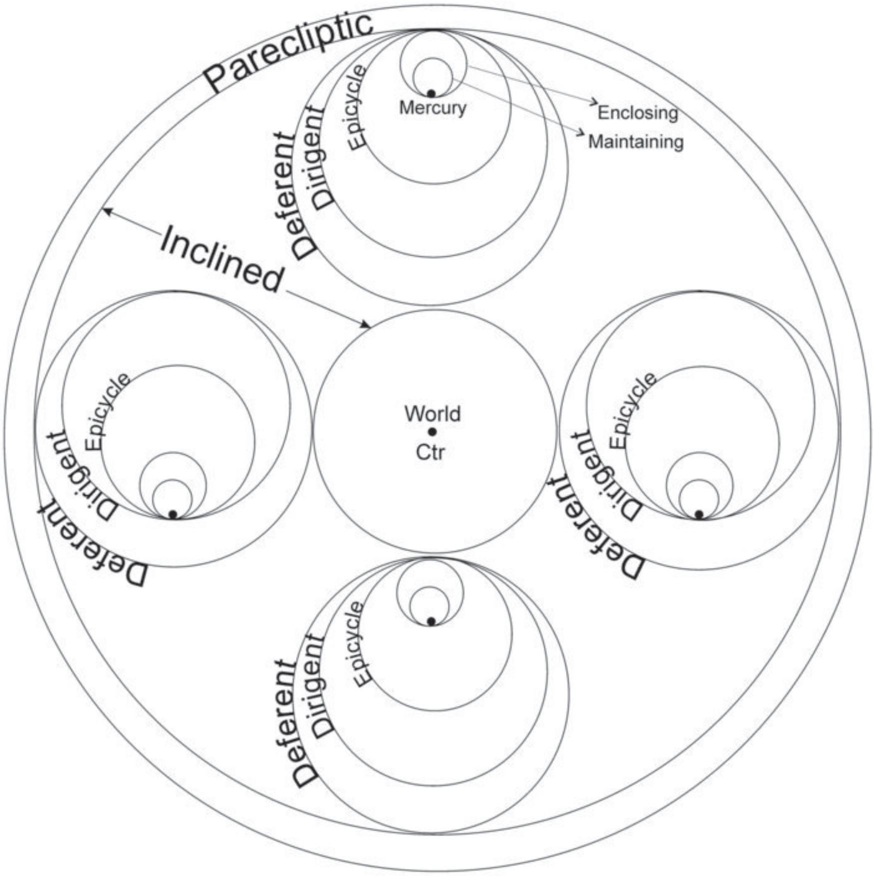


Fig. 7. Ibn al-Šāṭir's Mercury model
(solid-orb version at four different positions).

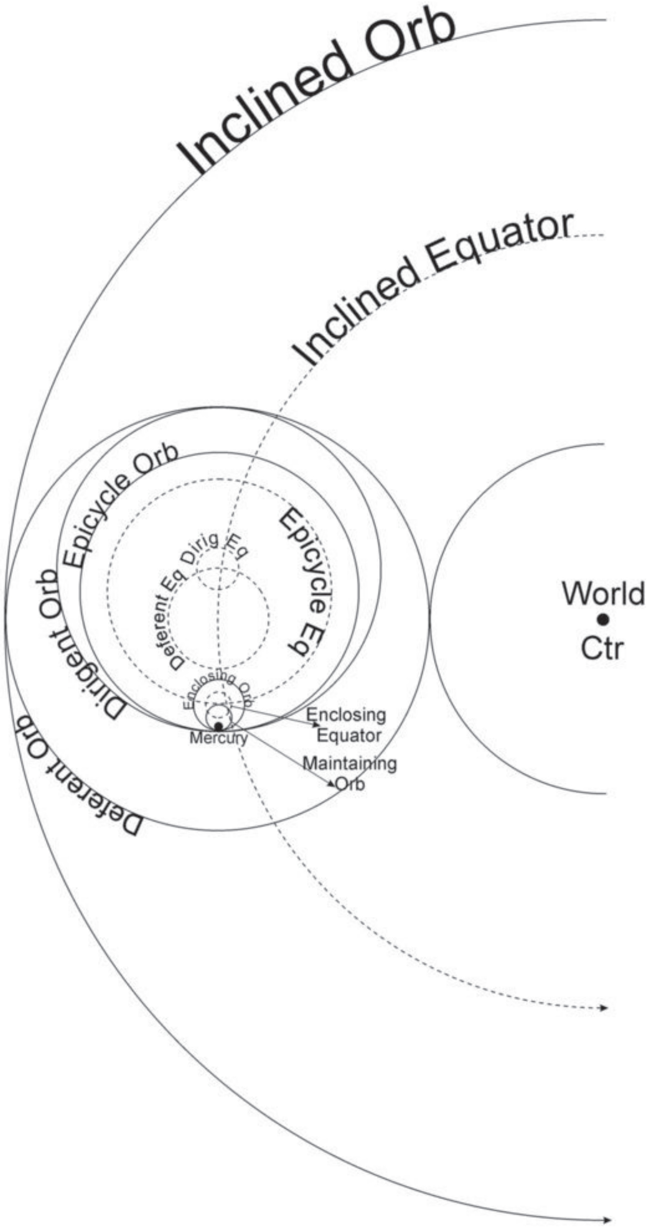
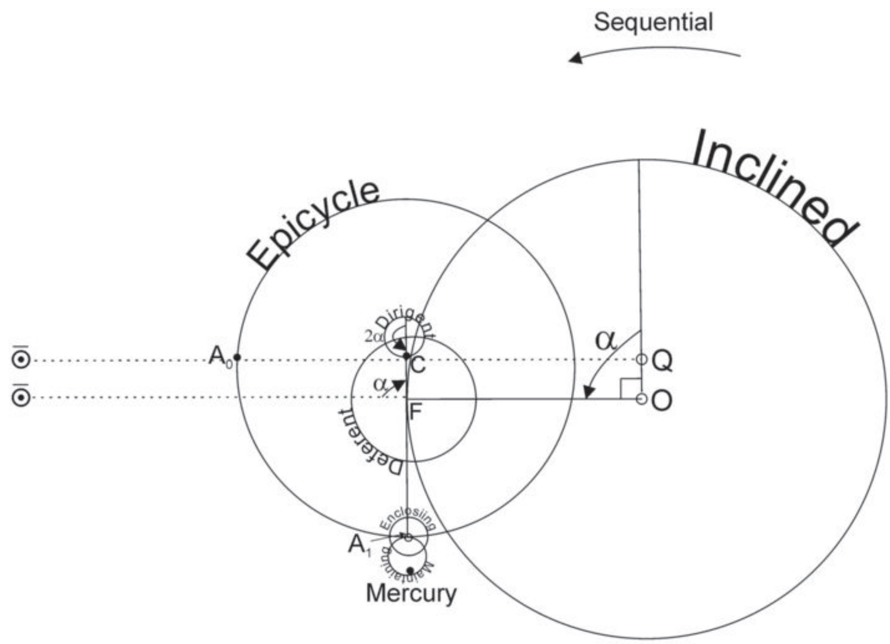


FIG. 8. "Inner equators" (in dashed lines) of the solid orbs. (Not to scale.)



C: epicycle center
F: deferent center
O: world center
Q: transposed Ptolemaic equant
A_0: $(\text{Apex})_0$
A_1: $(\text{Apex})_1$

FIG. 9. Ibn al-Šāṭir's Mercury model when $\alpha = 90°$ (without motion of epicycle; not to scale). The circles in this figure are the inner equators of fig. 8.

version (figure 7) they are in the ratio of 2:1 [32]. Because Mercury is embedded in the maintaining orb, it will oscillate on a straight line toward and away from the center of the epicycle. Note that the line (OF) connecting the world center and the deferent center is in the direction of the mean Sun, a point of considerable importance to which we shall return.

Let us now turn to the parameters. For the outer planets, Ibn al-Šāṭir seems to have adopted the Ptolemaic eccentricities *e* and then made the deferent radius $r_2 = 3e/2$ and the dirigent radius $r_3 = e/2$ [33]; the ratio $r_2 : r_3$ is then 3. For Venus, however, the ratio $r_2 : r_3 \approx 3.9$ rather than 3 [34]. For Mercury, the ratio is $r_2 : r_3 \approx 4.45$ [35]. One of the consequences for Mercury is that this results in considerably different amounts for the extremal distances. For Ptolemy, at apogee the distance between the world center and epicycle center is 69; at 180°, it is 57. For Ibn al-Šāṭir the corresponding distances are 65 and 55. We cannot give a satisfactory reason for these differences [36], which result in different elongations, as we will discuss below.

Chart 2 provides a list of Ibn al-Šāṭir's various schematic orbs (the "inner equators" or non-physicalized versions) for Mercury, their sizes and their motions, and a comparison with Copernicus' values (in both the *Commentariolus* and *De rev*.). For Copernicus, we follow Kennedy and Roberts in designating his orbs by vectors and norming r_1 to 60. Positive values for motions of orbs are in the sequence of the signs with respect to the apogee or epicyclic apex; negative values are counter-sequential [37].

[32] Although mathematically equivalent, the equal-circle model (presented by Copernicus in *De revolutionibus* in III.4 and by Ṭūsī in his *Taḥrīr al-Majisṭī*) is distinct from the 2:1 model (used in the *Commentariolus* and in Ṭūsī's *Taḏkira*); see also note †† in chart 2 below. The importance of distinguishing them for understanding the historical relationship between the various models had already been pointed to by M. di Bono, "Copernicus, Amico, Fracastoro and Ṭūsī's device"; see also Ragep, "From Tūn to Toruń".

[33] As explained by Kennedy and Roberts, this is so that $r_2 - r_3 = e$ and $r_2 + r_3 = 2e$, the two conditions needed to satisfy the necessary distances at apogee, perigee and quadratures ("The Planetary theory of Ibn al-Shāṭir", p. 230).

[34] $r_2 = 1;41$ and $r_3 = 0;26$.

[35] $r_2 = 4;5$ and $r_3 = 0;55$.

[36] To quote Kennedy and Roberts (referring to Venus): "We are at a loss to explain these new constants". Kennedy and Roberts, "The Planetary theory of Ibn al-Shāṭir", p. 231; their chart on p. 230 conveniently lists the parameters for r_2, r_3, and r_4 for all the planets. One might speculate, as does Hartner, that Ibn al-Šāṭir is basing himself on new observations, but this must remain speculation as long as we do not have *Taʿlīq al-arṣād*. Cf. Hartner, "Ptolemy, Azarquiel, Ibn al-Shāṭir, and Copernicus on Mercury", p. 24-5; repr. p. 311-12. For a recent attempt to reconstruct Ibn al-Šāṭir's observations for Mercury, cf. Penchèvre, "La *Nihāya al-sūl*", p. 492-3.

[37] Cf. Kennedy and Roberts, "The Planetary theory of Ibn al-Shāṭir", p. 230.

Name of orb	Ibn al-Šāṭir radius; motion	Copernicus (*Comm.*) radius; motion	Copernicus (*De rev.*) radius; motion
Parecliptic (r_0)	0; 38 parts [thickness]; $+1°/60$ years	N / A	N / A
Inclined (r_1)	60 parts; $+0; 59, 8, 10°$/day * ($+\alpha$)	60 parts; $+0; 59, 8, 11, 14°$/day † ($+\alpha$)	60 parts; $+0; 59, 8, 11, 22°$/day ‡ ($+\alpha$)
Deferent (r_2)	4; 5 parts; $-0; 59, 8, 10°$/day ($-\alpha$)	4; 2, 24 parts; $-0; 59, 8, 11, 14°$/day ($-\alpha$)	4; 25 parts; fixed
Dirigent (r_3)	0; 55 parts §; $+1; 58, 16, 20°$/day ($+2\alpha$)	1; 20, 48 parts ¶; $+1; 58, 16, 22, 28°$/day ($+2\alpha$)	1; 16 parts; $+1; 58, 16, 22, 44°$/day ($+2\alpha$)
Epicycle (r_4)	22; 46 parts; $+2; 7, 16, 0°$/day ‖ ($\kappa = \gamma - \alpha$)	22; 33, 36 parts; $+4; 5°$/day ** ($\gamma + \alpha$)	22; 35 parts $+2; 7, 16, 2, 18°$/day ($\kappa = \gamma - \alpha$)
Enclosing (r_5)	0; 33 parts; $+1; 58, 16, 20°$/day ($+2\alpha$)	0; 34, 48 parts ††; $+1; 58, 16, 22, 28°$/day ($+2\alpha$)	0; 34, 12 parts; $+1; 58, 16, 22, 44°$/day ($+2\alpha$)
Maintaining (r_6)	0; 33 parts; $-3; 56, 32, 39°$/day (-4α)	0; 34, 48 parts; $-3; 56, 32, 44, 56°$/day (-4α)	0; 34, 12 parts; $-3; 56, 32, 45, 28°$/day (-4α)

* In book I, ch. 7 of *Nihāyat al-su'l*, the value is given as $0; 59, 8, 9, 51, 46, 57, 32, 3°$. This is the Sun's tropical mean motion.

† This is based on the value Copernicus gives for the Sun's sidereal year, "365 days, 6 hours, and about $1/6$ of an hour". In his copy of the 1515 *Almagest*, Copernicus gives the year as $365; 15, 24, 45$, which translates to a daily motion of $0; 59, 8, 11, 16, 12°$; see Swerdlow, "The Derivation and first draft", p. 451-4.

‡ This is sidereal.

§ Although the value is given initially as $1/2$ plus $1/3$ of a degree, *i. e.*, as 0; 50 parts, the amount that is used later in the calculations is 0; 55 parts.

¶ This is exactly $1/3$ of the deferent; Copernicus gives a slightly different value, 1; 21, 36 [0; 34 using $R = 25$], but this is rounded. Swerdlow more precisely derives $1; 41^1/4$ and $0; 33^3/4$ [$R = 25$] from the Uppsala manuscript; see Swerdlow, "The Derivation and first draft", p. 509.

‖ All the manuscripts have $2; 18, 14, 2°$, which is incorrect.

** Because of the particular way in which Copernicus places his orbs, this is equal to the motion of Ptolemy's epicycle ($\approx 3; 6°$/day) *plus* the motion of center ($\approx 0; 59°$/day). Note that for Ibn al-Šāṭir and for Copernicus in *De rev.*, the orb's own rotation is the motion of Ptolemy's epicycle ($\approx 3; 6°$/day) *minus* the motion of center ($\approx 0; 59°$/day).

†† In the *Commentariolus*, Copernicus describes a spherical version of the rectilinear Ṭūsī-couple, which is what Ibn al-Šāṭir uses in the solid-sphere version of his Mercury model (figure 7); for ease of comparison, we have transformed this for r_5 and r_6 into the mathematically equivalent equal-circle version of the Ṭūsī-couple (figure 9). Note that Copernicus states that the motions of r_5 and r_6 are completed in a tropical year rather than a sidereal year, another indication that the *Commentariolus* model was originally geocentric; Swerdlow, "The Derivation and first draft", p. 503, 505.

CHART 2. Comparison of Ibn al-Šāṭir's and Copernicus' values for Mercury.

In figure 10, we see the complete schematic model of Mercury when $\alpha = 35°$ (about 35.5 days). Note that the planet P has moved about 145° ($2\alpha + \kappa$) from a fixed reference point A on the epicyclic diameter parallel to the apsidal line [38], about 110° ($\alpha + \kappa$) from A_0, the initial position of the epicycle apex, and about 75° (κ) from A_1, the transposed position of the epicycle apex. From Ibn al-Šāṭir's parameters, we can calculate the sidereal period to be 87.97 days, the synodic period to be 115.88 days, the latter the same as Ptolemy's. Ibn al-Šāṭir differentiates between what he calls the true epicycle, *i. e.*, a reference epicycle whose size is invariable, and an apparent epicycle, whose size is constantly changing due to the effect of the Ṭūsī-couple, which brings the planet toward and away from the epicycle center. (See figure T1 [appendix 2: Translation] for his illustration.) We will have more to say about the true and apparent epicycles below.

How well does Ibn al-Šāṭir's model replicate Ptolemy's results, in particular for the maximum elongations? Ibn al-Šāṭir's maximum elongations Δ can be obtained from the following formula [39]:

$$\sin(\Delta) = \frac{r_4 - 2 \cdot r_5 \cdot \cos(2\alpha)}{\left[r_1^2 + r_2^2 + r_3^2 + 2 \cdot r_1 \cdot (r_2 + r_3) \cdot \cos(\alpha) + 2 \cdot r_2 \cdot r_3 \cdot \cos(2\alpha)\right]^{1/2}}.$$

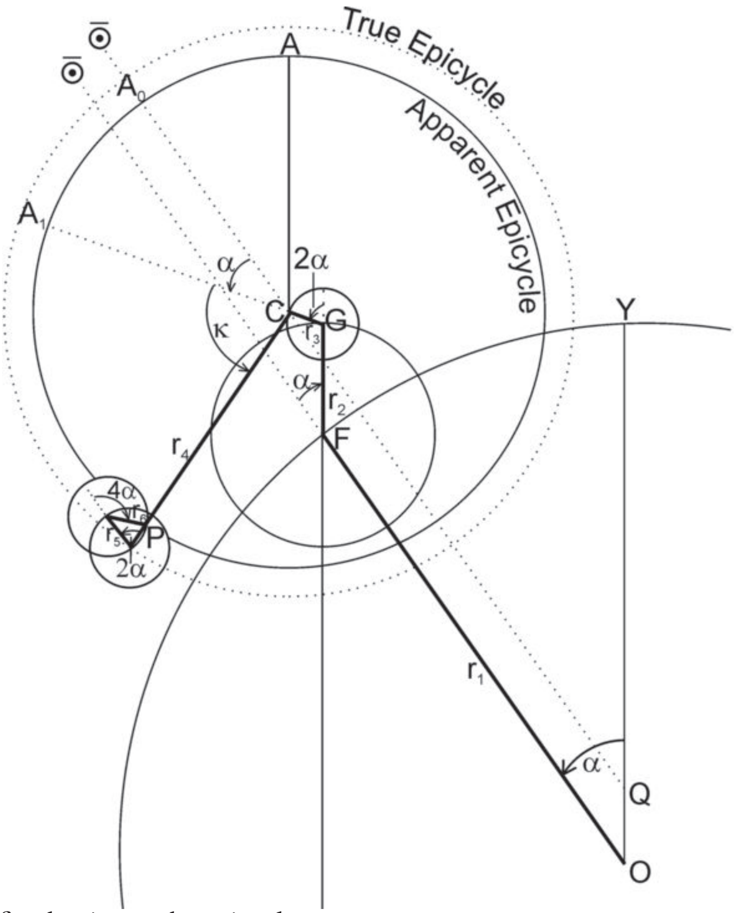
At the critical centrum values of 0°, 90°, and 180°, we find that Ibn al-Šāṭir's values are somewhat different from those of Ptolemy. Note the differences in chart 3 [40].

On the other hand, Ibn al-Šāṭir's value for greatest maximum elongation ($23; 53, 48$ at $117; 51°$) is remarkably close to Ptolemy's ($23; 53, 20$ at $120; 28°$). That Ibn al-Šāṭir's greatest elongation occurs near $117; 50$, whereas Ptolemy's is around $120; 30$, is due to the equation of center ($\approx 2; 40$); recall that uniform motion of center for Ptolemy is about the equant, whereas it is about the Earth for Ibn al-Šāṭir. From the Earth, the two models would thus predict almost the same maximum elongation at the same distance from the apogee. It would seem that Ibn al-Šāṭir attempted to match Ptolemy's greatest maximum elongation while being less concerned about the values for 0°, 90°, and 180° (see chart 4). It is not

[38] We ignore here the motion of the apsidal line due to the parecliptic.

[39] The numerator is the apparent radius of the epicycle (the true radius modified by the couple). The denominator is the distance of the center of the epicycle from the Earth. This latter distance formula can also be found in Hartner, "Ptolemy, Azarquiel, Ibn al-Shāṭir, and Copernicus on Mercury", p. 10; repr. p. 297.

[40] These values are different from those reported by Hartner, because he took $r_3 = 0; 50$, whereas we are using 0;55 based on textual evidence (Hartner, "Ptolemy, Azarquiel, Ibn al-Shāṭir, and Copernicus on Mercury", p. 23; repr. p. 310); see also note § in chart 2 above. For our calculations, we used a modern calculator; the differences using Ibn al-Šāṭir's sine table would be insignificant.



A: the fixed point on the epicycle
A_0: initial point of epicyclic apex
A_1: transposed apex
C: epicycle center
F: deferent center
G: dirigent center
O: world center
P: planet
Q: transposed Ptolemaic equant
Y: the point on the inclined orb toward the apogee
α: motion of center
κ $(= \gamma - \alpha)$: motion of Ibn al-Šāṭir's epicycle (equals motion of Ptolemaic epicycle minus motion of center)

Fig. 10. Complete schematic version of Ibn al-Šāṭir's Mercury model.

Centrum	Elongation	
	Ibn al-Šāṭir	Ptolemy and *De rev.*
0	19; 28, 16	19; 03
90	23; 24, 17	23; 15
180	23; 11, 59	23; 15

CHART 3. Comparison of elongation values.

clear whether he is oblivious (or perhaps indifferent) to the discrepancies brought about by the parameters needed to duplicate the value for 120° or whether he has new observations for apsides and quadratures. In any event, it is clear that Ibn al-Šāṭir has taken Ptolemy's closest distances at 120° and 240° quite seriously when assigning the parameters to his Mercury model. This will be an important consideration when we compare his model and approach to that of Copernicus in the *Commentariolus* and in *De revolutionibus*[41].

4. RELATION OF IBN AL-ŠĀṬIR'S MODELS TO THOSE OF COPERNICUS

As we stated at the outset, the most remarkable aspect of Copernicus' Mercury model in *De revolutionibus* is its virtual equivalence to Ibn al-Šāṭir's and the simple transformation needed to go from a geocentric to heliocentric version. To see what is involved, we turn to figure 11, which is a modified version of figure 2. Ibn al-Šāṭir's model is indicated using dashed lines (for which compare figures 1 and 10). The *De rev.* version is indicated with solid lines. The transformation is effected simply by bringing the mean Sun from its position on line OF in Ibn al-Šāṭir's model to point F. This instantly gives us what we might call the " Tychonic " version of the model. Everything is as it was in Ibn al-Šāṭir's model except that now the Sun moves about the Earth on circle OF counterclockwise. To complete the transformation to the *De rev.* model, one simply has the Earth move about the mean Sun on its orb / orbit FO in the counterclockwise direction.

[41] It is worth mentioning here that Copernicus in *De revolutionibus* was somewhat more successful in duplicating Ptolemy's maximum elongations at 0°, 90°, and 180° as indicated in our chart 3 (cf. Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 420). Since the Mercury model there is mathematically and astronomically equivalent to Ibn al-Šāṭir's model, we must conclude that either Ibn al-Šāṭir was unable to figure out how to adjust his parameters to achieve equivalence with Ptolemy (which seems unlikely), or he chose, for some reason, not to do so.

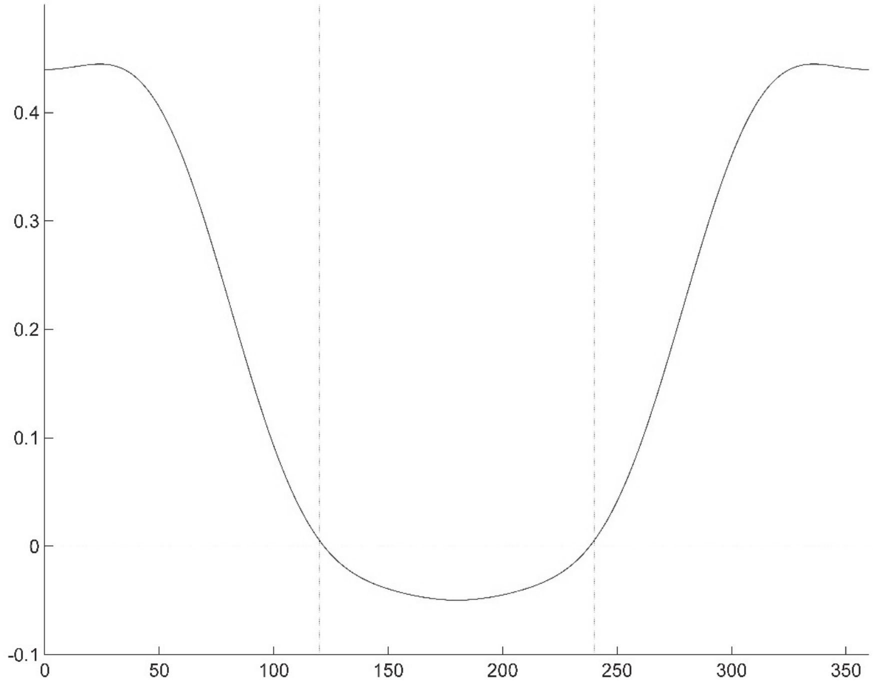


Chart 4. Deviation of Ibn al-Šāṭir's maximum elongations from Ptolemy's (x-axis is the centrum; y-axis is deviation in degrees [Ibn al-Šāṭir minus Ptolemy]).

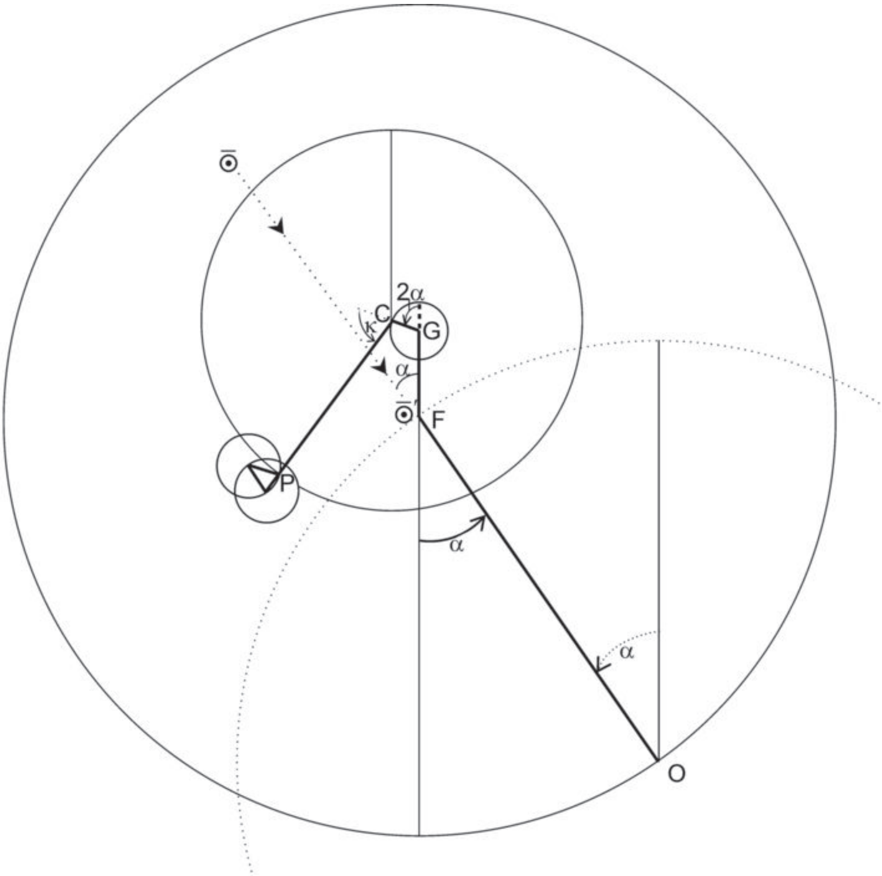


Fig. 11. Transformation of Ibn al-Šāṭir's Mercury model to the *De rev.* model.

Everything else remains exactly as before.

We maintain that this virtual equivalence between Ibn al-Šāṭir's quite complex Mercury model and Copernicus' *De rev.* model, which also holds for Venus, is compelling evidence that Copernicus depended on his Islamic predecessor for his models of the inner planets. Given the straightforward transformations needed to go from Ibn al-Šāṭir's models for the outer planets to Copernicus' models (outlined by Ragep in " Ibn al-Shāṭir and Copernicus "), we further maintain that Copernicus' models are all simple adaptations of Ibn al-Šāṭir's models.

Viktor Blåsjö and Noel Swerdlow have taken issue with this claim. Blåsjö argues that resemblances between models do not indicate proof of transmission or influence, since there are " natural " solutions to the problems posed by Ptolemy's models. Swerdlow does not deny that Copernicus had Ibn al-Šāṭir's models; rather, he does not think they are sufficient to explain Copernicus' various models nor his transition to a heliocentric cosmos. He insists instead that Copernicus was also dependent on Regiomontanus' alternative eccentric models. Blåsjö's arguments about "naturalness" are generally lacking in historical evidence, but he does point to an illuminating mistake in Swerdlow's understanding of the Mercury model that will figure in our own analysis. We deal with Blåsjö's other arguments regarding Mercury in appendix 1. As for Swerdlow's criticisms of Ragep's claims in "Ibn al-Shāṭir and Copernicus", which are central to this paper as well, we take them up in the subsequent discussion.

There is an important caveat to our argument regarding Copernicus' simple transformation of Ibn al-Šāṭir's Mercury model: this only works for *De revolutionibus*. In the earlier *Commentariolus*, the Mercury model exhibits a number of differences with the *De rev.* model, the most important being that the mean Sun and the center of Mercury's orb / orbit are coincident in the earlier work. Since we believe, like Swerdlow, that Copernicus had Ibn al-Šāṭir's Mercury model when writing the *Commentariolus*, we need to show how one might get to the latter from the former. We begin with a geocentric transformation of Ibn al-Šāṭir's model (figure 12), using a simplified version that dispenses with the Ṭūsī-couple. (Thus it is similar to, but not exactly the same as, the Venus model.) In order to show the transformation more clearly, we again make $\alpha = 35°$, $\kappa \approx 75°$, both motions starting at A.

The transformation consists of the following steps: 1) transpose the epicycle so that its center C is now at F; 2) transpose the double epicycle FGC along line FF′, which is parallel and equal to CP. Note that O and P are not moved, and they retain the same relationship as before. However, P is no longer on the epicycle.

Using vectors, we can see that we have made the following transformation, which has preserved both distance and direction between the Earth and the planet: $\overrightarrow{\mathrm{OF}} + \overrightarrow{\mathrm{FG}} + \overrightarrow{\mathrm{GC}} + \overrightarrow{\mathrm{CP}} = \overrightarrow{\mathrm{OC'}} + \overrightarrow{\mathrm{C'F'}} + \overrightarrow{\mathrm{F'G'}} + \overrightarrow{\mathrm{G'P}}$. Using the symbols for the radii of the orbs from chart 2, we have $\overrightarrow{r_1} + \overrightarrow{r_2} + \overrightarrow{r_3} + \overrightarrow{r_4} = \overrightarrow{r_1} + \overrightarrow{r_4} + \overrightarrow{r_2} + \overrightarrow{r_3}$.

It is then simple to transform this adaptation of Ibn al-Šāṭir's geocentric model into the heliocentric model of the *Commentariolus* (figure 13). Copernicus recognized the need to add a Ṭūsī-couple to vary the size of the epicycle, which has now become Mercury's deferent orb around the Sun. It may not be coincidental that Copernicus follows our reconstruction, first presenting the model without the couple (as in figure 12) and then justifying and adding the couple. In the *Commentariolus* model, as well as in Ibn al-Šāṭir's and *De rev.*'s models, the purpose of the couple is to vary the size of the epicycle or Mercury's orbit; we will have more to say about this below. However, unlike Ibn al-Šāṭir's model as well as the *De rev.* model, the Ṭūsī-couple produces this effect in the *Commentariolus* by bringing the center of the orb F′, rather than the planet, away from

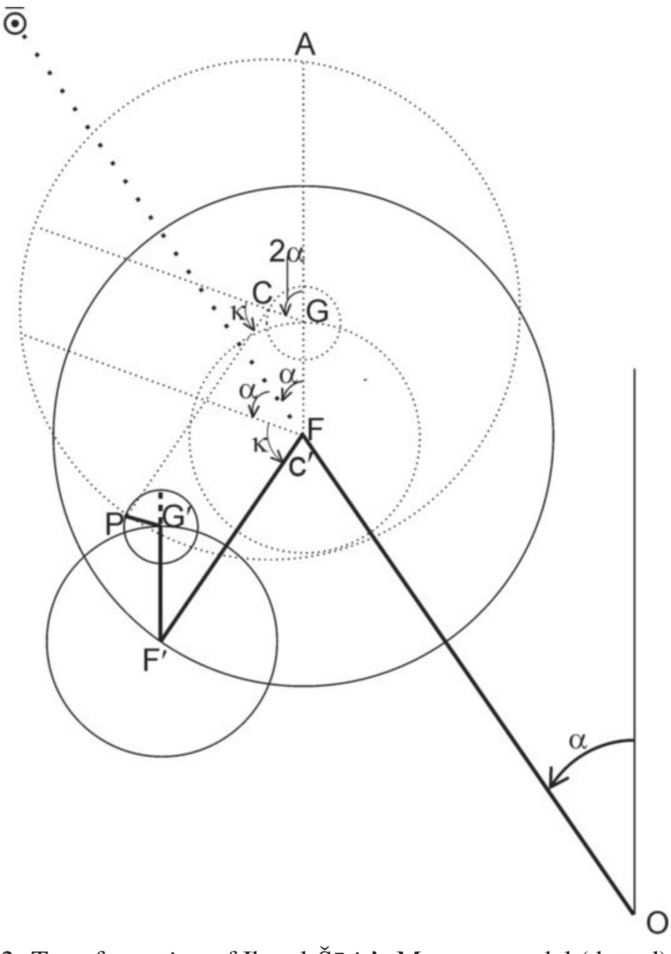


Fig. 12. Transformation of Ibn al-Šāṭir's Mercury model (dotted) to the geocentric version of the *Commentariolus* model.

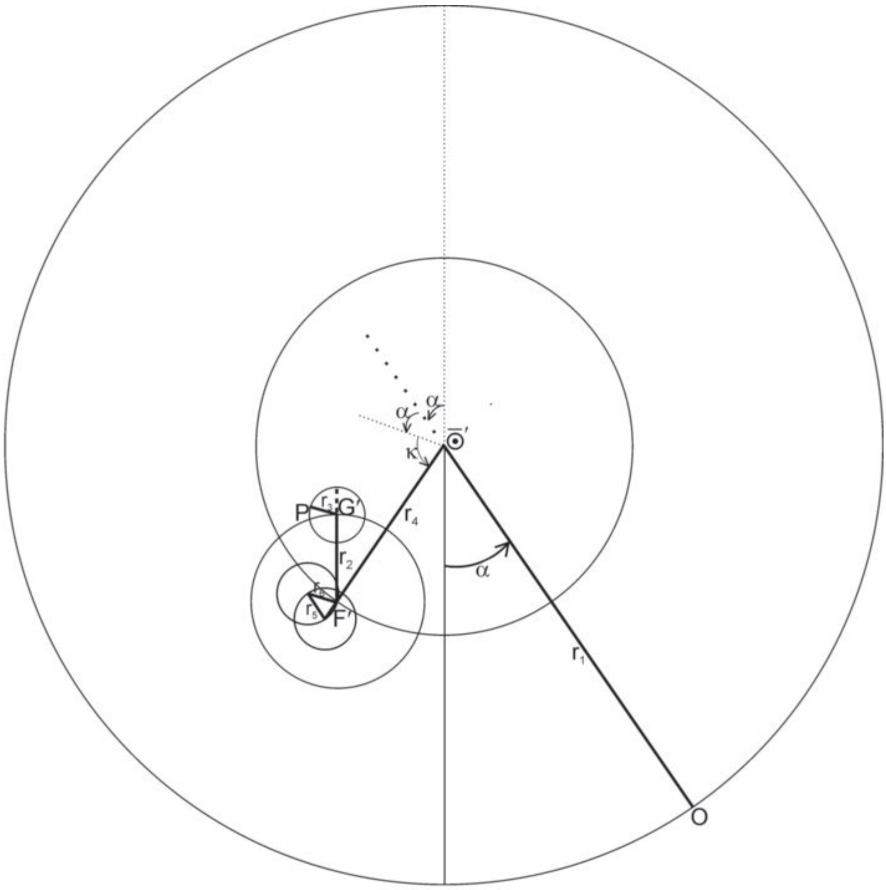


FIG. 13. Final transformation of Ibn al-Šāṭir's Mercury model to the heliocentric version in the *Commentariolus*.

and toward the epicycle center C′. Thus rather than $\overrightarrow{r_1} + \overrightarrow{r_2} + \overrightarrow{r_3} + \overrightarrow{r_4} + \overrightarrow{r_5} + \overrightarrow{r_6}$, we now have the mathematically equivalent (but astronomically different) $\overrightarrow{r_1} + \overrightarrow{r_4} + \overrightarrow{r_5} + \overrightarrow{r_6} + \overrightarrow{r_2} + \overrightarrow{r_3}$.

There are several other things to note here. First, both for the *Commentariolus* model and especially for the *De rev.* model, the "heliocentric bias" of Ibn al-Šāṭir's model, whereby the Sun is on the line from the Earth to the center of the primary deferent (the "inclined"), which line defines the motion of center, greatly facilitates the transformation from geocentric to heliocentric versions of the model. (This is discussed at length in Ragep, "Ibn al-Shāṭir and Copernicus".) The second thing to note is that the distinctive character of Ibn al-Šāṭir's double epicycle model is preserved in both the *Commentariolus* and *De rev*. And finally, despite the less straightforward transformation of Ibn al-Šāṭir's model in the *Commentariolus*, nothing about the transformation would have been beyond the capabilities of Copernicus.

But then the inevitable question: if Copernicus had Ibn al-Šāṭir's Mercury model at the time of writing the *Commentariolus*, why perform the above, rather involved transformation instead of the simple transformation that leads to the *De rev.* model? Here we need to speculate a bit, but only a bit. The *Commentariolus* models have several underlying conditions: 1) exactly as with Ibn al-Šāṭir, there are no eccentrics, only epicycles and concentric orbs; 2) the mean Sun lies at the center of the main deferent orb for each of the planets, this corresponding to Ibn al-Šāṭir's deferent center F (figure 10) that is on the line from the Earth to the mean Sun. It would seem that Copernicus in the *Commentariolus* wanted to follow Ibn al-Šāṭir, even if this led to serious practical difficulties, especially with Venus and Mercury (see below). It may also be the case that Copernicus, when writing the *Commentariolus,* was under the influence of the Paduan Averroists and saw Ibn al-Šāṭir's models, with their eschewing of eccentrics and the potential of a return to single, Aristotelian center, as a way to achieve a " quasi-homocentricity " [42].

5. SOME PRACTICAL PROBLEMS WITH THE *COMMENTARIOLUS* MODELS AND THE TRANSITION TO THE *DE REV.* MODEL(S)

As mentioned above, the Mercury model in the *Commentariolus* is mathematically equivalent to that of Ibn al-Šāṭir and the *De rev.* model. But mathematical equivalence here obfuscates a number of serious consequences to this reconfiguration of the model. (On the issue of " equivalent " models, see appendix 1.) First of all, there is no longer an obvious " equation of center ", *i. e.*, an angle defined by the Earth – epicycle center – equant. Swerdlow was able to define one (δ_1) at a constructed point Q (see figure 14), but this means the equation of center is no longer defined by the Earth / observer, an extraordinary departure from past practice. This alone would make finding the true position of the planet quite difficult for someone with Copernicus' mathematical toolkit, as would finding the elongation ($\delta_2 - \delta_1$) for any given centrum α, which is essential for finding the longitude for one of the lower planets. But even more challenging would be finding the *maximum* elongation of the planet for any centrum; since the planet is no longer on a defined circle to which one could draw a tangent line, the calculation involves first locating the planet with the awkward equation of center

[42] Ragep, " Ibn al-Shāṭir and Copernicus ", p. 408-9.

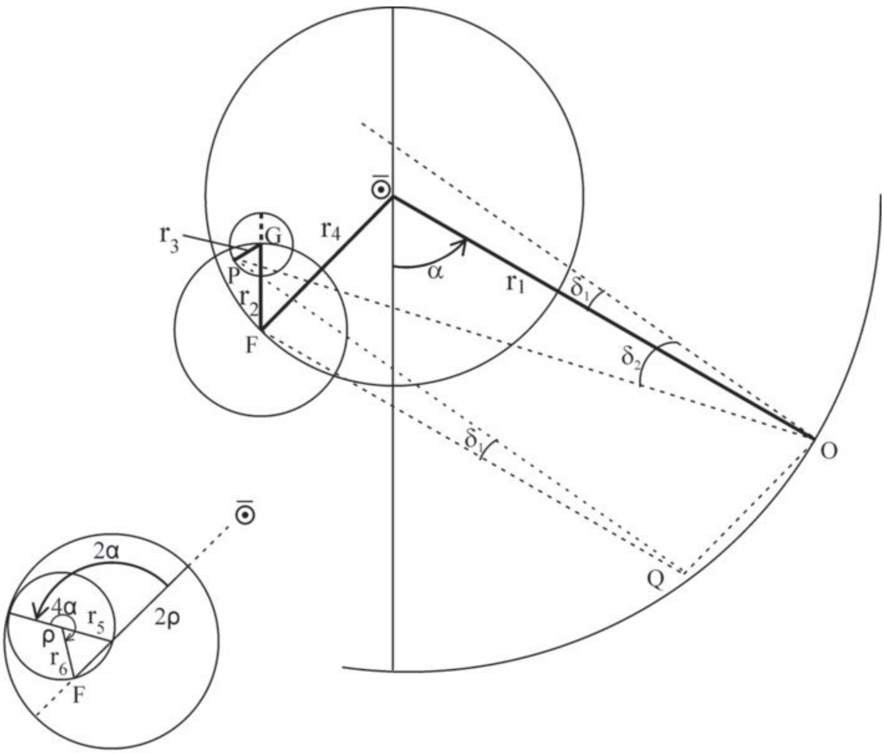


Fig. 14. The Mercury model in the *Commentariolus* (adapted from Swerdlow, "The Derivation and first draft", fig. 39, p. 501)*.

* Note that the mean motion α and the starting point of F are different from our figures 12 and 13.

and then rotating it through 360° to find the greatest maximum elongation for any centrum.

But even if we grant "mathematical equivalence" in theory, the fact remains that Copernicus was unable to derive parameters that would make the *Commentariolus* model "work". That Copernicus himself would have found using his model computationally challenging is made clear from the values that it generates. For example, the maximum equation of center is considerably off from that of Ptolemy, as also from Ibn al-Šāṭir and the *De rev.* model, as we see in chart 5. The maximum elongations tell a similar, though less dramatic, tale (see chart 6).

Part of the problem in the *Commentariolus* is that Copernicus retains the 3:1 ratio of the deferent to dirigent epicycles from the outer planets and fails to ad-

Ptolemy	Ibn al-Šāṭir	*Commentariolus*	*De revolutionibus*
3; 1, 45	3; 1, 53	2; 34, 4	3; 1, 7

CHART 5. Mercury's maximum equation of center. (It is not surprising that Swerdlow declares the *Commentariolus* value to be "absurd"; Swerdlow, "The Derivation and first draft", p. 509.)

Ptolemy (at 120; 28)	Ibn al-Šāṭir (at 117; 51)	*Commentariolus* (at 118)	*De revolutionibus* (at 120; 47, 28)
23; 53, 20	23; 53, 48	23; 47, 56	23; 51, 45

CHART 6. Mercury's maximum elongations.

just it as is done by both Ibn al-Šāṭir and the later Copernicus in *De rev.* [43]. So Copernicus here was either not interested or incapable of testing his parameters (in contrast to what he does in *De rev.*) [44]. For the equation of center this is particularly striking, since he should have been able to derive the quantity. For the maximum elongation, we very much doubt that he or any of his contemporaries could have derived the value without an extraordinary amount of effort. We are thus left with a model that is deeply flawed and almost impossible to test.

When Copernicus came to work seriously on what would become *De revolutionibus*, the inadequacies of his earlier models must have become all too apparent, which led him to abandon his earlier attempts to exclude eccentrics and have a single center for each planetary system. Copernicus was still working on Mercury, perhaps as late as 1539, when Rheticus arrived on the scene [45]. As Swerdlow has shown, Copernicus had first come up with a model different from the standard *De rev.* model. This can be established from the text of his holograph and its crossed-out parts, *i. e.*, without the corrections in the margin

43 From chart 2, we find that the ratio for Mercury is 4.45 for Ibn al-Šāṭir and 3.49 for *De rev*.

44 Swerdlow also notes a number of calculation errors ("The Derivation and first draft", p. 509).

45 N. Swerdlow, "Copernicus's four models of Mercury", in O. Gingerich and J. Dobrzycki (ed.), *Studia Copernicana XIII* (*Colloquia Copernicana, III): Astronomy of Copernicus and its background: Proceedings of the joint symposium of the IAU and IUHPS, co-sponsored by the IAHS, Torun, 1973* (Warsaw, 1975), p. 141-55 at 155 and n. 8. Based on the fact that the "standard" model is described in the *Narratio prima*, Swerdlow concludes that it was in place by 1539, but whether this occurred before or after Rheticus' arrival seems to us an open question.

on f. 176. This original model, which Swerdlow dubs the "deviant" version, is basically the same as the standard model (figure 11) but has the planet move on the circumference of a circle rather than its diameter. In other words, Copernicus uses something like the small circles employed by Ptolemy for his latitude theory in book XIII of the *Almagest* rather than a Ṭūsī-couple device [46].

It is not clear why he might have been experimenting with the small circles (perhaps he thought them simpler than the Ṭūsī-couple?) but in any event this "deviant" model [47] exhibits a mostly "correct" transformation of Ibn al-Šāṭir's model. Now it is of great historical interest that Copernicus reinstated eccentricities in *De revolutionibus*. Copernicus himself offers an explanation, at least a partial one, by citing changes in the eccentricities of Mars and Venus since the time of Ptolemy that have resulted from the motion of the mean Sun (*i. e.*, the center of the Earth's orbit) with respect to the orbit of the center of the epicycle carrying the planet [48]. Swerdlow and Neugebauer explain Copernicus' justification with careful analysis, but there seems to us to be another factor that may be at work. Could it be that Copernicus somehow realized that the *Commentariolus* model for the inner planets did not work? Once he tried to do the sort of derivation of the parameters from Ptolemy's observations for Venus and Mercury that he does in V.21-22 and V.27, he would have discovered that he could not obtain suitable elongations using his earlier model. For one thing, as we have mentioned, it is exceedingly difficult to compute the elongations for the *Commentariolus* models since the planets Venus and Mercury are not usually on their circle around the Sun (*i. e.*, r_4). It thus seems plausible that once Copernicus started the process of actually deriving parameters from observations, he would have realized that he needed a new model. Such a model, namely Ibn al-Šāṭir's, was already at hand and quite easily transformed into the *De rev.* model, which was much more amenable to computation.

6. THE REGIOMONTANUS DETOUR

In reference to our proposed transformation of Ibn al-Šāṭir's models directly into Copernicus', Swerdlow has insisted that Ibn al-Šāṭir's models are not sufficient to explain the models in the *Commentariolus* [49]. But because Ibn al-Šāṭir's

[46] Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, II, fig. 73, p. 658.
[47] The terminology is Swerdlow's; see his "Copernicus's four models of Mercury", p. 142.
[48] Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 299-300; see also I, 356 *sqq.* (for Mars) and I, 384 *sqq.* (for Venus).
[49] N. Swerdlow, "Copernicus's derivation of the heliocentric theory", p. 34 and passim.

Mercury model is virtually identical to the *De rev.* model, and Swerdlow has claimed that Copernicus had Ibn al-Šāṭir's models when writing the *Commentariolus*, and in particular the Mercury model [50], it would seem incumbent on him to explain why Copernicus was unable or unwilling to make the simple transformation ca. 1510 that he would make in 1543. To explore Swerdlow's reasoning a bit further, we have reconstructed, as best we can, the steps that he claims Copernicus took that would eventually lead to what he calls the standard *De rev.* model:

1) Copernicus first seeks to resolve the problem of irregular motion brought on by Ptolemy's equant ("first anomaly") with the solution offered by Ibn al-Šāṭir's models [51]; 2) he then is motivated to explore the "second anomaly" (*i. e.*, the one related to the planet's synodic motion) [52]; 3) this leads him to transform Ptolemy's epicyclic models into eccentric ones, based on propositions in Regiomontanus' *Epitome of the Almagest* [53]; 4) because, geocentrically, this leads to an unacceptable penetration of solid orbs, Copernicus is compelled to opt for a heliocentric system [54]; 5) Copernicus then incorporates Ibn al-Šāṭir's devices into the simple models (*i. e.*, ones that do not deal with the first anomaly) that he came up with in 3) and 4) [55]; 6) because of the problem in the transforma-

[50] "Since [the *Commentariolus's* Mercury model] is Ibn ash-Šāṭir's model, this is further evidence, and perhaps the best evidence, that Copernicus was in fact copying without full understanding from some other source, and this source would be an as yet unknown transmission to the west of Ibn ash-Shāṭir's planetary theory". Swerdlow, "The Derivation and first draft", p. 504.

[51] "[Copernicus'] original concern was the first, not the second, anomaly because it was in the representation of the first anomaly that Ptolemy's model violated the uniform and circular motion permitted to the rotation of a sphere … My own inclination is to suspect … [that] the identity with the earlier planetary theory [of Ibn al-Šāṭir] of Copernicus's models for the Moon *and* the first anomaly of the planets *and* the variation of the radius of Mercury's orbit *and* the generation of rectilinear motion by two circular motions seems too remarkable a series of coincidences to admit the possibility of independent discovery." Swerdlow, "The Derivation and first draft", p. 467, 469 (italics in original; clarifying words in brackets added by current authors).

[52] "It seems likely that in the course of the intensive study of planetary theory undertaken to solve the problem of the first anomaly, he carried out an analysis of the second anomaly leading to his remarkable discovery." Swerdlow, "The Derivation and first draft", p. 425. See also Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 56 and our comments below.

[53] Swerdlow, "The Derivation and first draft", p. 471-8.

[54] Swerdlow, "The Derivation and first draft", p. 477.

[55] This is nowhere stated as such. We are led to this conclusion since Swerdlow's entire discussion of the transformation from epicyclic to eccentric models involves orbs in which the first anomaly does not play a role. See Swerdlow, "The Derivation and first draft", fig. 17-22, p. 472-7. At some point, these "eccentric" models would need to be supplied with devices

tion from geocentric eccentric to heliocentric models, Venus and Mercury have serious deficiencies that make them similar to but significantly different from Ibn al-Šāṭir's models[56]; 7) by the time of writing *De revolutionibus*, Copernicus modified the *Commentariolus* model so that it " worked " computationally, ending up with a correct heliocentric version of Ibn al-Šāṭir's model[57].

In essence, Swerdlow is asking us to believe that Copernicus had the " correct " Mercury model all along, at least the one he eventually set forth in *De rev.*, but decided not to use it, instead taking this complicated, not to say convoluted, detour. According to Swerdlow in his original study of the *Commentariolus*, Copernicus did not fully understand Ibn al-Šāṭir's Mercury model[58]. But as Blåsjö has recently shown, and as we will discuss below, Swerdlow based his assessment on a misunderstanding of what Copernicus was saying regarding the behavior of the Mercury model.

Furthermore, Swerdlow's suggestion that somehow the problems with the first anomaly spurred Copernicus to explore the second anomaly is doubtful. Here is what he and Neugebauer say about this alleged problem:

> Copernicus probably undertook an investigation of the second anomaly, and of the eccentric model, because even with the Marāgha solution to the first anomaly, the uniform motion of the planet on the epicycle must still be measured from the mean apogee lying on a line directed to the equant (see fig. 5.53 for Venus). Thus, technically there is still a violation of uniform circular motion, or in physical terms, of the uniform rotation of the epicyclic sphere[59].

But this is really a non-problem as Naṣīr al-Dīn al-Ṭūsī pointed out:

> the [difficulty for the Moon] that was mentioned as arising on account of the anomaly in alignment is not present [for Mercury] because the alignment [of its epicycle diameter] is toward the point with respect to which the uniformity of motion occurs[60].

Even if somehow one thought this was a problem with Ptolemy's model, it is

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to account for the individual eccentricities, equants, etc. of Ptolemy's models.

[56] " The models in the *Commentariolus* were not intended for practical application – at least not with the crude and incomplete parameters supplied in the text – and at the time of its composition Copernicus was evidently not secure in constructing a model for Mercury. " Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 410.

[57] " He finally did reach a correct model – correct in the sense of doing what was expected of it – in *De revolutionibus* … it is properly equivalent to Ibn ash-Shāṭir's model… " Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 410.

[58] " … he copied it without fully understanding what it was really about. " Swerdlow, " The Derivation and first draft ", p. 504.

[59] Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 56.

[60] Ragep, *Ṭūsī's Memoir on astronomy*, I, 172. Ṭūsī generalizes this to the other 4 vacillating planets on I, 184.

certainly not with Ibn al-Šāṭir's model as one can see by examining figure 7 (Ibn al-Šāṭir's solid-orb version) and chart 2, where all the orbs are rotating uniformly. Would Copernicus not have understood this? This seems unlikely: the deft way Copernicus handles the transformation of Ibn al-Šāṭir's Mercury model in the *Commentariolus*, as well as his well-advised adoption of the actual model in *De rev.*, bespeaks of someone quite at home with the astronomical traditions to which he was heir. This then makes Swerdlow's claim for Copernicus' motivation for investigating the second anomaly, and the move toward eccentric models, dubious at best.

Let us now turn to some specific points Swerdlow has brought up in favor of his " Regiomontanus detour " [61]. At the base of his entire reconstruction, the only concrete evidence he has, is the claim that *eccentricitas* in the Uppsala notes refers to the radius of the eccentric in the transformation of the epicycles in Ptolemy's planetary models. After listing the values for the *eccentricitas* of Mars, Jupiter, Saturn, and Mercury, Swerdlow has this to say:

> These numbers directly give the proportion of the radius of the epicycle to the radius of the eccentric where the radius of the eccentric is 10000. Copernicus, however, calls the number for each planet an *eccentricitas*. The substitution of an eccentricity for the epicyclic radius can refer only to the eccentric model for the second anomaly mentioned briefly by Ptolemy in *Almagest* XII, 1 (Manitius 2, 268-269); it is this alternate model that leads directly to the heliocentric theory [62].

The consistent use of 10000 in the alternative models is what one would expect if Swerdlow's reconstruction were correct. But in fact, this is only true for Mars, Jupiter, and Saturn. For Mercury, the listed *eccentricitas* value in the Uppsala notes is 2250 (later changed to 2256) indicating a radius of 6000. It is true that in the margin one finds 376, but this is not labeled as " the " *eccentricitas* and in any case is based on a radius of 1000, not 10000. If Copernicus is developing alternative eccentric models, why would he use different radii for his norms? Indeed later, when discussing Mercury, Swerdlow recognizes this and then gives an alternative explanation, saying that " Copernicus was using sine tables normed to a radius of 6000 or 60000 … It is possible that Copernicus used sines normed to 60000 for all the planets, and then divided by 6 to produce the numbers in U " [63]. All this is odd and, to us, unconvincing. Why would Copernicus change norms if he is consistently transforming Ptolemy's epicycle models

[61] For a recent summary of Swerdlow's position, see his " Copernicus's derivation of the heliocentric theory ". For an alternative to Swerdlow's reconstruction and a re-evaluation of the critical Uppsala notes, see Ragep, " Ibn al-Shāṭir and Copernicus ", which contains a fuller exposition of the following.

[62] Swerdlow, " The Derivation and first draft ", p. 471.

[63] Swerdlow, " The Derivation and first draft ", p. 505.

to eccentric ones? It is much more plausible to see the numbers listed with the label *eccentricitas* simply as part of a series of steps in the heliocentric transformation of Ibn al-Šāṭir's models. This is most clearly illustrated with Mercury's parameters in the upper part of the Uppsala notes. It would appear that Copernicus, for the *eccentricitas*, originally wrote 2250, which is Ptolemy's epicycle radius normed to 6000 (or 60000 / 10 if we were to accept that the "original" number was 60000). But at some point, Copernicus changed the 0 of 2250 to a 6, which is consistent with the 376 (2256 / 6) in the margin. The explanation for this is provided by Swerdlow in his derivation of what Copernicus called the *diversitas diametrj*, which is the displacement resulting from the Ṭūsī-couple. As Swerdlow shows, this displacement, given as 1151 in the Uppsala notes, comes from a mean epicycle radius of 22560[64]. It would seem that Copernicus originally took Ptolemy's radius of 2250 and then changed it so it would be consistent with the *diversitas diametrj* of 1151. This slight modification of the *eccentricitas*, though mathematically insignificant, does, we think, provide a window for understanding Copernicus' use of *eccentricitas* as well as his procedures in the Uppsala notes. Our suggestion is that *eccentricitas* simply meant the eccentricity, or *off-centeredness* from the mean Sun, of either the Earth (for the outer planets) or the main deferent (r_4) of the planet itself (for the inner planets) *after* the transformation of Ibn al-Šāṭir's models into their heliocentric versions in the *Commentariolus*. For both the outer and inner planets, the values for the *eccentricitas* in the upper part of the Uppsala notes are equivalent to the radii of Ptolemy's epicycles (except for the slightly revised value for Mercury). We can see what this looks like for the outer planets in figure 15.

Taking Mars as our example, we find in the Uppsala notes that the Ptolemaic epicycle of 39.5 (or 3950 with a deferent radius OF of 6000) has been changed to 6583, normed to 10000. This now, in our reconstruction, represents the radius of the Earth's "orbit" around the Sun in figure 15. In the instructional note separating the upper and lower parts of the Uppsala notes, Copernicus writes: "proportio orbium celestium ad eccentricitatem 25 partium" (the proportion of the celestial orb to an eccentricity of 25 parts). In other words, Copernicus wishes to provide a unified "solar system" based on an *eccentricitas* of 25, which is the Earth-Sun distance in the unified system, that then allows for a simple calculation of the "semidyameter orbis", or radius $\overline{\odot}'$F of the celestial orb for each planet. For the outer planets, this is straightforward: in the case of Mars,

[64] Swerdlow, "The Derivation and first draft", p. 508.

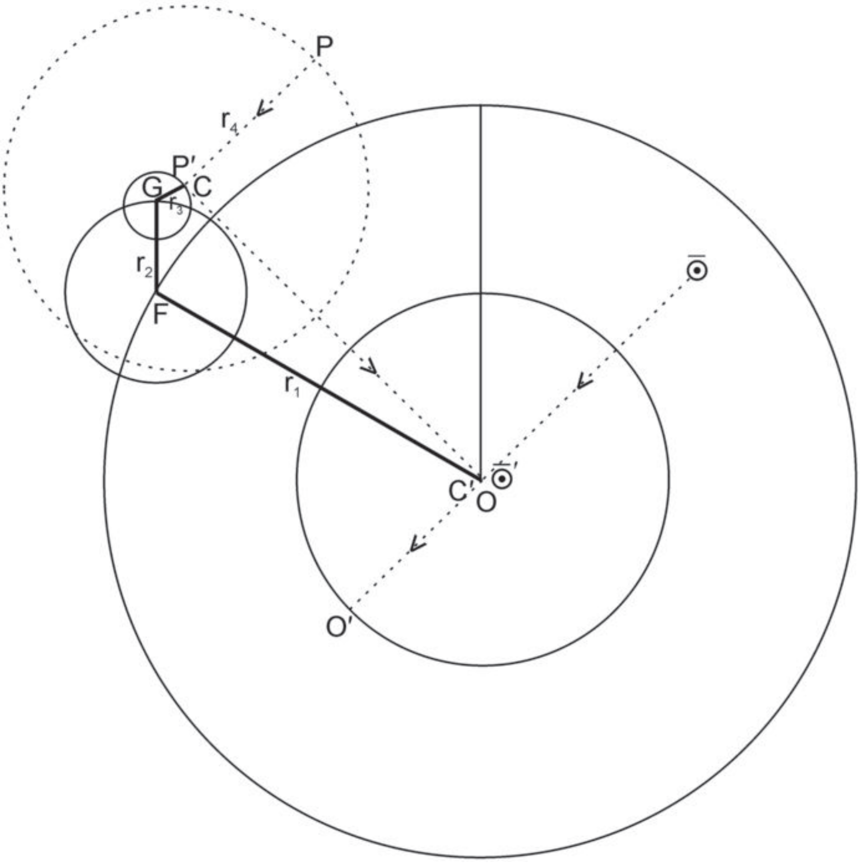

FIG. 15. Transformation of Ibn al-Šāṭir's models for the outer planets into the *Commentariolus* models (primed letters / symbols indicate location after the transformation).

we have $r_4 : 25 = 10000 : r_1 \Rightarrow 6583 : 25 = 10000 : \overline{\odot}'\mathrm{F} \Rightarrow \overline{\odot}'\mathrm{F} \approx 38$ (as in the lower part of the Uppsala notes)[65].

For Mercury and Venus, however, the situation is less straightforward, and the designation of *eccentricitas* in the case of Mercury *could* be an indication of Regiomontanus' eccentric model, inasmuch as it definitely does not indicate the Earth-Sun distance in the *Commentariolus* version. For the *eccentricitas* of 2256 in the upper part of the Uppsala notes is r_4 in our figure 13, while the *eccentricitas* of 25 in the instructional note is represented by r_1. So rather than a ratio of 2256:25 or 376:25, analogous to what we used for Mars, we need the following proportion to reach Mercury's *semidyameter orbis*: $r_1 : 25 = r_4 : \overline{\odot}'\mathrm{F}' \Rightarrow 1000 : 25 = 376 : \overline{\odot}'\mathrm{F}' \Rightarrow \overline{\odot}'\mathrm{F}' = 9;24$. Now this may seem to count against our interpretation, since one could argue, as does Swerdlow, that despite the *eccentricitates* indicating different radii in our diagrams (r_1 for the upper planets, r_4 for the lower), in all cases *eccentricitas* would be an appropriate moniker for each of the eccentricities of Regiomontanus' eccentric models, whether for the upper or lower planets. But to emphasize our earlier point, since Copernicus is not consistent in his norms in the upper part of the Uppsala notes nor in the way he is using *eccentricitas* (as some version of a transformed epicycle in the upper part, as the Earth-Sun distance in the instructional note), we think "off-centeredness from the mean Sun" fits the term and is compatible with his usage throughout the notes.

Moreover there are other reasons for considering both the Uppsala notes and the *Commentariolus* as strongly suggesting that Ibn al-Šāṭir's models are the sole basis for Copernicus' longitudinal models in the *Commentariolus*[66]. Here we concentrate on Mercury. Swerdlow states in his study of the *Commentariolus* that "The statement [by Copernicus] that Mercury 'appears' to move in a smaller orbit when the earth is in the apsidal line and in a larger orbit when the earth is 90° from the apsidal line is utter nonsense as a description of the apparent motion of Mercury"[67]. He goes on to make the following, striking assertions:

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65 A. Goddu has asserted that "Ragep claims that the *eccentricitas* for each planet is the Earth – mean Sun distance and, hence, 10000 (or 1000 in the case of Mercury) is the *eccentricitas* for each planet." ("Birkenmajer's Copernicus: Historical context, original insights, and contributions to current debates", *Science in context*, 31 (2018): 189-222 at 210.) But clearly Goddu did not understand Ragep's argument in "Ibn al-Shāṭir and Copernicus", repeated here, where 6583 is explicitly given as the *eccentricitas* for Mars. His other comments regarding the ultimate origin of Copernicus' numbers for the *eccentricitates* in U (the *Alfonsine tables*) and the use of the genitive (*eccentricitas martis* 6583) are not particularly relevant to the discussion. The latter point ignores the fact that a Latin genitive (as in other languages) can be used in different ways; thus, it could just as well mean "the eccentricity *of* Mars is 6583" as "the eccentricity *for* Mars is 6583", *i. e.*, in the case of Mars' planetary model.

66 Some of the following repeats points made in Ragep, "Ibn al-Shāṭir and Copernicus".

67 Swerdlow, "The Derivation and first draft", p. 504.

> This misunderstanding must mean that Copernicus did not know the relation of the model to Mercury's apparent motion. Thus it could hardly be his own invention for, if it were, he would certainly have described its fundamental purpose rather than write the absurd statement that Mercury " appears " to move in a larger orbit when the earth is 90° from the apsidal line. The only alternative, therefore, is that he copied it without fully understanding what it was really about. Since it is Ibn ash-Shāṭir's model, this is further evidence, and perhaps the best evidence, that Copernicus was in fact copying without full understanding from some other source, and this source would be an as yet unknown transmission to the west of Ibn ash-Shāṭir's planetary theory [68].

While we concur that this is Ibn al-Šāṭir's Mercury model, which, as stated above, leads to unacknowledged problems with Swerdlow's analysis, we do not agree that Copernicus did not understand the model. Part of Swerdlow's argument is that " Copernicus apparently does not realize that the model was designed, not to give Mercury a larger orbit (read *epicycle*) when the earth (read *center of the epicycle*) is 90° from the apsidal line, but to produce the greatest elongations when the earth (*center of the epicycle*) is $\pm 120°$ from the aphelion (apogee) [69] ". But as Blåsjö has pointed out, there is a plausible way to read what Copernicus is saying that shows he was aware that the simple double-epicycle model (see our figure 12) would not work for Mercury without an adjustment, *i. e.*, the introduction of the Ṭūsī-couple device. Nevertheless, it is curious that Copernicus only refers to the situation with reference to the apsis and quadratures and not at $\pm 120°$ as in the *Almagest* and also in *De revolutionibus*. Blåsjö thinks that it was not necessary for Copernicus to mention the maximum elongations at the trines " since his intended readership would of course be very familiar with Ptolemaic theory and realize at once that this corollary carries over directly insofar as the two theories are equivalent [70] ". But as we will argue in appendix 1, it is highly unlikely that Copernicus' " intended readership ", or anyone else for that matter, would have seen the greatest elongations at the trines as somehow a " corollary " to the effect of the Ṭūsī-couple. Blåsjö also wishes us to believe that by showing that Swerdlow misunderstood what Copernicus was saying, this somehow disproves Swerdlow's conclusion that Copernicus was copying Ibn al-Šāṭir's model. Although this is an unwarranted leap on Blåsjö's part, his analysis does provide a key to showing an even stronger connection between Ibn al-Šāṭir and Copernicus.

Indeed, given the overwhelming evidence of the similarities, and in several cases the virtual identity, of Copernicus' and Ibn al-Šāṭir's models, we are led to

68 Swerdlow, " The Derivation and first draft ", p. 504.
69 *Ibid.*
70 Blåsjö, " A critique of the arguments for Maragha influence on Copernicus ", p. 193.

conclude that Copernicus knew of his predecessor's models in some form. But in which form? Because Copernicus does not use Ibn al-Šāṭir's parameters, and in fact makes some ill-advised choices, we think it much more likely that he had diagrams but not Ibn al-Šāṭir's text. The case of the variable size of the circumference of Mercury's orbit is revealing. Looking at the " schematic " diagram in Ibn al-Šāṭir's *Nihāyat al-su'l* (figure T1 in the translation, appendix 2), one is struck by how perfectly it depicts what Copernicus describes. In his diagram, Ibn al-Šāṭir has shown both the " apparent epicycle orb " on which is the planet and the " true epicycle orb ", which is the " reference " epicycle orb without the effect of the Ṭūsī-couple. (See also figure 10 above.) Even though Ibn al-Šāṭir, as we have seen, was aware of the importance of the nearest distances occurring at the trines [71], he did not feel the need to indicate this on his diagram; his purpose was to show the effect of the Ṭūsī-couple on the model, which causes the epicycle to " shrink " at 0° and 180°, and " expand " at 90° and 270°. Bearing this in mind, and with a view to Ibn al-Šāṭir's diagram, let us quote Copernicus:

> But this combination of circles, although adequate to the other planets, is not adequate to Mercury because, when the Earth is in the views of the apsis mentioned above *[i.e, at 0° and 180°]*, the planet appears to move by traversing a far smaller circumference, and on the other hand, when the Earth is at quadratures [to the apsis], *[i.e., at 90° and 270°]*, by traversing a far larger circumference than the proportion of the circles just given permits. Since, however, no other anomaly in longitude is seen to arise from this, it seems suitable that it take place on account of some kind of approach [toward] and withdrawal from the center of the sphere on a straight line [72].

It would seem that Copernicus was following Ibn al-Šāṭir to a " + ".

Ibn al-Šāṭir's diagram also helps explain another, heretofore puzzling aspect of the Uppsala notes [73]. In the upper part of the Uppsala notes for Mercury, Copernicus writes 6 or 600 for $r_1 + r_2$. However, the " ecce " of 2256 (or 376) in conjunction with the 115.1 (or 19) for the *diversitas diametrj*, the displacement resulting from the Ṭūsī couple, implies $r_1 + r_2 = 576$ [74]. But Copernicus uses 540 to derive the values in the lower part of U, *i. e.*, $r_1 = 1;41\,{}^1/_4$ and $r_2 = 0;33\,{}^3/_4$. Regarding this, Swerdlow says: " I do not know why Copernicus had these problems [75] ". However, looking again at fig. T1, we can conjecture

[71] See the above discussion of Ibn al-Šāṭir's values for the maximum elongations, which are remarkably close to Ptolemy's near 120°.

[72] Swerdlow, " The Derivation and first draft ", p. 503 (Swerdlow's translation; italics are from the current authors).

[73] The following is taken from Ragep, " Ibn al-Shāṭir and Copernicus ".

[74] Swerdlow, " The Derivation and first draft ", p. 507, where he derives 576(0). As he notes (p. 508-9), Copernicus seems to have had considerable problems in converting from the upper value in U for $r_1 + r_2$ to the values for the two epicycles in the lower part.

[75] Swerdlow, " The Derivation and first draft ", p. 509.

that Copernicus reasoned (incorrectly) as follows: the largest size of the epicycle ("apparent epicycle orb") is $2256 + 115.1 = 2371.1$ at 90°. Its smallest size ("apparent epicycle orb") is $2256 - 115.1 = 2140.9$ at 0°. But rather than taking the radius of the "true epicycle orb", *i. e.*, 2256 (or 376), he adopted the "apparent epicycle orb" at $\overline{\alpha} = 0°$ as his reference epicycle, since it is the starting point. If we take the maximum equation to occur at 90°, then the Ptolemaic eccentricity of 6 (or 600) should be measured there with the epicycle being 2371.1. But at $\overline{\alpha} = 0°$, the ratio of the two "apparent" epicycles is $2140.9/2371.1 \approx 0.9$. So the sum of the eccentricities ($r_1 + r_2$) should be proportionally lowered, at least according to this reasoning, *i. e.*, $0.9 \times 600 = 540$[76]. Along with Copernicus' description of a varying planetary "circumference" (epicycle in Ibn al-Šāṭir's model) and the explanation for 540 arising from the diagram, we would argue that Copernicus had at his disposal something like fig. A1 / T1. This is the sense in which we can say that Copernicus had Ibn al-Šāṭir's Mercury model when composing the *Commentariolus* and later *De rev*.

7. CONCLUSION

The remarkable similarity between Ibn al-Šāṭir's Mercury model and that in *De rev.* should long ago have settled the question of whether Copernicus was dependent on his Islamic predecessor. Although Swerdlow has championed a connection between Islamic astronomy and Copernicus, his interjection of a Regiomontanus detour has, we believe, considerably muddied the waters and inhibited the simple conclusion that Copernicus built his system almost exclusively on the foundation of Ibn al-Šāṭir's models. Blåsjö's arguments for Copernicus' independence from Islamic influence, based on the elusive concept of "naturalness", would have very different models be classified as equivalent (see appendix 1). As argued elsewhere, Ibn al-Šāṭir's models are fundamentally different not only from those of Ptolemy but also from his "Marāgha" predecessors[77]. Because of the "heliocentric bias" brought about by a rejection of eccentrics and by making the Earth the actual center of motion, Ibn al-Šāṭir's models considerably facilitated Copernicus' transition from an Earth-centered to a Sun-centered cosmology. There was a wide array of non-Ptolemaic Mercury models that were developed after Naṣīr al-Dīn al-Ṭūsī admitted that this complex model had defeated him[78]: Quṭb al-Dīn al-Šīrāzī claims to have invented nine different Mercury

[76] This also works, of course, if one uses 376 and 19 instead of 2256 and 115.1.
[77] Ragep, "Ibn al-Shāṭir and Copernicus", p. 396-7, 408.
[78] Ragep, *Ṭūsī's Memoir on astronomy*, I, 208.

models [79], and Khafrī presents four in his supercommentary on Ṭūsī's *Taḏkira* [80]. We should also not forget Biṭrūjī's neo-Aristotelian model as well as other homocentric models inspired by him [81], and, of course, Copernicus might have well begun thinking about Mercury when he first encountered Peurbach, as Michela Malpangotto has suggested [82]. There was and is nothing " natural " about any of these models. If anything, they show a remarkable range of human ingenuity. Copernicus did not come up with Ibn al-Šāṭir's models because they were " natural ". But that he chose them was part of his remarkable genius.

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APPENDIX 1
THE ISSUE OF EQUIVALENCE AND " NATURAL " SOLUTIONS

V. Blåsjö has claimed that " the technical similarities [between Copernicus' models and those of his Islamic predecessors] … are all natural consequences of natural principles, making independent discovery perfectly plausible [83] ". As mentioned previously, the notion of " natural " solutions is problematic; there is no " natural " solution to the equant problem (or to any of the other difficulties related to Ptolemaic astronomy) as evidenced by the myriad solutions that were put forth. Indeed, Ibn al-Šāṭir's solution is highly individualistic and is quite different from those of both his predecessors (such as Quṭb al-Dīn al-Šīrāzī) and successors (such as 'Alī Qushjī) [84]. His Mercury model in particular is quite distinct, as we have endeavored to show, and its virtual identity with the *De rev.* model is not something that can be dismissed as a " natural " outcome. And

[79] Quṭb al-Dīn al-Šīrāzī, *Fa'alta fa-lā talum*, Majlis-i šūrā ms. 3944, f. 7b. For an analysis of some of these models, see Amir-Mohammad Gamini, " Quṭb al-Dīn al-Shīrāzī and the development of non-Ptolemaic planetary modeling in the 13th century ", *Arabic sciences and philosophy*, 27/2 (2017): 165-203.

[80] G. Saliba, " A sixteenth-century Arabic critique of Ptolemaic astronomy: The work of Shams al-Dīn al-Khafrī ", *Journal for the history of astronomy*, 25/1 (1994): 15-38.

[81] B. R. Goldstein (ed. and transl.), *Al-Biṭrūjī: On the principles of astronomy*, 2 vol. (New Haven, CT, 1971), I, 140-2, II, 375-85.

[82] Malpangotto, " L'univers auquel s'est confronté Copernic ".

[83] Blåsjö, " A critique of the arguments for Maragha influence on Copernicus ", p. 183.

[84] On this point, see Saliba, " A sixteenth-century Arabic critique of Ptolemaic astronomy ".

Copernicus himself over his lifetime came up with different models for Mercury (four according to Swerdlow); which of these is supposed to count as " natural "?

Part of the problem with Blåsjö's approach is that he is far too willing to dismiss differences between models as irrelevant, especially physical differences, as long as there is what he takes to be mathematical equivalence. But Blåsjö's reductionism leads to a number of untoward conclusions, not least because his notion of mathematical equivalence is itself problematic. To explore this a bit further, let us turn to his claims regarding the nearest distance issue for Mercury. As we have seen, Swerdlow takes Copernicus' silence on the matter in the *Commentariolus* to mean that he did not fully understand his own model. In response, Blåsjö uses his notion of " equivalence " to assert that " There is no need for Copernicus to mention this since his intended readership would of course be very familiar with Ptolemaic theory and realize at once that this corollary carries over directly insofar as the two theories [that of Ptolemy and Copernicus] are equivalent [85] ". Setting aside the dubious notion of an " intended readership " in 1510 that would be experts on one of the most difficult problems of Ptolemaic astronomy, it is clear from our above discussion of maximum elongation and the equation of center that it is simply wrong to claim that the *Commentariolus* model is equivalent to those of Ptolemy, Ibn al-Šāṭir, and *De rev.*, if one means by " equivalent " that they can produce equivalent results. One might be able to somehow adjust the parameters in the *Commentariolus* to reach results that would be closer to those of the other models, but Copernicus clearly did not do this. Nor is it at all likely that he tested the *Commentariolus* model to see if it was equivalent. The fact that the value for the equation of center is so far off is a clear indication of this (chart 5 above).

In short, the fact that the Mercury model in the *Commentariolus* was not only impractical but also exceedingly difficult to test undermines Blåsjö's claim that finding the maximum elongations at 0, ± 90, and 180° " eliminates the need for Copernicus to address the issue " of maximal elongation at $\pm 120°$, since somehow this latter is a corollary of the former. Furthermore, this requires us to believe that Copernicus understood this property of Ptolemy's model, something that is certainly not self-evident inasmuch as there is some doubt that Copernicus even had a copy of the *Almagest* when he wrote the *Commentariolus* [86].

Let us turn to the question of whether Blåsjö might nevertheless be correct in asserting that the maximal elongations at $\pm 120°$ are somehow " *a corollary* " that are only derived after the model has been determined by observations for the 0°, $\pm 90°$, 180° cases that Ptolemy brings forth. Mathematically speaking,

[85] Blåsjö, " A critique of the arguments for Maragha influence on Copernicus ", p. 193.
[86] Swerdlow, " The Derivation and first draft ", p. 426.

there is some truth to this: since the shape of the curve described by C in figure 6 above is an oval, rather close to an ellipse [87], it would naturally follow that once one has the major and minor axes the other positions fall into place. But this bit of anachronistic reasoning has little bearing on the way in which Ptolemy most likely proceeded; for even after fixing his parameters using observations at 0°, ±90°, 180°, he still had to confirm that the model actually predicted the observations for ±120°. That it does is hardly a " corollary "; indeed, Swerdlow has convincingly argued that it was neither mathematical necessity nor observational precision that results in the model being in accord with the observations at ±120°. Rather, the model itself most likely was constructed to account for observations that seemed to show (erroneously as it turned out) that elongations at ±120° were greater than those at 180°. Swerdlow is then led to conclude " that some, perhaps most [of the observations], were [then altered] " to take into account the theoretical model with its two perigees [88]. It is unlikely that anyone before Swerdlow (other than Ptolemy himself) understood this, at least not in the analytical detail that Swerdlow brings to the task. So the original motivation for Ptolemy's model, and alleged curve-fitting, does not in itself count against Blåsjö's speculation about why Copernicus does not feel the need to explain that his model in the *Commentariolus* accounts for Ptolemy's reported elongations at the trines. It is at least conceivable that he had analyzed the model in the *Almagest* and understood that fixing the parameters for 0°, ±90°, 180° would achieve his desired result. But this is doubtful for several reasons. For one, almost everyone before Copernicus who had any understanding of the model did remark on the two perigees and understood that this was fundamental to the model [89]. That Copernicus does not do so is thus odd. Furthermore, for us to

[87] W. Hartner, " The Mercury horoscope of Marcantonio Michel of Venice: A study in the history of Renaissance astrology and astronomy ", *Vistas in astronomy,* 1 (1955): 84-138 at 109-22, reprinted in W. Hartner, *Oriens-Occidens*, I, 440-95 at 465-78.

[88] Swerdlow, " Ptolemy's theory of the inferior planets ", p. 51-4 (quotation is on p. 54). This brief summary can hardly do justice to Swerdlow's incisive and compelling explanation of Ptolemy's Mercury model and its origins. Although hardly conclusive, it is noteworthy that Ptolemy presents the observations establishing the need for two perigees (IX.8) *before* deriving the distances between the centers and the radius of the small circle (IX.9). Once he has the parameters, he then " proves " that the model will produce the needed two perigees, a result that Swerdlow remarks may seem like " luck " but is much more likely a consequence of " adjusting " the observations and model in advance [G. J. Toomer (transl.), *Ptolemy's Almagest* (London, 1984), p. 453-60].

[89] This is quite explicit, for example, in Ṭūsī's *Taḏkira* (Ragep, *Ṭūsī's Memoir on astronomy*, I, 168-9 and 176-7 [fig. T9]), a work well known to Ibn al-Šāṭir. Because Ibn al-Šāṭir is so familiar with his predecessors (including Ṭūsī), he evidently does not feel the need to discuss the two perigees in his chapter on Mercury (see appendices 2-3); however, he does indicate that he is aware of Ptolemy's Mercury model having the perigees at points other than 180°

accept that Copernicus could consider the perigees at $\pm 120°$ a corollary, one would need to show that he had sufficient understanding of Ptolemy's model so that his own could replicate its parameters and output. But as we have seen, this is far from the case, at least at the time of the composition of the *Commentariolus*. Thus to believe Blåsjö's main contention, one needs to assume that Copernicus when writing the *Commentariolus*: a) would not mention the most prominent aspect of Mercury's model because this was a "corollary" to Ptolemy's "equivalent" model; and also assume, b) that Copernicus would put forth a model that did not produce equivalent results. Needless to say, we find this untenable. On the other hand, by the time he composed *De revolutionibus*, Copernicus not only does not ignore the perigees at $\pm 120°$, he in fact adjusts the parameters of the model to account for them (something obviously not done in the *Commentariolus*) and achieves a result fairly close to Ptolemy's[90]. But this was done many years later and has no bearing on Blåsjö's contention, which is focused on the earlier *Commentariolus*.

APPENDIX 2 (TRANSLATION)
IBN AL-ŠĀṬIR'S *NIHĀYAT AL-SU'L*, BOOK I, CHAPTER 21

On the configuration of the orbs of Mercury according to our procedure in conformity with observation

We conceive of an orb in the plane of the zodiacal orb and on its two poles and its center; it is called *the parecliptic*. We conceive of a second orb whose plane is inclined from the plane of the parecliptic one-half plus one-quarter degree at the apogee in the southern direction. This inclination is not fixed; according to [another] opinion, which is more correct, it is inclined 1/6 degree and is of fixed inclination[91]. The plane of the inclined [orb] intersects the plane of the parecliptic at two facing points, one of which is called the head and the other the tail. We conceive of a third orb whose center is on the equator of the inclined [orb], its radius being 4 parts, 5 minutes using parts by which the radius of the inclined is 60 parts; it is called *the deferent*. We conceive of a fourth orb whose center is on the deferent equator, its radius being 1/2 plus 1/3 of a degree [*sic*][92]; it is called

in his introductory remarks in *Nihāyat al-su'l*, which deal with difficulties of the Ptolemaic models (Oxford, Bodleian, Marsh ms. 139, f. 3b and Penchèvre, "La Nihāya al-sūl", p. 40-1).

90 See above and Swerdlow and Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus*, I, 422-4.

91 Both opinions, as it turns out, are due to Ptolemy: the variable inclination of the inclined orb is presented in the *Almagest*; a fixed inclination of 1/6 degree is in the *Planetary hypotheses*. See Neugebauer, *A history of ancient mathematical astronomy*, II, 909.

92 Later the value that is used is 55 minutes.

the dirigent. We conceive of a fifth orb whose center is on the dirigent equator, its radius being 22 parts, 46 minutes of those parts; it is called *the epicycle* orb. We conceive of a sixth orb whose center is on the epicycle equator, its radius being 33 minutes; it is called *the enclosing* [orb][93]. We conceive of a seventh orb whose center is on [the equator of] the enclosing [orb], its radius being equal to the radius of the enclosing [orb], namely 33 minutes; it is called *the maintaining* [orb] and Mercury is embedded on the equator of this orb.

As for *the motions*: the parecliptic moves on the two ecliptic poles sequentially, one degree every sixty years, this being the same as the motion of the apogees[94]. The inclined moves sequentially equal to Mercury's motion of center, which is equal to the Sun's [motion] of center. It is in a nychthemeron $0;59,8,10$. As for the deferent, it moves counter-sequentially in its uppermost part, this also being equal to Mercury's motion of center[95]. As for the epicycle orb, it moves sequentially in its uppermost part in the amount of the excess of Mercury's proper motion over its motion of center, it being in a nychthemeron $2;18,14,2$[96]; it is a simple motion.

As for Mercury's proper motion, it is a simple motion that is compound because it is in the amount of the motion of this epicycle, which is $2;18,14,2$ plus the motion of Mercury's center, which is $0;59,8,10$. This is [simple?] because the two motions are in the same direction, so the separation of the planet from the apex is in the amount of the sum of the two motions, namely $3;6,24,10,1,38,37,28,42$, which is the compounded proper motion of Mercury, and it is uniform with respect to the epicycle center.

What will clarify this further is that when the inclined moves a quarter revolution, and the deferent moves a quarter revolution, and the dirigent moves a half revolution, the apex, which is the starting point of its proper motion, will shift a quarter revolution sequentially. However, by observation it is found to shift sequentially equal to the proper motion of Mercury, namely $3;6,24,10$. Thus the motion of the epicycle about its center sequentially is in the amount of the excess of this proper [motion] over the motion of center, since they are both in the same direction. This has thus been clarified[97].

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[93] MS L adds "and the containing [*shāmil*]". This term is used later in this chapter for the enclosing orb.

[94] Note that Ibn al-Šāṭir differentiates the motion of the apogees from the precessional motion. See note 31.

[95] The following is implied from what follows but is missing in all the manuscripts: <The dirigent moves sequentially in its uppermost part, this being equal to twice Mercury's motion of center.>

[96] $3;6,24,10,1,38,37,28,42° - 0;59,8,10° = 2;7,16,0°$, not $2;18,14,2°$. We do not know the source of this error, but it is attested in all the manuscripts. Note that it is repeated in the following paragraph.

[97] For further clarification, see above, figure 9 and the accompanying explanation.

As for the enclosing [orb], it moves sequentially in its uppermost part equal to twice Mercury's motion of center, which is daily $1;58,16,20$. As for the maintaining [orb], it moves counter-sequentially in its uppermost part 4 times Mercury's motion of center, which is daily $3;56,32,39$.

So Mercury remains on the line extending from the epicycle center to the center of the enclosing [orb], approaching and moving away from the epicycle center, it being on the line and not departing from it. When the epicycle center is at the apogee or perigee, Mercury will be at its nearest distance to its epicycle center; this nearest [distance] is called the epicycle's apparent radius, and it is 21 1/3 parts [98]. And when the center is three signs [away], Mercury will be at its farthest distance from the center of the epicycle, namely $23°52'$ [99]. Thus the farthest distance of Mercury from the center of the world is 86 2/3 [100] and its nearest [distance] 33 1/3 [101]; however, Mercury does not come near the nearest distance of its solid orbs, according to what we have explained before in another venue [102].

As for the sizes of the solid orbs: the radius of the deferent sphere is $28;52$ [103]; the radius of the dirigent sphere is $24;47$ [104]; the radius of the epicycle sphere is $23;52$ [105]; the radius of the enclosing sphere is $1;6$; and the radius of the maintaining sphere is $0;33$. All are with parts whereby the radius of the parecliptic is 60 parts. So the farthest distance of the parecliptic is 88 [parts] and 52 minutes [106]. Above that is the thickness of the parecliptic; let us assume it to be fully complete at $89;30$. And the nearest [distance] of its orbs is $31;8$ [107] but it is less than that due to the conjunction of the orb, so we assume it to be $31;0$. [108] And God is all-knowing.

[98] This should be 21 2/3: $22;46 - 1;6 = 21;40^p$.

[99] $22;46 + 1;6 = 23;52^p$.

[100] $60 + 4;5 + 0;55 + 21;40 = 86;40^p$.

[101] $60 - (4;5 + 0;55 + 21;40) = 33;20^p$.

[102] With reference to figure 6, one can see that the planet never reaches the "nearest distance" of the solid orbs, which is the point of tangency between the deferent and the concave surface of the inclined orb. "Another venue / place" probably refers to another work.

[103] $0;33 + 0;33 + 22;46 + 0;55 + 4;05 = 28;52$.

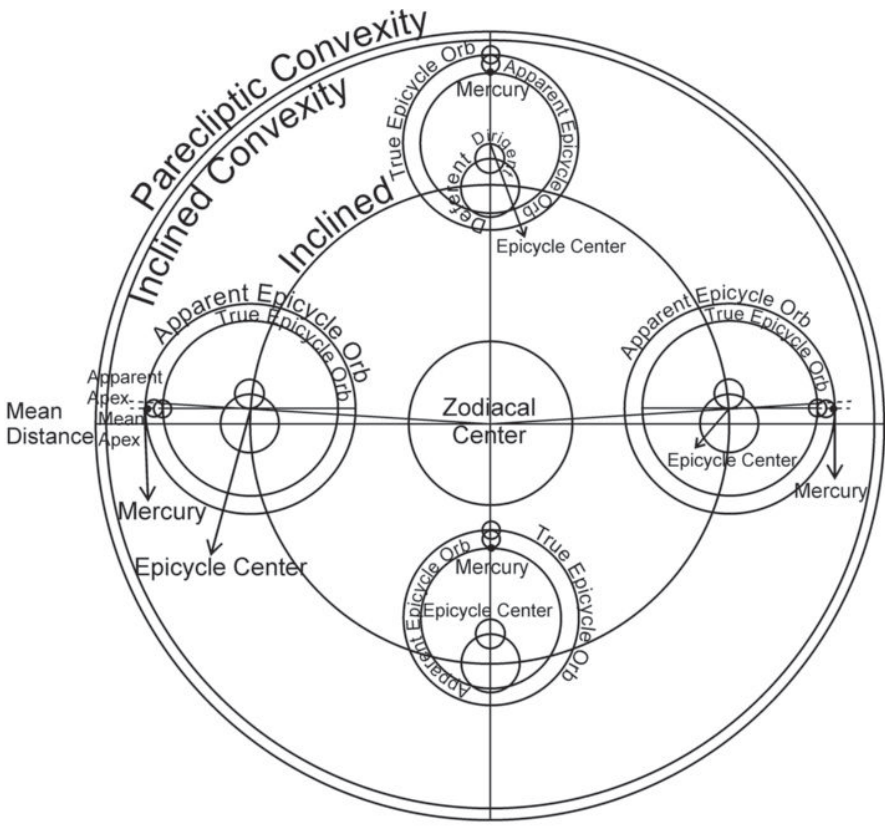
[104] $0;33 + 0;33 + 22;46 + 0;55 = 24;47$.

[105] $0;33 + 0;33 + 22;46 = 23;52$.

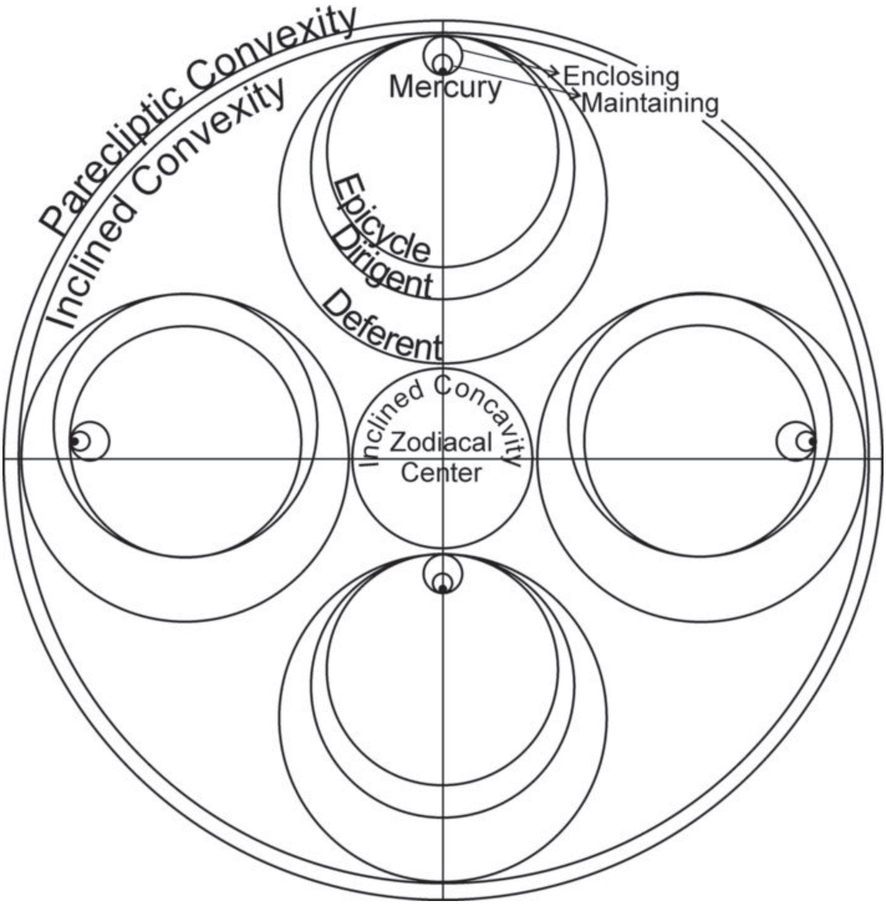
[106] $60 + 28;52 = 88;52$.

[107] $60 - 28;52 = 31;8$.

[108] This notion of conjunction [*ittiṣāl*] seems to be peculiar to Ibn al-Šāṭir. The idea is that the orb on which is the nearest distance (in this case the inclined) needs extra thickness. Thus the "nearest distance" will be less than what has been calculated thus far. He also applies this for the other planets, explaining it first for Saturn at the end of chapter 12, where he calls it *iḫlāṭ* rather than *ittiṣāl*. There is also a scholium [*tanbīh*] at the end of chapter 19 on Venus that explains how to transform the schematic circles into solid orbs that gives instructions for adding the *ittiṣāl*.



[Fig. T1.] This is the illustration of the orbs of Mercury according to which the centers of the complete spheres are as pictured in a plane for the apogee, the perigee and the mean distances.



[Fig. T2.] This is the illustration of Mercury's solid orbs, which are complete spheres, as pictured in a plane for the apogee, the perigee and the mean distances.

APPENDIX 3 (ARABIC TEXT)

Manuscripts used and sigla [109]

B (ب): Oxford, Bodleian, Marsh ms. 290 [f. 29a, line 7 – f. 30a, line 3]
H (ح): Oxford, Bodleian, Huntington ms. 547 [f. 40b, line 8 – f. 41b, line 4]
D (د): Oxford, Bodleian, Marsh ms. 501 [f. 30b, line 4 – f. 31b, line 1]
S (س): Tehran, Sipahsalar ms. 598 [page 38, line 7 – page 40, line 4]; copy date: 935/1528
F (ف): Jerusalem, Khālidiyya ms. 992 [f. 26b, line 5 – f. 27b, line 12]
Q (ق): Istanbul, Süleymaniye, Kadızade Mehmed Ef. ms. 339 [f. 30b, line 16 – f. 33a, line 7]; copy date: 751/1350
G (گ): Mashhad, Guharshad ms. 1409 [f. 49b, line 7 – f. 51b, line 5]; copy date: 1275/1858
L (ل): Leiden, Leiden University ms. Or 194 [f. 46b, line 12 – f. 48b, line 1]
M (م): Oxford, Bodleian, Marsh ms. 139 [f. 28a, line 3 – f. 29a, line 1]; copy date: 768/1366
Y (ی): Balıkesir, Balıkesir İl Halk Kütüphanesi, Dursunbey ms. 54 [page 40, line 10 – page 42, line 4]; copy date: 1075/1664

Note on the manuscripts

An analysis of these copies has revealed that Ibn al-Šāṭir originally wrote the first part of *Nihāyat al-su'l* (" On the configuration of the heavens ") without a clear intention to add other parts. However, at the end of ms. F, Q, and M, Ibn al-Šāṭir indicates that he will add a second part that would include planetary " equations " (*ta'dīlāt*). This part seems never to have been written and might have been superseded by his *Zīj*. It would seem that subsequently he decided to add a different part 2, this one dealing with the configuration of the Earth (*hay'at al-arḍ*). Most of our manuscript witnesses contain this part 2. That being the case, Ibn al-Šāṭir, or a copyist, then changed the explicit that we find in ms. F, Q, and M, so that it now reads in our other manuscript witnesses that the second part is on the configuration of the Earth and a third part would be on " equations ". But like the original promise of a second part on equations, this third one was, as far as we can tell, also never written.

None of the manuscripts are free of errors, and there are real problems (as mentioned in the notes to the translation) with several of the parameters. It would

[109] One more copy of the *Nihāya* is Cairo, Dār al-kutub, Taymūr Riyāḍa, ms. 154, which is incomplete and does not include the chapter on Mercury. (See: David A. King, *Fihris al-maḫṭūṭāt al-'ilmiyya al-maḥfūẓa bi-dār al-kutub al-miṣriyya*, vol. 2 [Cairo, 1986], p. 35.)

seem, based on our experience with this chapter, that the textual tradition of *Nihāyat al-su'l* became corrupt at a fairly early stage. Ms. B, H, and M are arguably the best witnesses; ms. Q, which one might have expected to be reliable based on its date and provenance, turned out to be corrupt in a number of places. Of the Iranian manuscripts, ms. G was copied from ms. S, which itself is not particularly useful.

Apparatus conventions

[Separates reading in edition from any variant
: Separates variant and manuscript sigla
+ Added in
– Missing from
= Indicates another variant
(…) Editors' comments
[!] *sic*

الباب الحادي والعشرون في هيئة أفلاك عطارد على مذهبنا الموافق للرصد

نتوهّم فلكاً في سطح فلك البروج وعلى قطبيه ومركزه ويُسمّى الممثّل. ونتوهّم فلكاً ثانياً سطحه مائل عن سطح الممثّل نصف وربع درجة عند الأوج إلى جهة الجنوب. وهذا الميل غير ثابت، وعلى قولٍ مائل سدس جزء وهو ثابت الميل

[١] الباب الحادي والعشرون] الباب ٢١: ب، ح، د. [١] هيئة] –ف. [٣] نتوهّم] ح، ق، ل، م = يتوهم: ب، ى = فنتوهم: د (متغيّر من « فيتوهم ») = فيتوهم: س، گ = تتوهم: ف. [٣] ويُسمّى الممثّل] –ل. [٣] ونتوهّم] ح، ق، ل، م = ويتوهم: ب، س، گ، ى = د (متغيّر من « ويتوهم ») = وتتوهم: ف. [٤] مائل] (فوق السطر في ح مع رمز « صح »). [٤] نصف وربع درجة] مه دقيقة: د. [٤–٥] عند الأوج إلى جهة الجنوب] (في هامش ق). [٥] قولٍ] + انه: د. [٥] سدس] + درجه (مشطوب في م). [٥] سدس جزء] سدس جزؤ: ح، ى = ب دقيقه: د (+ « سدس » في الهامش مع رمز « خ ») = + جزء: ل.

وهو الأصحّ. وسطح المائل يقاطع سطح الممثّل على نقطتين متقابلتين، تسمّى إحداهما الرأس والأخرى الذنب. ونتوهّم فلكاً ثالثاً مركزه على منطقة المائل ونصف قطره أربعة أجزاء وخمس دقائق بالأجزاء التي بها نصف قطر المائل ستّون جزءاً، ويُسمّى الحامل. ونتوهّم فلكاً رابعاً مركزه على منطقة الحامل ونصف قطره نصف وثُلث درجة[!]، ويُسمّى المدير. ونتوهّم فلكاً خامساً مركزه على منطقة المدير ونصف قطره اثنان وعشرون جزءاً وستّ وأربعون دقيقة من تلك الأجزاء، ويُسمّى فلك التدوير. ونتوهّم فلكاً سادساً مركزه على منطقة التدوير ونصف قطره ثلاث وثلاثون دقيقة، ويُسمّى المحيط. ونتوهّم فلكاً سابعاً مركزه على المحيط ونصف قطره مثل نصف قطر المحيط وهو ثلاث وثلاثون دقيقة، ويُسمّى الحافظ وعطارد مركوز على منطقة هذا الفلك.

١ سطح] لسطح: س، گ. ١ الممثّل] (فوق السطر في ل). ١ متقابلتين] متقابلين: ل. ١ تسمّى] يسمى: ب، ف، ل. ٢ إحداهما] ب، ح، د، س، ف، ق، گ = احدهما: ل، م، ى. ٢ والأخرى] والاخر: ب. ٢ ونتوهّم] ح، د (متغيّر من « ويتوهم »)، ق، م = ويتوهم: ب، س، ف، گ، ى = سوهم: ل. ٢ مركزه] (محجوب في س)، (بياض في گ). ٣ أربعة أجزاء وخمس دقائق] د ه: د. ٣ قطر] + قطر: (مشطوب في ح) = + الفلك: س، گ. ٣ المائل] الممثل: ف = الميل: ق. ٣–٤ ستّون جزءاً] ٦٠ جزوا: د = ستون: ل. ٤ يُسمّى] + الفلك: س، گ = تسمى: م. ٤ ونتوهّم] ح، د (متغيّر من « ويتوهم »)، ق، ل، م = ويتوهم: ب، س، ف، گ، ى. ٥ نصف وثُلث درجة] ن دقيقه: د. ٥ ويُسمّى] + الفلك: س، گ. ٥ ونتوهّم] ح، د (متغيّر من « ويتوهم »)، م = ويتوهم: ب، س، گ، ى = تتوهم: ف = ونتوهم: ق = وسوهم: ل. ٦ جزءاً] جزوا: ى. ٦ اثنان وعشرون جزءاً وستّ وأربعون دقيقة] كب مو: د. ٧ الأجزاء] + المذكورة: س، گ. ٧ ويُسمّى] وتسمى: م. ٧ ونتوهّم] ح، د (متغيّر من « ويتوهم »)، م = ويتوهم: ب، س، گ، ى = وتتوهم: ف = ونتوهم: ق = وسوهم: ل. ٧ منطقة] + فلك: س، گ. ٨ ثلاث] ثلث: ح، ف، ق = ثلثه: ى. ٨ وثلاثون] ب، س، گ، م = وثلثون: ح، ف، ق، ل، ى = + درجة: ق (مشطوب). ٨ ثلاث وثلاثون دقيقة] لجـ ٠: د (يوجد رمز « ق » فوق السطر). ٨ المحيط] + والشامل أيضا: ل (في الهامش مع رمز « نخـ »)، ى. ٨ ونتوهّم] ح، د (متغيّر من « ويتوهم »)، ق، ل، م = ويتوهم: ب، س، گ، ى = وتتوهم: ف. ٩ على] + منطقة: ف، ق. ٩ مثل] ميل: ى. ٩ ثلاث] ثلث: ح، ف، ق، م، ى. ٩ وثلاثون] ب، س، گ = وثلثون: ح، ف، ق، ل، م، ى. ٩ ثلاث وثلاثون دقيقة] لجـ ٠ دقيقه: د.

وأمّا الحركات فإنّ الممثّل يتحرّك على قطبي البروج إلى التوالي في كلّ ستّين سنة درجة وذلك مثل حركة الأوجات. وأمّا المائل فإنّه يتحرّك إلى التوالي مثل حركة مركز عطارد وهو مثل مركز الشمس وهو في اليوم بليلته ٠ نط ح ى. وأمّا الحامل فإنّه يتحرّك في أعلاه إلى خلاف التوالي مثل حركة مركز عطارد أيضاً. وأما فلك التدوير فإنّه يتحرّك في أعلاه إلى التوالي بقدر فضل حركة خاصّة عطارد على حركة مركزه وهو في اليوم بليلته ٠ ب يح يد ب[!] وهو حركة بسيطة.

وأما حركة خاصة عطارد فإنّها بسيطة مركّبة لأنّها بقدر حركة هذا التدوير التي هي ٠ ب يح يد ب[!] مع حركة مركز عطارد التي هي ٠ نط ح ى، وذلك لكون الحركتين إلى جهة واحدة، فيحصل مفارقة الكوكب للذروة بقدر مجموع الحركتين وهي ٠ جـ و كد ى ا لح لز كح مب، وهي حركة خاصة عطارد المركّبة وهي مستوية عند مركز التدوير.

١ قطبي] + فلك: س، گ. ١ ستّين] ٦٠: د. ٢ سنة] –ل. ٢ درجة] + واحدة: س، گ. ٣ وهو] فهو: ف، ق. ٣ وهو في اليوم بليلته ٠ نط ح ى] + مط نب (فوق السطر في د) = وهو ٠ نط ح ى في اليوم بليلته: س = وهو ٠ ٠ نط ح ى في اليوم بليلته: گ. ٤ حركة] حركت: ى. ٥ فلك] –س، –گ. ٥ أعلاه] عله: گ. ٥ إلى] + خلاف: ب (فوق السطر)، ل، ى. ٦ وهو (الأوّل)] ب، ح، د، ل، م، ى = –س، –گ = وهي: ف، ق. ٦ ٠ ب يح يد ب] ٠ ب نح يد ب: ب، ى = + لعله ٠ ب و بو ٠ ط نا لط نو لط: م (في الهامش). ٦ وهو (الثاني)] وهذه: ف، ق، م. ٨ خاصة] –ف. ٨ لأنّها] + ٠ ب يح يد ب: س، گ. ٩ التي هي ٠ ب يح يد ب] ح، د، ق، م = التي هي ٠ ب نح يد ب: ب، ل، ى = –س، –گ. ٩ حركة] حركت: ى. ٩ هي] + في اليوم بليلته: س، گ. ١٠ لكون] الكون: گ. ١٠ فيحصل] فتحصل: ف، ق. ١٠ مفارقة] مفارقت: ى. ١٠ الكوكب] (في هامش ق). ١١ ٠ جـ و كد ى ا لح لز كح مب] ح، د، ق، م = كد ى ا لح لز مب: ب (ويوجد « ٠ حـ و » في الهامش) = ٠ جـ و كد ى ا لح لز كح مب ٠ ٠: س = ٠ جـ و كه ى ا لح لو كح مب ٠ ٠: گ = ٠ جـ و كد ى ا لح لز يح مب: ل = ٠ جـ و كد ى ا بح لر لح مب: ى. ١١ حركة] حركت: ى. ١٢ المركّبة] المركب: ى.

ومما يزيد ذلك إيضاحاً أنّه إذا تحرّك المائل ربع دائرة وتحرّك الحامل ربع دائرة وتحرّك المدير نصف دائرة، انتقلت الذروة التي هي مبتدأ حركته الخاصّة ربع دائرة إلى التوالي. وإنّما وجدت بالرصد أنّها منتقلة إلى التوالي مثل حركة خاصّة عطارد التي هي ٠ جـ و كد ى، فيكون حركة التدوير حول مركزه إلى التوالي بقدر فضل هذه الخاصّة على حركة المركز لكونهما إلى جهة واحدة. فقد اتّضح ذلك.

وأمّا الشامل فإنّه يتحرّك في أعلاه إلى التوالي مثل ضعف حركة مركز عطارد وهو في اليوم ا نح يو ك. وأمّا الحافظ فإنّه يتحرّك في أعلاه إلى خلاف التوالي أربعة أمثال حركة مركز عطارد وهي في اليوم جـ نو لب لط.

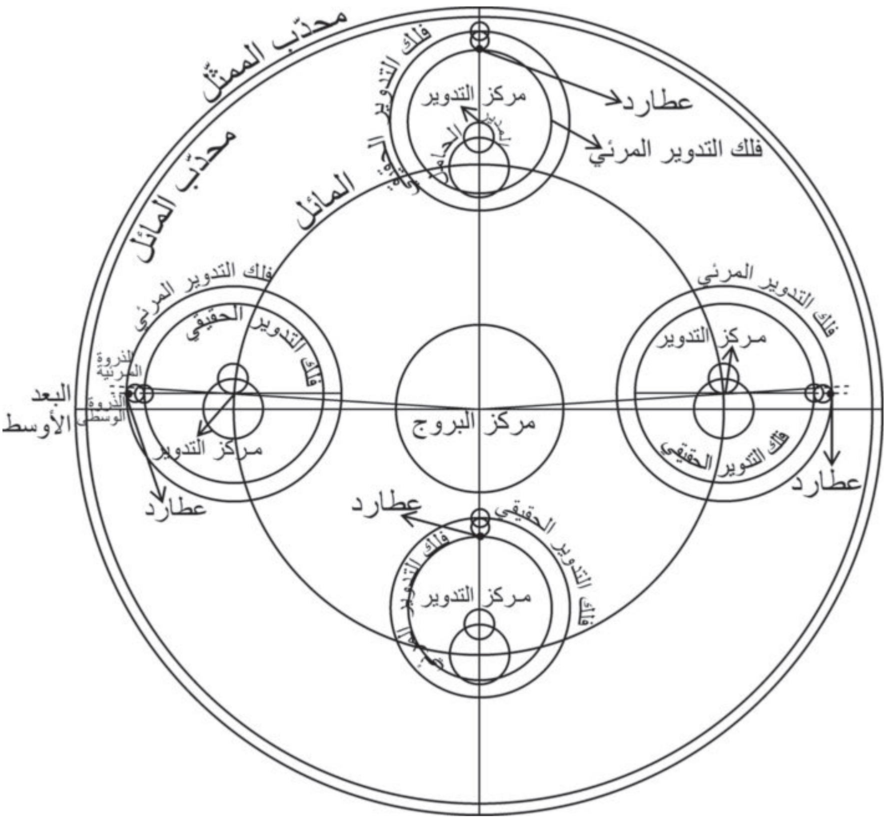
١ ذلك] ذالك: د، س. ١–٢ وتحرّك الحامل ربع دائرة] –ب، –ل، –ى. ٢ مبتدأ] ح، س، گ = مثل: ب، ل، ى = مبدأ: د، ف، ق = منند: م. ٢ حركته] ل = حركة: ب، ح، د، ف، ق، م = الحركة: س، گ = بحركت: ى. ٣ وجدت] وجد: ف، ق، م. ٣ بالرصد] بارصادنا: ى. ٣ منتقلة] ننقله: ب = منتقلت: ى. ٣ حركة] حركت: ى. ٤ ٠ جـ و كد ى] ب، د، ق، ل، م = ٠ ج و كد ى ا: ح = ٠ ج و كد ى ب: س، گ. ٤ فيكون] فتكون: د، ق، م. ٤ حركة] حركت: ى. ٤ حول] حركة: ب، ل = حركت: ى. ٥ بقدر] بقد: م. ٥ التوالي بقدر فضل] التوالي بعد: ب (« بعد » متغيّر إلى « بقدر »)، ل. ٥ هذه] (« هذه » متغيّر إلى « حركة » في ب) = + الحركة: (مشطوب في ق). ٥ على] (مشطوب في ب) = عند: د (« على » في هامش د مع رمز « خ »). ٥ حركة] وحركة: ب = + هذا: ف. ٥ بقدر فضل هذه الخاصّة على حركة المركز] بقدر حركت الخاصة و حركت المركز: ى. ٥ لكونهما] لكونها: س، گ. ٦ ذلك] –ل، –ى. ٧ الشامل] + وهو المحيط: س، گ. ٧ التوالي] –ى. ٧ حركة] –ى. ٨ ا نح يو ك] ل (+ « ا يح ٠ يح ك » في هامش ل مع رمز « بحـ »)، ى = ا نح يح ك: ب (« يح » متغيّر إلى « يو ») = ا يح ٠ يح ك: ح، د، ف، ق = ا يح ٠ يح ك بليلته: س، گ = ا نح ٠ يح ك: م. ٨ الحافظ] ب، ح، س، گ = الحافظة: د، ف، ق، ل، م، ى. ٨ فإنّه] ح، س، گ = فانها: ب، د، ف، ق، ل، م، ى. ٨ يتحرّك] تتحرك: د، ق، م. ٩ مركز] –ى. ٩ وهي] وهو: ى. ٩ اليوم جـ نو لب لط] ى = اليوم ج نو لو لط: ب (« لو » متغيّر إلى « لب »)، د، ف، ق، م = ج نو لز لط ٠ ٠: ح = اليوم بليلته ج نو لز لط: س = اليوم بليلته ج نو لو لط: گ = اليوم جـ نو لز لط: ل.

فلا يزال عطارد على الخطّ الخارج من مركز التدوير إلى مركز الشامل، يقرب من مركز التدوير ويبعد وهو على الخطّ غير خارج عنه. وإذا كان مركز التدوير في الأوج أو الحضيض كان عطارد في أقرب قربه من مركز تدويره، ويسمّى هذا القرب نصف قطر التدوير المرئي وهو أحد وعشرون جزءاً وثُلث[!] جزء. وإذا كان المركز ثلاثة بروج كان عطارد في أبعد بعده من مركز التدوير وهو ثلاث وعشرون درجة واثنتان وخمسون دقيقة. فيكون أبعد بعد عطارد من مركز العالم ستّة وثمانون وثُلثين وأقرب قربه ثلاثة وثلاثون وثُلث، إلّا أنّ عطارد لا يقرب إلى أقرب قرب أفلاكه المجسّمة على ما أوضحنا قبل في غير هذا الموضع.

[١] عطارد] عطار: ح. [٢] وإذا] اذا: ف، ق. [٣] أو] ب، ف، ق، ل، م، ى = و: ح، د، س، گ. [٣] تدويره] تدوير عطارد: ف، ق. [٤] نصف] –ف. [٤] التدوير] –ل. [٤] المرئي] المرى: ب، ف، ق، م، ى. [٤] أحد] احدي: س، گ. [٤] جزءاً] جزواً: س، گ، م، ى. [٤] أحد وعشرون جزءاً وثُلث جزء] كا ك: د. [٥] ثلاثة] ق = ثلاث: ب، ح، د، س، ف، گ، ل، م = ثلث: ى. [٥–٦] ثلاث وعشرون درجة واثنتان وخمسون دقيقة] كجـ نب: د (يوجد رمز « ق » فوق السطر). [٥] ثلاث] ثلث: ح، ف، ق، ى. [٦] وعشرون] عشرون: ف. [٦] درجة] جزواً: س، گ. [٦] واثنتان] ف، ق = واثنان: ب، ح، س، گ، ل، م، ى. [٧] ستّة] ست: س، گ. [٧] ستّة وثمانون وثُلثين] فو م: د. [٧] وثُلثين] ف، ق = وثلثي: ب، ح، س، گ، ل، م، ى. [٧] قربه] + ثلاث وعشرون جزوا واثنان خمسون دقيقه فيكون ابعد بعد عطارد من مركز العالم ست وثمانون وثلثين واقرب قربه: گ. [٧] ثلاثة] ب (متغيّر من « ثلاث »)، ل، م = ثلاث: س، گ = ثلثة: ح، ف، ق، ى. [٧] وثلاثون] ب = وثلثين: ح، ف، ق = وثلاثين: س، گ، م = وثلثون: ل. [٧] ثلاثة وثلاثون وثُلث] لج ك: د. [٧] وثُلث] ح، س، گ = وثلثا: ب، ف، ق، ل، م، ى. [٧] لا] ب، د، ف، ق، ل، م، ى = الا: ح، س، گ. [٧] يقرب] ب، ح، د، س، گ، ل = نقرب: ق = بقرب: م. [٨] قرب] قربه: ق = –ل. [٨] في غير هذا الموضع] ح، د، س، گ = في غير موضع: ب، ف، ق، ل، م، ى. [٨] هذا] (فوق السطر في ح).

وأما أقدار الأفلاك المجسّمة فإنّ نصف قطر كرة الحامل كح نب؛ ونصف قطر كرة المدير كد مز؛ ونصف قطر كرة التدوير كجـ نب؛ ونصف قطر كرة الشامل ا و؛ ونصف قطر كرة الحافظ ٠ لجـ، الجميع بالأجزاء التي بها نصف قطر الممثّل ستّون جزءاً. فيكون أبعد بعد الممثّل ثمانية وثمانين واثنتين وخمسين دقيقة. وفوق ذلك سمك الممثّل ولنفرضه تتمّة فط ل، وأقرب قرب أفلاكه لا ح وأقلّ من ذلك باتّصال الفلك فنفرضه لا ٠ والله أعلم.

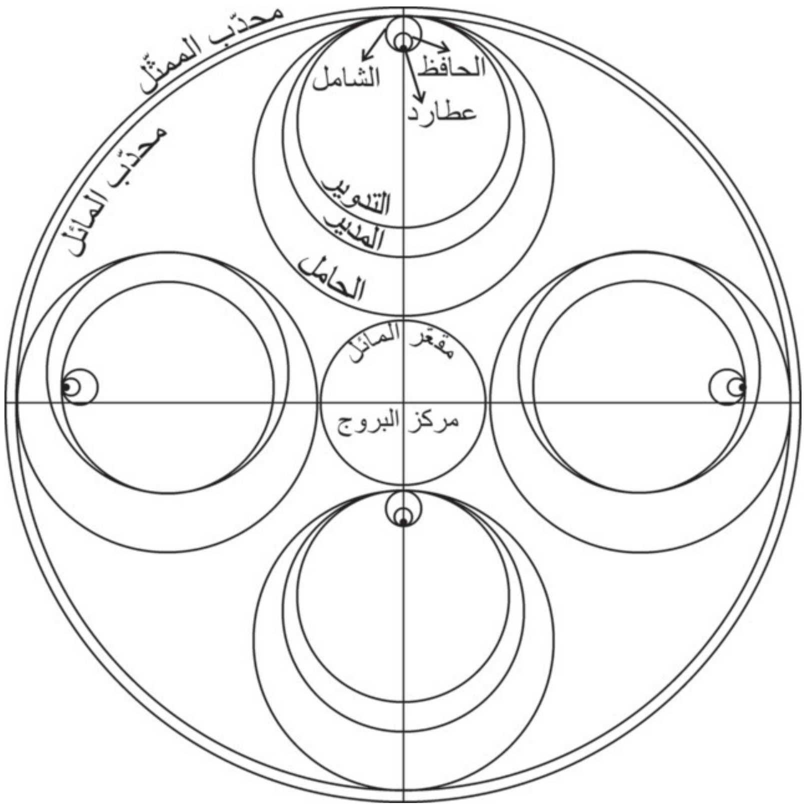
[١] الأفلاك] افلاكه: ب. [١] قطر كرة] ب، ق، ل، م = قطره: ح = قطر: د، س، گ. [١] كح نب] كح ىب: ب، ح، د، س، ف، ق، گ، ل، م، ى. [١–٢] ونصف قطر كرة المدير كد مز؛ ونصف قطر كرة التدوير كجـ نب] ونصف قطر كرة التدوير كجـ نب ونصف قطر كرة المدير كد مز: د. [٢] كجـ نب] ب، ح، س، ف، ق، ل، م، ى = كح نب: گ. [٢] كرة (الأوّل)] + التدوير: (مشطوب في ل). [٢] كد مز] ب، ح، ف، ق، ل، م، ى = كد مب: س، گ. [٣] ا و] ا ز: ب، ل، ى. [٣] ٠ لجـ] ح، س، ف، ق، گ، م = ا ٠ لجـ: ب (متغيّر من « ا و لجـ »)، ل، ى = ٠ لجـ ٠: د. [٤] ستّون] ٦٠: د = ستين: ف، ق. [٤] جزءاً] جزوا: د، س، گ، م. [٤] فيكون] فكون: ق. [٤] الممثّل (الثاني)] ل (ويوجد « المائل » في هامش ل مع رمز « ىح »). [٤–٥] ثمانية وثمانين واثنتين وخمسين دقيقة] فح نب: د (يوجد رمز « ق » فوق السطر)، س، گ = + واقرب قربه: (مشطوب في هامش ح). [٤] وثمانين] + درجة: (في هامش ل). [٤] واثنتين] ح، م = اثنين: ب = واسنتين: ف = واسن: ق = واسن: ل = —ى. [٥] وفوق ذلك] وفوقه: ى. [٥] الممثّل] للمل: م. [٥] ولنفرضه] ولتفرضه: ف. [٥] تتمّة] —د. [٥] فط ل] ب، ف، ق، ل، م، ى = نط ل: ح، د، س، گ. [٦] وأقلّ من ذلك] —ب = + نصف قطر الكوكب: (مشطوب في ق). [٦] باتّصال] واتصال: ف. [٦] فنفرضه] ونفرضه: س، گ. [٦] لا ٠] لا: ب. [٦] والله أعلم] —ب، —د، —ف، —ق.



[Fig. A1.]

صورة أفلاك عطارد على أنّها مراكز الأكر التامّة على حسب ما يتصوّر على البسيط في الأوج والحضيض والبعدين الأوسطين.

١ صورة] وهذه صورة: ح، د، س، گ. ١–٢ التامّة على حسب ما يتصوّر على البسيط في الأوج والحضيض والبعدين الأوسطين] ح، س، گ = –ب = + فتامله: د = التامة على حسب ما نفع على البسيط: ف = التامّة على حسب ما تقع على البسيط: ق = التامّة على حسب ما يتصوّر: ل = التامّة على حسب ما يتصوّر على البسيط: م = التامت على حسب ما يتصور في البسيط: ى.



[Fig. A2.]

وهذه صورة أفلاك عطارد المجسّمة وهي كرات تامّة على حسب ما يتصوّر على البسيط في الأوج والحضيض والبعدين الأوسطين.

[١–٢] وهذه صورة ... والبعدين الأوسطين] صورة أفلاك عطارد المجسّمة: ب = + والله اعلم: ح، د = صورة أفلاك عطارد المجسّمة وهي كرات تامّة على حسب ما يتصور على البسيط في الأوج والحضيض والبعدين: س = صورة أفلاك عطارد المجسّمة وهي كرات تامّة على حسب ما يتصور على البسيط في الأوج والحضيض والبعدين الأوسطين: ق، م = صورة أفلاك عطارد المجسّمة وهي كرات تامّة على حسب ما يصور على البسيط في الأوج الحضيض والبعدين: گ = وصورة أفلاك عطارد المجسّمة وهي كرات تامّة على حسب ما يتصور على البسيط في الأوج والحضيض والبعدين الأوسطين والله اعلم: ل = وفي الصفحة الاتية صورة افلاك عطارد في المجسمة الكراة التامة حسب ما يتصور على البسيط: ى. [١] وهذه] –ف. [٢] الأوسطين] الاسطر: ف.

Ibn al-Shāṭir

Sajjad Nikfahm-Khubravan

F. Jamil Ragep

Ibn al-Shāṭir

Ibn al-Shāṭir (b. probably 705/1306, d. 777/1375–6) was one of the most important astronomers of pre-modern Islam, writing on a variety of topics and producing one of the most innovative astronomical systems prior to the advances of early modern Europe. His full name is ʿAlāʾ al-Dīn Abū l-Ḥasan ʿAlī b. Ibrāhīm b. Muḥammad b. al-Humām Abī Muḥammad b. Ibrāhīm b. Ḥassān b. ʿAbd al-Raḥmān b. Thābit al-Anṣārī al-Awsī. Sources do not agree about his birth date, but the one reported by al-Ṣafadī (15 Shaʿbān 705/2 March 1306), who met Ibn al-Shāṭir, seems the most reliable (al-Ṣafadī, 20:302).

Ibn al-Shāṭir was born in Damascus. His father died when he was six, after which he was raised by a cousin on his father's side, who was married to Ibn al-Shāṭir's maternal aunt. His stepfather's name was ʿAlī b. Ibrāhīm b. Yūsuf b. al-Shāṭir, who was known as Ibn al-Shāṭir, whence the name under which our Ibn al-Shāṭir came to be known. His stepfather taught him the art of ivory inlaying *(taṭʿīm)*, so he became known as al-Muṭaʿʿim. He apparently earned a good living and lived in a fine house in the Bāb al-Farādīs quarter of Damascus (Ibn Ḥajar, 1:116; al-Ṣafadī, 20:302; cf. al-Maqrīzī, 2:526).

According to al-Ṣafadī (20:302), Ibn al-Shāṭir studied the mathematical sciences with his stepfather, ʿAlī b. Ibrāhīm. Later, in 719/1319, he travelled to Cairo and Alexandria to further his studies (Ibn Ḥajar, 1:116; cf. al-Maqrīzī, 2:526). During this period, Egypt was home to several prominent scientists working in astronomy, especially involving instruments and practical applications (King, Astronomy, 531, 534–5). Amongst these was Ibn al-Sarrāj (d. after 748/1347–8), with whom Ibn al-Shāṭir corresponded and exchanged treatises regarding an instrument known as *al-rubʿ al-mujannaḥ*, which Ibn al-Sarrāj invented and Ibn al-Shāṭir modified. Ibn al-Shāṭir's treatise is not extant, but Shams al-Dīn Muḥammad b. Abī l-Fatḥ al-Ṣūfī al-Miṣrī (fl. c.900/1495) summarised it in one of his treatises (Charette, 15 n. 63; al-Ṣafadī, 20:307).

Ibn al-Shāṭir was the long-time chief muezzin *(raʾīs al-muʾadhdhinīn)* and timekeeper *(muwaqqit)* at the Umayyad Mosque in Damascus (al-Ṣafadī, 20:302). His roles at the Umayyad Mosque secured his fame, and his works, as indicated by ownership notes, were esteemed by later generations of timekeepers (e.g., Tehran, Sepahsālār, MS 598, fol. 1a). Although Ibn al-Shāṭir never occupied a formal teaching position, Jamāl al-Dīn al-Māridīnī (d. 809/1406–7), who later became a timekeeper at al-Azhar Mosque in Cairo and was the grandfather of Sibṭ al-Māridīnī (d. c.900/1495), studied under him (King, Analog computer, 219–20 n. 2).

1. Ibn al-Shāṭir and astronomy

Ibn al-Shāṭir was firmly within the Hellenistic traditions of astronomy and their continuation in the Islamic world, and he had access to many of the works of his predecessors in these traditions. At some point, Ibn al-Shāṭir decided to test Ptolemy's (fl. c.140 C.E.) observations. This led him to write a work titled *Nihāyat al-ghāyāt fī aʿmāl al-falakiyyāt* ("The culmination of goals regarding astronomical operations"), which is not extant, but is based, according to Ibn al-Shāṭir in his *al-Zīj al-jadīd*, on Ptolemy's models in the *Almagest*. Later, basing himself on alternatives to Ptolemy's models, he wrote *Taʿlīq al-arṣād*, not extant, in which he established his new models based on his own observations (al-Ṣafadī, 20:306). The tradition of alternatives to Ptolemy's models dates back at least to Ibn al-Haytham (d. c.431/1040); this tradition found fault with Ptolemy's violations of the accepted physics that demanded uniform circular motions in the heavens resulting from the rotations of spherical orbs (Saliba, 134–70). Later astronomers in this tradition usually listed several problems with Ptolemaic models, ten of which were cited by al-Ṣafadī as well known. He went on to say that Ibn al-Shāṭir supplemented this list with an additional nineteen problems and claimed that he had solved them all in his *Taʿlīq al-arṣād*. Al-Ṣafadī notes, however, that Ibn al-Shāṭir wrote two monographs, *Maqāla fī qurb falak al-burūj min muʿaddil al-nahār* and *Maqāla fī ḥarakat* *al-iqbāl wa-l-idbār*, in which he denied the existence of two of the ten well known problems, namely, the variability of the obliquity of the ecliptic in the *Maqāla fī qurb*, and variable precession in the *Maqāla fī ḥarakat* (al-Ṣafadī, 20:304–6).

Ibn al-Shāṭir later presented his new models in *Nihāyat al-suʾl fī taṣḥīḥ al-uṣūl* ("The culmination of inquiry into correcting the hypotheses") but without the full derivations found in *Taʿlīq al-arṣād*. The *Nihāyat al-suʾl* is in the genre of *hayʾa basīṭa* (simplified theoretical astronomy, i.e., presented mostly without the geometrical derivations). Most of the extant manuscripts comprise an introduction and two additional parts: one on the configuration of the celestial realm *(hayʾat al-samāʾ)* and one on the configuration of the Earth, that is, the sublunary realm *(hayʾat al-arḍ)*. An additional part on the calculation of planetary equation tables (promised in some manuscript copies) seems never to have been written; he probably decided instead to write *al-Zīj al-jadīd*, several copies of which are extant. This *zīj* (an astronomical handbook with tables) is innovative (thus *jadīd*, new); in it, the new models of *Nihāyat al-suʾl* were used instead of the standard Ptolemaic models. Al-Ṣafadī mentions a *zīj* written by Ibn al-Shāṭir for Sayf al-Dīn Tankiz (d. 740/1340), the Damascus-based viceroy of Syria, whence it is called *al-Zīj al-Sayfī*. In the *Nihāyat al-suʾl* and *al-Zīj al-jadīd*, Ibn al-Shāṭir refers to works by Ptolemy; Ibn al-Haytham; Jābir b. Aflaḥ (fl. first half of the sixth/twelfth century); Ibn Rushd (Averroës, d. 595/1198); al-Biṭrūjī (Alpetragius, fl. 586/1190, in al-Andalus; Ibn al-Shāṭir incorrectly calls him al-Majrīṭī); Muʾayyad al-Dīn al-ʿUrḍī (d. c.664/1266); Naṣīr al-Dīn al-Ṭūsī (d. 672/1274); Muḥyī l-Dīn al-Maghribī (d. 682/1283); and Quṭb al-Dīn al-Shīrāzī (d. 710/1311).

Although Ibn al-Shāṭir is often included in the so-called Marāgha School (Marāgha was the site of a famous observatory) of al-ʿUrḍī, al-Ṭūsī, and al-Shīrāzī, his models differ fundamentally, inasmuch as he insists on making the Earth both the mathematical and cosmological centre of the Universe. This is accomplished by dispensing with eccentric orbs (ones surrounding the Earth but with different centres) and using only Earth-centred orbs and epicycles (orbs that do not surround the Earth). This seems to be a compromise system that solves Ptolemy's violations using epicycles rather than eccentrics while making the Earth the primary mathematical centre. This "quasi-homocentric" cosmology may well owe something to the works of sixth/twelfth-century Andalusians such as Ibn Rushd and al-Biṭrūjī, who sought to return to the pure homocentric system of Aristotle (Ragep, 408). Unlike the astronomy of al-Biṭrūjī, however, Ibn al-Shāṭir's models can faithfully reproduce Ptolemy's mathematical results, which generally represent celestial motions accurately.

With the exception of *al-Zīj al-jadīd*, Ibn al-Shāṭir's works had less influence than one might expect. The manuscript tradition of his works is spotty; many works are lost, and important works, such as *Nihāyat al-suʾl*, are replete with copyists' errors. Nevertheless, references to him and his work are not uncommon in Islamic lands, and there is strong evidence that he was known in other cultural contexts.

We know that Shams al-Dīn al-Miṣrī and Taqī al-Dīn Ibn Maʿrūf al-Rāṣid (d. 993/1585), two prominent astronomers of the early modern period, owned copies of *Nihāyat al-suʾl* (Oxford, Bodleian

Library, MS Marsh 139, fol. 64b, owned by Shams al-Dīn in 908/1502–3; Tehran, Sepahsālār, MS 598, fol. 1a, owned by Taqī al-Dīn in 970/1562–3). In his *Sidrat muntahā al-afkār fī malakūt al-falak al-dawwār* ("The Lotus Tree of ultimate contemplation regarding the realm of the revolving orb"), Taqī al-Dīn criticised Ibn al-Shāṭir's models (Istanbul, Nuruosmaniye, MS 2930, fol. 2a). The *Nihāyat al-su'l* was also mentioned by Ghars al-Dīn Ibn Aḥmad b. al-Khalīl al-Ḥalabī (d. c.971/1563–4; see Rosenfeld and İhsanoğlu, 327) and ʿAbd al-Qādir b. Muḥammad al-Manūfī al-Shāfiʿī (fl. 980/1572–3; see Rosenfeld and İhsanoğlu, 340). *Al-zīj al-jadīd* was popular, and numerous commentaries, super-commentaries, and abridgements of it are extant (ʿAzzāwī, 51–2; King, *Survey*, 62).

Since the 1950s, there has been strong evidence, based on remarkable similarities, that Nicholas Copernicus (d. 1543 C.E.), when writing his early work known as the *Commentariolus*, knew of Ibn al-Shāṭir's planetary models (Roberts; Kennedy and Roberts; Swerdlow and Neugebauer, 61 and passim). It has also lately come to light that a Jewish scholar named Moses Galeano brought knowledge of Ibn al-Shāṭir's models to Italy at about the time Copernicus was studying there (Langermann, 290–6; Morrison). Most historians have argued that Ibn al-Shāṭir's models showed Copernicus a way to resolve some of the irregularities of Ptolemy's models, but they had little to do with his turn to heliocentrism. An argument has, however, recently been made that Ibn al-Shāṭir's models exhibit a "heliocentric bias" that may well have influenced Copernicus's decision to propose a new, Sun-centred cosmology (Ragep, 396).

2. Ibn al-Shāṭir and astronomical instruments

Al-Ṣafadī records a meeting that took place in Ramaḍān 743/February 1343 at Ibn al-Shāṭir's home in Damascus, at which time he was shown an interesting astrolabe with an attached clock, both of which were automated (al-Ṣafadī, 20:302–4). This is just one of Ibn al-Shāṭir's contributions to the long-standing Islamic tradition of making astronomical instruments; this tradition included both improving existing instruments and inventing new ones. These instruments were for 1) observation and measurement, 2) the simulation of heavenly motions, and 3) solving problems in spherical astronomy. Names of astronomical instruments invented by Ibn al-Shāṭir and his monographs on each are as follows (the first five are mentioned by al-Ṣafadī, 20:307):

1) *Al-rubʿ al-tāmm li-mawāqīt al-Islām* ("The complete quadrant for timekeeping in Islam"), described in *al-Nafʿ al-ʿāmm fī l-ʿamal bi-l-rubʿ al-tāmm* ("The general advantage of using the complete quadrant"), in which Ibn al-Shāṭir promised an abridged version of the treatise, which is probably *al-Risāla lil-rubʿ al-tāmm* ("Treatise on the complete quadrant") = *Risāla fī l-ʿamal bi-l-rubʿ al-tāmm al-mawḍūʿ li-mawāqīt al-Islām* ("Treatise on the use of the complete quadrant as applied to timekeeping in Islam"); King, *Survey*, 62).

2) *Al-rubʿ al-jāmiʿ* ("The universal quadrant"), originally described in *Tuḥfat al-sāmiʿ fī l-ʿamal bi-l-rubʿ al-jāmiʿ* ("The gift to the learner on the use of the universal quadrant"), which is not extant; its abridgement by Ibn al-Shāṭir himself, *Nuzhat al-sāmiʿ fī l-ʿamal bi-l-rubʿ al-jāmiʿ* ("The learner's delight on the use of the

universal quadrant"), exists; see King, *Fihris*, 2:543).

3) *Al-mamarrāt al-āfāqiyya* ("Horizon transits") (apparently not extant).

4) *Al-rubʿ al-mujannaḥ* ("The 'winged' quadrant"), which is probably the modified version of the instrument invented by Ibn al-Sarrāj, mentioned by Shams al-Dīn al-Miṣrī.

5) *Al-āla al-jāmiʿa* ("The universal instrument"), described in *al-Ashiʿʿa al-lāmiʿa fī l-ʿamal bi-l-jāmiʿa* ("Shining rays on the use of the universal [instrument]"). Abū ʿAlī al-Marrākushī (d. c.700/1300) is mentioned in this treatise. Taqī al-Dīn al-Rāṣid's *al-Thimār al-yāniʿa min quṭūf al-āla al-jāmiʿa* ("Ripe fruits from the harvest of the universal instrument") was inspired by it; King, Fihris, 2:533).

6) *Ṣandūq al-yawāqīt* ("Box of gems/sapphires"), a multi-purpose instrument in which a magnetic compass was fitted in order to align it in the cardinal directions, described in *Tashīl al-mawāqīt fī l-ʿamal bi-ṣandūq al-yawāqīt* ("Facilitating timekeeping by using the box of gems"), which is not extant; on the instrument itself, see Janin and King, 190).

7) *Al-rubʿ al-ʿAlāʾī* ("The ʿAlāʾī quadrant"), described in *al-Risāla fī l-rubʿ al-ʿAlāʾī* ("Treatise on the ʿAlāʾī quadrant"; Schmalzl, 100).

8) *Al-murabbaʿa* ("The square instrument"), attributed to Ibn al-Shāṭir by Ibn al-ʿAṭṭār (King, *Survey*, 62–3). A certain Ibn al-Ghuzūlī composed in 779/1377–8 a treatise based on a work by Ibn al-Shāṭir dealing with *al-murabbaʿa* (Charette, 17). King suggests that Ibn al-Shāṭir is the author of the anonymous treatise in Cairo that is about the same instrument.

9) *Al-rubʿ al-kāmil* ("The perfect quadrant"), described in *Risālat al-rubʿ al-kāmil* ("Treatise on the perfect quadrant").

Ibn al-Shāṭir also wrote several works on instruments that were invented before him: 1) *al-Ishārāt al-ʿimādiyya fī l-mawāqīt al-sharʿiyya* ("Fundamental indications on legally sanctioned timekeeping"), or *Risāla fī l-ʿamal bi-l-usṭurlāb wa-rubʿ al-muqanṭarāt wa-l-rubʿ al-mujayyab* ("Treatise on the use of the astrolabe, the almucantar quadrant, and the sine quadrant"); 2) *Īḍāḥ al-mughayyab fī l-ʿamal bi-l-rubʿ al-mujayyab* ("Elucidation of the obscure regarding the use of the sine quadrant"); 3) *Kashf al-mughayyab fī l-ḥisāb bi-l-rubʿ al-mujayyab* ("Uncovering the obscure regarding calculation with the sine quadrant"); 4) *al-Zubd al-marʾī fī l-ʿamal bi-l-jayb bi-ghayr murī* ("The manifest essence on the use of the sine quadrant without the *muri* [string calculator]").

We are fortunate to have several instruments made by Ibn al-Shāṭir: 1) an astrolabe, bearing the date 726/1326, currently held by the Observatoire National, Paris (Mayer, 42); 2) an exemplar of his *al-Āla al-jāmiʿa* ("Universal instrument") made in 738/1338 and dedicated to Shaykh ʿAlī b. Muḥammad al-Darbandī, in the Museum of Islamic Art, Cairo (Mayer, 40); 3) another exemplar of *al-Āla al-jāmiʿa*, bearing the same year and dedicatee, in Bibliothèque Nationale, Paris (Mayer, 41); 4) an exemplar of his original instrument, the *Ṣandūq al-yawāqīt* ("Box of sapphires"), dated 767/1366, recently located in Aleppo (present location and situation unknown; Reich and Wiet, 195; Janin and King, 187); it was dedicated to Mankalī-Bughā, the viceroy of Aleppo (d. 774/1372–3); 5) fragments of his sundial, dated 773/1371–2 and constructed for the Umayyad Mosque in Damascus, preserved in the National Museum, Damascus; a replica of it was made in about 1873 by a certain Shaykh Muḥammad al-Ṭanṭāwī (d. 1886) and placed in the Umayyad Mosque (Badrān, 365; King, Ibn al-Shāṭir, 361).

3. Other works by Ibn al-Shāṭir

Three works on mathematics are attributed to Ibn al-Shāṭir, but there is no known copy: *al-Maḥṣūl fī ḍabṭ al-uṣūl* (on geometry), *Kitāb fī l-misāḥa* (on surveying), and *Kitāb fī l-ḥisāb* (on arithmetic). There are also several works that have been attributed to Ibn al-Shāṭir but are uncorroborated, including A5-A9, A16, A19, A21-A25, A28, A30-A32, and A34-A35 (Rosenfeld and İhsanoğlu, 254–6).

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Sajjad Nikfahm-Khubravan
F. Jamil Ragep

Section IV

Other Islamic Connections with Copernicus

ᶜALĪ QUSHJĪ AND REGIOMONTANUS: ECCENTRIC TRANSFORMATIONS AND COPERNICAN REVOLUTIONS

F. JAMIL RAGEP, University of Oklahoma

In 1973, Noel Swerdlow presented a new and significant reconstruction of how Copernicus arrived at the heliocentric theory.[1] This reconstruction was based upon several bits of newly-interpreted information, most importantly a set of notes in Copernicus's hand contained in an Uppsala University manuscript. These notes provided compelling evidence that Copernicus had transformed Ptolemy's epicyclic models of the planets into eccentric models as a first step in developing a Sun-centred astronomy.[2] But this transformation depended upon a general proposition that one could indeed convert all the epicycle models into eccentric ones. Curiously, Ptolemy denied this, claiming in Book XII of the *Almagest* that this was possible only for the outer planets (Mars, Jupiter, and Saturn) but not the inner ones (Mercury and Venus). From a modern perspective this seems odd, and it is not entirely clear why Ptolemy could not see that the epicycles of the inner planets, with a proper consideration of speeds, could also be converted into eccentrics. Indeed, Ptolemy's modern translator Gerald Toomer says: "I do not understand why Ptolemy does not recognize this."[3]

Be that as it may, it would seem that no one else recognized this until the fifteenth century. Swerdlow found what he believed to be the source for the propositions Copernicus needed to begin his conversions, namely Book XII, Chapters 1 and 2 of Regiomontanus's *Epitome of the Almagest*.[4] In Chapter 2, Regiomontanus gives a brief sketch and proof of the crucial theory for the inner planets, which would allow Copernicus to convert all the planets from epicyclic to eccentric models. Though Copernicus is sparing in his references and nowhere cites Regiomontanus for these propositions, his use of the *Epitome* is well-documented, and there would seem to have been no other European source that he could have depended upon.[5]

Whatever subsequent use was made of them, Regiomontanus's own motivation for including these propositions at the beginning of Book XII has remained unclear. Swerdlow himself signalled this when he stated: "For some reason the eccentric model must have caught Regiomontanus's attention…."[6] And Michael H. Shank has recently remarked that "We do not yet know specifically what, apart from his compulsive thoroughness, motivated Regiomontanus to explore the eccentric models of the second anomaly".[7] What is especially odd about Regiomontanus's interest is that it is apparently so unprecedented. Neither in Europe nor in the Islamic world does this eccentric alternative alluded to by Ptolemy seem to have generated much interest. And the motivation to extend this alternative to the lower planets, after being rejected by the great authority himself, is even more puzzling. Finally, there is the odd way in which Regiomontanus presents the two propositions. He himself gives no motivation — he just presents them. There is no mention of Ptolemy, no statement that Ptolemy was wrong, no explanation of why Ptolemy made his mistake, no claim of credit.

One possibility is that Regiomontanus does not claim credit because he was not in fact the originator of the proposition. Indeed, it would seem, based on evidence presented in the sequel, that an older contemporary of Regiomontanus named ᶜAlī Qushjī may well have been the discoverer of this crucial proposition and that Regiomontanus learned of it either while in Italy or through the intermediation of Cardinal Bessarion, who had originally suggested to Regiomontanus and his collaborator Georg Peurbach that they write the *Epitome*.[8]

Most readers of this journal will be acquainted with Regiomontanus and Peurbach, and perhaps even Bessarion, but ᶜAlī Qushjī is most likely an unknown figure. This is regrettable since he is, at least in my opinion, one of the major figures in astronomy of the fifteenth century.

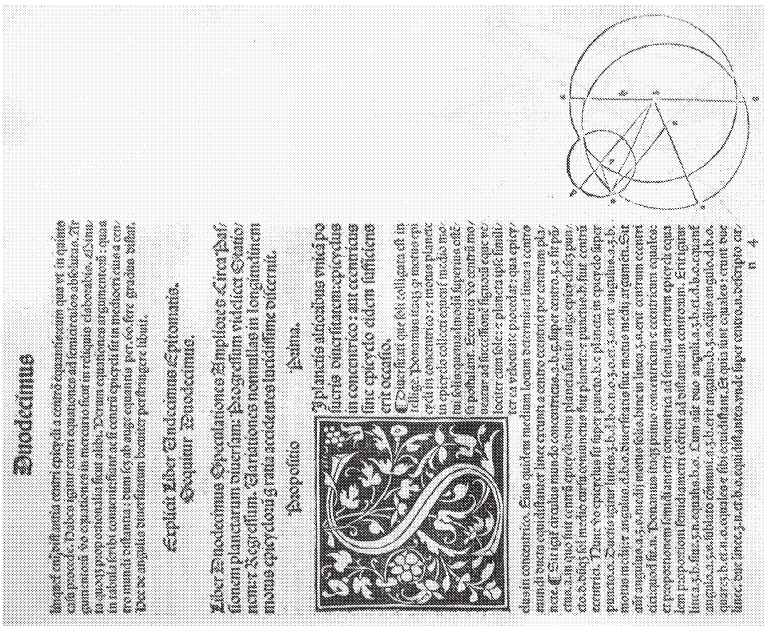
ᶜAlī Qushjī was a son of a falconer,[9] but not just any falconer. His father worked at the Samarqand court of Ulugh Beg, a grandson of Tīmūr Lang (Tamerlane: 1336–1405). Ulugh Beg was governor of Transoxiana and Turkestan from 1409 until 1447, at which time he briefly became the supreme Timurid ruler until he was killed by order of his son in 1449. A major patron of the arts and sciences, in particular the mathematical sciences, Ulugh Beg attracted to Samarqand a wide array of scientists who taught at the *madrasa* (school) and worked at the impressive astronomical observatory.[10] It was in this environment that Qushjī received his education under such luminaries as Jamshīd al-Kāshī (d. *c.* 1429), Qāīḍzāde al-Rūmī (d. after 1440), and Ulugh Beg himself. After the deaths of Kāshī and Rūmī, Qushjī may have assumed a primary role at the observatory, whose main product was the *Zīj* (astronomical handbook with tables) of Ulugh Beg. When Ulugh Beg was assassinated, Qushjī was forced to seek patronage at a number of courts in Central Asia and Persia. One of his most important works during this later period was his commentary on Naṣīr al-Dīn al-Ṭūsī's theological work, the *Tajrīd al-ᶜAqā'id*. His renown spread to the Ottoman conqueror of Constantinople, Mehmet II, who invited him to Constantinople where he became a professor of the mathematical sciences at two *madrasa*s. Although he spent only two or three years in Constantinople before his death in 1474, Qushjī's influence continued in Ottoman circles for centuries as a result of his writings and the activities of his students.[11]

One thing that seems to have been emphasized in the Samarqand School was the importance of the mathematical sciences. Biographical accounts of Qāḍīzāde al-Rūmī, for example, highlight his difficulties with his teacher al-Sayyid al-Sharīf al-Jurjānī, who thought his student overly interested in mathematics at the expense of philosophy.[12] Kāshī is also noted for his embrace of the mathematical sciences, as we can see from his letters to his father,[13] and Ulugh Beg, like a number of Mongol/Turkic rulers, was predisposed to support the mathematical sciences; in addition, he himself became proficient in them.[14] It was in this atmosphere that the young ᶜAlī Qushjī was raised, and this seems to have had a profound effect upon his intellectual outlook. In his commentary on Ṭūsī's *Tajrīd*, for example, he makes the rather startling case that astronomy should dispense with its dependence upon Aristotelian physics.[15] Even more surprising, he there claims that since there are no good observational proofs for the Earth's motion and since he does not wish to depend upon Aristotle's natural philosophical arguments, the Earth's rotation is a possibility.[16]

It is with this background in mind that we can now turn to his proof that eccentric models could be used for the two lower planets. The motivation for dealing with this problem seems to have arisen in the context of his work on a Mercury model that could serve as an alternative to Ptolemy's.[17] Qushjī was in a long line of Islamic astronomers who objected to the irregular (i.e. non-uniformly rotating) motions contained in several of Ptolemy's planetary models and who had not infrequently proposed alternative models.[18] In the course of his presentation, Qushjī remarks that Ptolemy had mentioned that it was not possible to assign an eccentric as a substitute for Mercury's epicycle to represent the second anomaly, i.e. that having to do with the planet's relationship with the Sun. This was because observation showed that the time between the fastest motion and mean motion was always greater than between mean motion and least motion, a situation Ptolemy contended could be represented by an epicyclic hypothesis (in which the epicycle rotation at the apex was in the same direction as its deferent) but not by an eccentric hypothesis.[19] But Qushjī dismisses this contention, saying "the situation is not as stated by Ptolemy". He then claims that he has a "geometrical proof" but that it would not be appropriate to present it in this treatise on Mercury.[20] And indeed a several-page excursus in a four- or five-folio work would not have been appropriate.

The geometrical proof forecast in the Mercury treatise is clearly contained in the text edited and translated below. But the context is somewhat different. In the Mercury treatise, as we have seen, Qushjī refers to *Almagest* IX.5 in which Ptolemy denied that an eccentric hypothesis could account for the asymmetrical times in the second anomaly of the five retrograding planets. In this treatise, however, the focus is on XII.1, where Ptolemy actually presented just such a model for the upper planets (though without making an explicit connection to IX.5) but denied it for the lower ones. One might speculate that Qushjī had come upon his proposition while experimenting with different models for Mercury and, contra-Ptolemy, had tried to substitute an eccentric for the epicycle, which might explain why he was interested in the asymmetries of time in Mercury's second anomaly. But by the time he came to publish his proof, Qushjī perhaps noticed that Ptolemy in XII.1 had implicitly contradicted his statement in IX.5 as far as the upper planets were concerned and all that remained was to show that the epicycle models of the lower planets could be transformed into eccentrics as well.

Qushjī would seem to be claiming at least some priority for his discovery. He states that "most" of the experts have agreed with Ptolemy in denying that eccentrics could be used to replace epicycles for the lower planets, citing in particular Quṭb al-Dīn al-Shīrāzī (1236–1311). But the fact that he does not say "all" experts could be interpreted as meaning that someone may have questioned Ptolemy on this point. At any rate, from what Qushjī says in the Mercury treatise, he wants to take credit



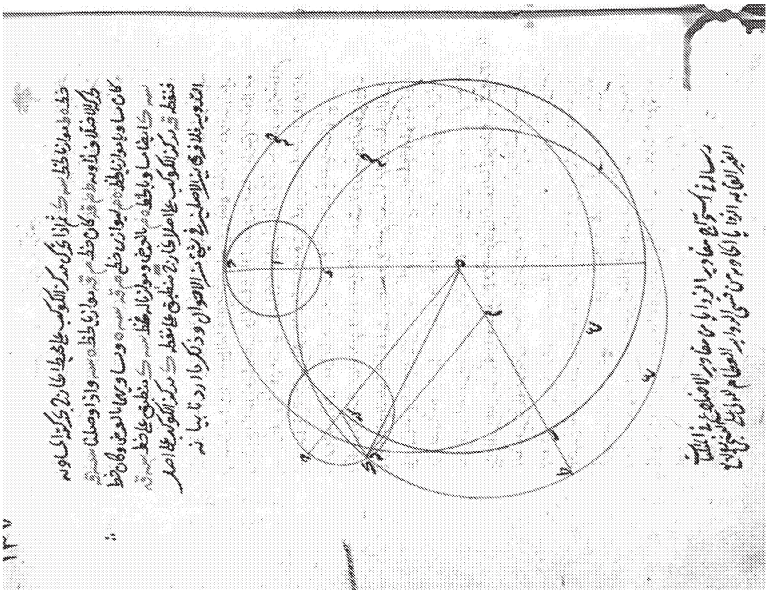


FIG. 1. Comparison of diagrams of Regiomontanus and Qushjī.[28] (*Left*) J. Regiomontanus and G. Peurbach, *Epytoma Joannis de monte regio In almagestum ptolemaei* (Venice, 1496), n4r, and (*right*) ʿAlī Qushjī, *Fī anna aṣl al-khārij…*, Carullah MS 2060, f. 137a. Reproductions courtesy of the History of Science Collections, University of Oklahoma Libraries, and of the Süleymaniye Library, Istanbul, respectively.

at least for the geometric proof.

Unfortunately, none of the three extant manuscripts provides a date of composition, but we can give an approximate date based upon other evidence. It would seem reasonable to date it to a time shortly after the Mercury treatise. Since that work cannot be precisely dated either, we can only assign both to within a certain range. Obviously the Mercury treatise was written before Ulugh Beg's assassination in 1449, since it is dedicated to him. Saliba makes a good argument for dating it to sometime in the 1420s, after Qushjī returned to Samarqand from a period of exile brought on by court intrigue.[21] And İhsan Fazlıoğlu, on the basis of other evidence, has further refined the date to *c.* 1428.[22] So we would not be too far amiss to assign a date of *c.* 1430 for this treatise on the eccentric hypothesis.

How much further did Qushjī or his students go with his discovery? Swerdlow has stated that "Copernicus's derivation of his theory rests upon the eccentric model of the second anomaly and therefore upon these two propositions in the *Epitome*. In this way Regiomontanus provided the foundation of Copernicus's great discovery. It is even possible that, had Regiomontanus not written his detailed description of the eccentric model, Copernicus would have never developed the heliocentric theory".[23] Swerdlow goes on to claim that "While I do not believe that Regiomontanus ever advocated the heliocentric theory, he was, through these two propositions, virtually handing it to any taker".[24] Can we say the same for Qushjī? Since research has just begun into the legacy of ᶜAlī Qushjī, in particular into the Istanbul circle of scientists that he helped initiate, we can only speculate. But it is certainly of considerable interest that Qushjī, like Copernicus, was open to the possibility of the Earth's rotation based upon a new, non-Aristotelian physics.[25]

Inevitably these sorts of discoveries raise anew the question of transmission of late (post-1200) Islamic astronomy to the West. Because of the paucity of research from Europeanists, we do not as yet have a great deal of knowledge of how and under what circumstances this and other products of Islamic science might have been received in the period after the translation movements of twelfth-century Spain and Sicily.[26] But the mounting number of 'coincidences' between early modern European astronomy and late medieval Islamic astronomy can only be held to be 'parallel' developments if one accepts the increasingly implausible idea that somehow the 500-year tradition of non-Ptolemaic astronomy in Islam was recapitulated in Europe in scrupulous detail in a 50-year span in the last part of the fifteenth century.[27]

TRANSLATION AND TEXT

The edited Arabic text presented below is based upon the three extant manuscripts. There are few textual problems. My comments are given as endnotes to the English translation.

The following are the manuscripts, *sigla*, and abbreviations that have been used:

C Istanbul, Süleymaniye Library, Carullah MS 2060, ff. 136a–137a

H Bursa, Yazma Library, Hüseyin Çelebi MS 751, ff. 124a–125a

L	Istanbul, Süleymaniye Library, Lâleli MS 3743, f. 60a
[	Separates reading in edition from any variant
:	Separates variant and manuscript *sigla*
+	What follows is an addition to the text
C_{ab}	Above the line in C
C_{mr}	In the margin of C
C_{un}	Under the line in C

Treatise on the Eccentric Hypothesis[29] Being Possible for the Two Lower [Planets] Just as for the Others By Master ᶜAlī Qushjī

In the name of God, the beneficent, the merciful. In Him is my trust.

The author of the *Almagest* held that the eccentric hypothesis is possible for the three [planets] that can be at all elongations from the Sun but that it was not possible for the two lower planets since this hypothesis results in all elongations whereas these two do not become elongated from the Sun except by a small amount.[30] So only the epicyclic hypothesis is possible for them. Most experts have agreed with him on that, including our master, the most learned author of the *Tuḥfa*.[31] But perhaps they came to this conclusion at first glance when they thought that the middle of direct and of retrograde motion, according to the eccentric hypothesis, would need to be at the apogee and perigee, whose positions on the orb are in opposition. They believed that according to the eccentric hypothesis the planet, since it is in conjunction with the mean Sun at the middle of direct motion, would be in opposition to [the Sun] at the middle of its retrogradation, and vice versa. So in going from the middle of direct motion to the middle of retrogradation, it will undergo all elongations from the Sun, which is contrary to the epicyclic hypothesis; in the [latter] case, the middle of the direct motion and of the retrogradation occur at the apex and the [epicyclic] perigee, their positions on the orb being the same.[32]

Thus according to the epicyclic hypothesis, the two lower planets will be in conjunction with the mean Sun both at the middle of direct motion and at the middle of retrogradation. So they will not experience all elongations from the mean Sun in going from the middle of direct motion to the middle of retrogradation; rather they do not become elongated from it except in the amount that is dictated by the epicycle radius.

But the situation is not as they have believed. For the mean motion according to the eccentric hypothesis could rather proceed sequentially in the amount of the sum of the Sun's mean motion and the motion of anomaly, while the motion of the eccentric is counter-sequential in the amount of the motion of anomaly.[33] So in the amount by which the eccentric causes the planet's centre to elongate from the mean Sun counter-sequentially, the eccentric's deferent by its motion sequentially will restore it. So the elongation between the planet's centre and the mean Sun will end up being none other than the amount of the equation as it was according to the epicyclic hypothesis, [whereby] it will be elongated from it only by the amount of

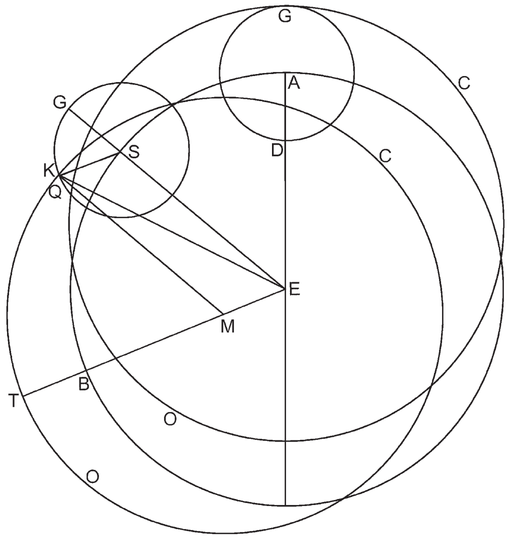


FIG. 2

the equation. So the equation at each instant according to the two hypotheses is the same.[34] Thus the centre of the two lower planets will only become elongated from the mean Sun by the same amount according to each of the two hypotheses.

In order to prove this: let circle *AB* with centre *E* be the equator[35] of both the deferent of the eccentric and of the epicycle; circle *GD* the equator of the epicycle; circle *CO* about centre *M* the equator of the eccentric. Let us take the planet to be in the middle of direct motion at the apogee of the eccentric [according to the eccentric hypothesis or] at the apex of the epicycle according to the epicyclic hypothesis. Then let the centre of the epicycle move through angle *AES* by the mean motion[36] and the centre of the planet through angle *GSK* by the motion of anomaly. We join *EK*. We will show that the centre of the planet according to the eccentric hypothesis is also at point *K*. This is so since if the eccentric apogee moves by the motion of the eccentric's deferent with a motion equal to the sum of the mean motion and anomaly through angle *AET*, then angle *SEB* is its excess over the mean and is equal to angle *GSK*, which is the anomaly. Therefore line *ET* is parallel to line *SK*. Then when the centre of the planet moves on the circumference of the eccentric with [the eccentric's] motion through angle *TMQ*, which is equal to the motion of anomaly, line *MQ* will be parallel to line *ES*.[37] When we join *SQ*, it will be equal and parallel to line *EM* since lines *MQ* and *SE* are parallel and are equal by assumption. Line *SK* is also equal to line *EM* by assumption and is parallel to it. So line *SK* is coincident with line *SQ*. Therefore point *Q* is the centre of the planet according to the eccentric hypothesis and coincident with point *K*, which is the centre of the planet according to the epicyclic hypothesis. So there is no difference between the two hypotheses in any particular. That is what we sought to prove.[38]

(C:136a; H:124a; L:60a)

رسالة في أنّ أصل الخارج يمكن في السفليين كما في غيرهما للمولى علي القشجي[1]

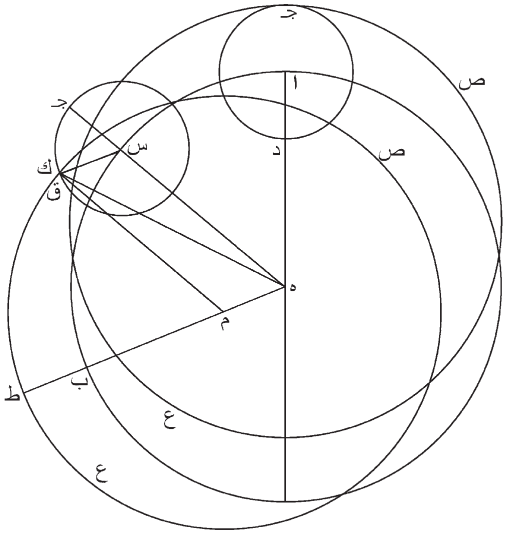
(C:136b; H:124b)

بسم الله الرحمن الرحيم وبه ثقتي[2]

ذهب صاحب المجسطي إلى أن أصل الخارج إنّما يمكن في الثلاثة[3] التي يبعد عن الشمس كلّ البعد ولا يمكن في السفليين لاقتضاء هذا الأصل كلّ الأبعاد وهما[4] لا يبعدان عن الشمس إلا بقدر[5] يسير ففيهما لا يمكن إلاّ أصل التدوير ووافقه على ذلك أكثر المحققين ومنهم مولانا العلاّمة صاحب التحفة ولعلّهم إنّما[6] حكموا بذلك لمّا رأوا في بادئ أنظارهم أنّ وسطي الاستقامة والرجعة على أصل الخارج إنّما يكونان عند الأوج والحضيض وموضعاهما على الفلك متقابلان وظنّوا أنّ الكوكب[7] على أصل الخارج إن كان في وسط الاستقامة مقارناً لوسط الشمس فهو عند وسط[8] الرجعة يصير مقابلاً له وبالعكس فيحصل له في وصوله من وسط الاستقامة إلى وسط الرجعة جميع الأبعاد من الشمس بخلاف أصل التدوير فإنّ وسطي الاستقامة والرجعة منه إنّما يكونان[9] عند الذروة والحضيض وموضعاهما على الفلك واحد .

فالسفليان على أصل التدوير يقارنان وسط الشمس عند وسطي الاستقامة والرجعة معاً فلا يحصل لهما في وصولهما من[10] وسط الاستقامة إلى وسط الرجعة جميع الأبعاد من وسط الشمس بل لا يبعدان عنه إلاّ بقدر ما يقتضيه نصف قطر التدوير .

وليس الأمر كما ظنّوا فإنّ حركة الوسط على أصل الخارج إنّما يتعرّض إلى التوالي بمقدار مجموع حركتي وسط[11] الشمس والاختلاف وحركة الخارج إلى



خلاف التوالي بمقدار حركة الاختلاف فبمقدار ما يبعد الخارج مركز[12] الكوكب عن وسط الشمس إلى خلاف التوالي يردّه حامل الخارج بحركته على التوالي ولا يبقى البعد[13] بين مركز الكوكب ووسط الشمس إلاّ بمقدار التعديل كما أنّه على أصل[14] التدوير لا يبعد عنه إلاّ بمقدار التعديل والتعديل في كلّ حين على الأصلين واحد فمركز السفليين على كلا الأصلين /(H:125a) لا يبعدان عن[15] وسط الشمس إلا بمقدار[16] واحد .

وليكن لبيان ذلك دائرة اب على مركز ه منطقة حاملي الخارج والتدوير معاً ودائرة جد منطقة التدوير ودائرة صع على مركز م منطقة الخارج المركز ولنفرض الكوكب في وسط الاستقامة على أوج الخارج وذروة التدوير على أصل التدوير[17] ثم ليتحرك مركز التدوير بحركة الوسط زاوية اهس ومركز الكوكب بحركة الاختلاف زاوية[18] جسك[19] ونصل ه ك ونبيّن أنّ مركز الكوكب على أصل الخارج أيضاً على نقطة ك وذلك لأنّ أوج الخارج إذا تحرّك بحركة حامل الخارج حركة مساوية لمجموع حركتي الوسط والاختلاف زاوية

اهط كان(!) زاوية سهب فضلها على الوسط مساوية لزاوية جسك الاختلافية فكان /(C:137a) خط هط موازياً لخط سك ثم إذا تحرّك مركز الكوكب على محيط الخارج بحركته المساوية لحركة الاختلاف زاوية طمق كان خط مق موازياً لخط هس وإذا وصلنا سق كان مساوياً موازياً لخط هم لتوازي[20] خطي مق سه وتساويهما بالفرض[21] وكان خط سك أيضاً مساوياً لخط هم بالفرض[22] وموازياً له فخط سك منطبق على خط سق فنقطة ق مركز الكوكب على أصل الخارج منطبق على نقطة ك مركز الكوكب على أصل التدوير فلا فرق بين الأصلين في شيء من الأحوال وذلك ما أردنا بيانه[23] .

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[1] القشجي] القوشجي : H (بدون نقط) = +رحمه له تع : C = +عليه الرحمه : H. [2] بسم الله الرحمن الرحيم وبه ثقتي] بسم (؟) الله سبحانه : L. [3] الثلاثة] الثلثة : C, H. [4] وهما] هما : L. [5] بقدر] +~~يسير~~ : C. [6] إنما] نما : C_{un}. [7] الكوكب] الكواكب : H. [8] وسط] C_{mr}. [9] يكونان] يكون : H (الياء بدون نقط). [10] من] H_{un}. [11] وسط] وسطى : L. [12] مركز] +فلكى(؟) : L. [13] البعد] +~~عن ذلك~~ : C. [14] أصل] +~~الحارح~~ : H. [15] عن] +(كلمة مشطوبة غير مقروءة) : L. [16] بمقدار] +(عدة كلمات مشطوبة غير مقروءة) : L. [17] التدوير] الحارح : H = والتدوير (الياء بدون نقط) : H_{ab}. [18] اهس ومركز الكوكب بحركة الاختلاف زاوية] C_{mr}. [19] جسك] د(؟) س ~~ك~~ ك : C = د(؟) س ك : L. [20] لتوازي] ليوازى : C. [21] بالفرض] بالعرض : C, H, L. [22] بالفرض] بالعرض : C, H, L. [23] أردنا بيانه] اردناه والحمد لله والصلوه على محمد واله احمعين : H (بدون نقط). (في ورقة 132a من مخطوطة H نجد الشكل الموجود في النصّ مكرّر مع الجملة التالية : «هذا الشكل في رسالة كتبها المولى علي القوشجي لبيان أنّ أصل الخارج يمكن في السفليين كما في غيرهما» (بدون نقط).

Acknowledgments

I would like to thank İhsan Fazlıoğlu, David King, Sally Ragep, and Noel Swerdlow for their suggestions and encouragement. My gratitude extends as well to the curators and librarians who allowed access to their collections: Nevzat Kaya (Istanbul), Ömer Kurmuş (Bursa), and Marilyn Ogilvie and Kerry Magruder (Oklahoma). Research for this article was undertaken while I was a fellow at the Max Planck Institute for the History of Science (Berlin); its director, L. J. Daston, provided just the right environment for this article to coalesce. All remaining deficiencies are, of course, my own.

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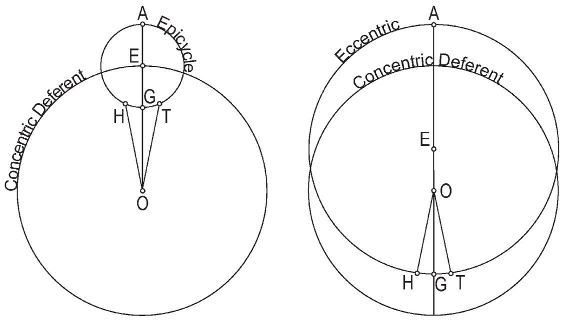
1. N. M. Swerdlow, "The derivation and first draft of Copernicus's planetary theory: A translation of the Commentariolus with commentary", *Proceedings of the American Philosophical Society*, cxvii (1973), 423–512. Good summaries can be found in N. M. Swerdlow and O. Neugebauer, *Mathematical astronomy in Copernicus's De revolutionibus* (2 parts, New York and Berlin, 1984), 54–64, and in M. H. Shank, "Regiomontanus on Ptolemy, physical orbs, and astronomical fictionalism", *Perspectives on science*, x (2002), 179–207, pp. 184–5.
2. Recently B. R. Goldstein ("Copernicus and the origin of his heliocentric system", *Journal for the history of astronomy*, xxxiii (2002), 219–35) has argued that Copernicus's *motivation* for the heliocentric system arose from his insistence upon the distance–period relationship for the planets and a rejection of Ptolemy's nesting hypothesis, since the latter violated the former for the Sun, Venus, and Mercury. If true, this would make Swerdlow's reconstruction problematic since it depends upon Copernicus's reaching his decision in favour of heliocentrism only after rejecting a proto-Tychonic model that resulted in an intersection of orbs (specifically those of Mars and the Sun), something not allowed in most of ancient and medieval cosmology. For Swerdlow, Copernicus's initial motivation for exploring alternative models had to do with Ptolemy's violations of uniform, circular motion, thus placing him firmly within the tradition of Islamic theoretical astronomy (*hay'a*). Goldstein's position, though plausible, has little, if any, textual support from the *Commentariolus*, depending instead upon the much later *De revolutionibus*. A straightforward reading of the introduction to the *Commentariolus*, as well as consideration of the considerable space Copernicus devotes in that work to his alternative *mathematical* models, would seem to indicate that the "equant problem" was foremost in his mind around 1510. But whatever the motivation, Goldstein agrees that the mathematical transformation from a geocentric to heliocentric cosmology would still rely, as argued by Swerdlow, upon the propositions found in Book XII of Regiomontanus's *Epitome*, which is the subject of the current article.
3. G. J. Toomer, *Ptolemy's Almagest* (New York and Berlin, 1984), 555 (n. 2).
4. Though completed in 1463, the *Epitome* was first printed in 1496 in Venice, after Regiomontanus's death. For details, see Swerdlow and Neugebauer, *op. cit.* (ref. 1), 51. Swerdlow has translated Book XII.1–2 in his *op. cit.* (ref. 1), 472–5.
5. For Copernicus's references to his sources, or lack thereof, see Swerdlow, *op. cit.* (ref. 1), 437.
6. Swerdlow, *op. cit.* (ref. 1), 471.
7. Shank, *op. cit.* (ref. 1), 185.
8. On Bessarion's role in encouraging the writing of the *Epitome*, see Swerdlow and Neugebauer, *op. cit.* (ref. 1), 50–51. It is worth noting that Bessarion was originally from the Black Sea town of Trebizond, which fell to the Ottomans in 1461.
9. Whence the name Qushjī: kuş (Ottoman: qūsh) is the Turkish word for falcon or hawk; kuşçu (Ottoman: Qūshjī) is falconer.
10. For accounts of the Samarqand school and observatory, see: A. Sayılı, *The observatory in Islam* (Ankara, 1960), 260–89; E. S. Kennedy, "The heritage of Ulugh Beg", in *idem*, *Astronomy and astrology in the medieval Islamic world* (Aldershot and Brookfield, VT, 1998), XI; İ. Fazlıoğlu,

"Osmanlı felsefe-biliminin arkaplanı: Semerkand matematik-astronomi okulu", *Dîvân ilmî araştırmalar*, xiv/1 (2003), 1–66; and G. Saliba, "Reform of Ptolemaic astronomy at the court of Ulugh Beg", in *Studies in the history of the exact sciences in honour of David Pingree*, ed. by C. Burnett *et al.* (Leiden, 2004), 810–24.

11. For an account of Qushjī's life, see İ. Fazlıoğlu, "Ali Kuşçu", in *Yaşamları ve yapıtlarıyla Osmanlılar ansiklopedisi*, ed. by E. Çakıroğlu (Istanbul, 1999), i, 216–19 and *idem*, "Qūshjī", in *Biographical encyclopaedia of astronomers*, ed. by T. Hockey (Springer/Kluwer, forthcoming).
12. F. J. Ragep, "Ḳāḍī-zāde Rūmī", *The encyclopaedia of Islam* (Leiden, 2004), xii, 502.
13. E. S. Kennedy, "A letter of Jamshīd al-Kāshī to his father: Scientific research and personalities at a fifteenth century court", *Orientalia*, xxix (1960), 191–213; reprinted in E. S. Kennedy *et al.*, *Studies in the Islamic exact sciences* (Beirut, 1983), 722–44. *Cf.* M. Bagheri, "A newly found letter of Al-Kāshī on scientific life in Samarkand", *Historia mathematica*, xxiv (1997), 241–56.
14. E. S. Kennedy, "Ulugh Beg as scientist", in *idem*, *Astronomy and astrology in the medieval Islamic world* (ref. 10), X.
15. F. J. Ragep, "Freeing astronomy from philosophy: An aspect of Islamic influence on science", *Osiris*, xvi (2001), 49–71 (espec. 61–63).
16. *Ibid.* and F. J. Ragep, "Ṭūsī and Copernicus: The Earth's motion in context", *Science in context*, xiv (2001), 145–63 (espec. 156–7).
17. This work has been edited and translated by G. Saliba, "Al-Qushjī's reform of the Ptolemaic model for Mercury", *Arabic sciences and philosophy: A historical journal*, iii (1993), 161–203.
18. For an overview, see G. Saliba, "Arabic planetary theories after the eleventh century AD", in *Encyclopedia of the history of Arabic science*, ed. by R. Rashed (3 vols, London and New York, 1996), i, 58–127.
19. *Cf.* Toomer, *op. cit.* (ref. 3), ix.5, 442 and n. 38. For an informed discussion of this passage, see O. Neugebauer, *A history of ancient mathematical astronomy* (3 vols, New York, 1975), i, 149–50.
20. Saliba, *op. cit.* (ref. 17), 172 (English translation), 194 (Arabic text); I have slightly modified Saliba's translation.
21. Saliba, *op. cit.* (ref. 17), 166.
22. Fazlıoğlu, *op. cit.* (ref. 11).
23. Swerdlow, *op. cit.* (ref. 1), 472.
24. *Ibid.*, 475–6 (n. 8).
25. In Ragep, *op. cit.* (ref. 16), I present evidence indicating a possible connection between Copernicus and his Islamic predecessors (including Qushjī) regarding the question of the Earth's rotation.
26. A possible transmission through Italy has been advanced (Swerdlow and Neugebauer, *op. cit.* (ref. 1), 47–48, 55). But the role of Bessarion in transmitting materials to Peurbach and Regiomontanus cannot be discounted.
27. For an elaboration of this point, see F. J. Ragep, "Copernicus and his Islamic predecessors: Some historical remarks", *Filozofski vestnik*, xxv (2004), 125–42.
28. Needless to say, the similarity in orientation of the two diagrams is striking. If one accepts that this is a case of borrowing, one might speculate that the additional epicycle and eccentric in the initial positions in Qushjī's diagram have been removed for visual simplification. One caveat: the Latin lettering does not correspond to the Arabic.
29. Hypothesis translates *aṣl*, which in turn was used to translate the Greek ὑπόθεσις. Both in Greek and in Arabic, the meaning is 'basis', especially that upon which something else is constructed. There is no implication of the modern sense of hypothesis, i.e. a tentative theory that needs to be verified. *Cf.* Toomer, *op. cit.* (ref. 3), 23–24.
30. See xii.1 of the *Almagest* (Toomer, *op. cit.* (ref. 3), 555).
31. The reference is to *al-Tuḥfa al-Shāhiyya* (The royal gift) by Quṭb al-Dīn al-Shīrāzī (1236–1311). The passage in question occurs in bk. ii, ch. 8. The work as a whole has not been edited or printed, but

this chapter has been edited and translated by Robert Morrison and will appear in a forthcoming issue of the *Journal for the history of Arabic science*.

32. Qushjī evidently has the following diagram in mind, which is an adaptation of a diagram that one may find, among other places, in *al-Tadhkira fī ᶜilm al-hay'a* by Naṣīr al-Dīn al-Ṭūsī (1201–74). (See F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's memoir on astronomy* (2 vols, New York, 1993), i, 138–9.) In the *Almagest* (xii.1), Ptolemy had used a single diagram for both the epicyclic and the eccentric models, but Ṭūsī has split them into two separate representations.



A: apogee; *E*: centre of epicycle/eccentric; *G*: epicyclic perigee/perigee; *HT*: retrograde arc; *O*: centre of world.

Qushjī speculates that the reason Ptolemy had denied the possibility of an eccentric for the lower planets was because he thought that, like the upper planets, they would thereupon undergo retrograde motion at opposition to the Sun, which is contrary to observation. Looking at the above diagram, one can see that, indeed, at first glance one might think that the epicyclic model would allow for both the middle of direct motion and retrogradation to occur at conjunction whereas the eccentric model would require the two to occur 180° apart. This would be the case if one were to assign the same motions to the eccentric models of both the upper and lower planets, namely the concentric deferent moving (west to east) with the motion of the mean Sun while the eccentric moves (east to west) with the motion of the epicyclic anomaly. But as Qushjī shows below, one can adjust the motions appropriately so that an eccentric model will work for the lower planets.

33. For the eccentric model of the upper planets, the mean motion (or motion of centre) is sequential, i.e. in the order of the zodiacal signs, and equal to the Sun's mean motion, while the eccentric motion is counter-sequential and equal to the motion of anomaly (see O. Pedersen, *A survey of the Almagest* (Odense, 1974), 339–40). For the eccentric model of the lower planets that Qushjī describes here, the mean motion (or motion of centre) must be sequential and equal to the sum of the Sun's mean motion and the motion of anomaly; as with the upper planets, the eccentric here moves counter-sequentially and its motion is equal to the motion of anomaly. (See below and figure in text.)

34. Referring to the text figure, the equation in the epicyclic model is $\angle KES$; in the eccentric model, $\angle QES$.

35. Equator translates "minṭaqa", which can be used for both an equator on the surface of a sphere and also a parallel "inner" equator, as it is here; see Ragep, *op. cit.* (ref. 32), ii, 414, 437–8.

36. One would normally expect "motion of the centre", i.e. of the centre of the epicycle. Perhaps Qushjī felt that since he was dealing with both models simultaneously, "mean motion" was more appropriate.

37. This is so since $\angle SET = \angle QMT = \angle GSK =$ motion of anomaly.

38. The accompanying diagram occurs in all three manuscripts with essentially the same lettering and orientation. (MS L, however, is missing line *EK*.) It is also reproduced a second time in MS H, f. 132a with the following header: "This figure is in the treatise written by master ᶜAlī Qūshjī to prove that the eccentric hypothesis is possible for the lower planets as it is for the others."

Ṭūsī and Copernicus: The Earth's Motion in Context*

F. Jamil Ragep

University of Oklahoma

Argument

A passage in Copernicus's *De Revolutionibus* regarding the rotation of the Earth provides evidence that he was aware, whether directly or indirectly, of an Islamic tradition dealing with this problem that goes back to Naṣīr al-Dīn al-Ṭūsī (1201–1274). The most striking similarity is the use of comets by both astronomers to discredit Ptolemy's "proofs" in the *Almagest* that depended upon observational evidence. The manner in which this question was dealt with by Copernicus, as an astronomical rather than natural philosophical matter, also argues for his being within the tradition of late medieval Islamic astronomy, more so than that of medieval Latin scholasticism. This of course is bolstered by his use of non-Ptolemaic models, such as the Ṭūsī couple, that have a long history in Islam but virtually none in medieval Europe. Finally, al-Qūshjī, who was in Istanbul just before Copernicus was born, entertained the possibility of the Earth's rotation; this also opens up the possibility of non-textual transmission.

1. Introduction

In recent years, there has been considerable discussion regarding the possible influence of late medieval Islamic astronomy[1] on the work of Copernicus and other Renaissance astronomers. For the most part, this influence has been presumed to be limited to mathematical models and, perhaps, to criticism of Greek astronomy that had led to these models. The story can be recapitulated as follows: Islamic astronomers, beginning with Ibn al-Haytham in the eleventh century, faulted a number of Ptolemy's models on the grounds that they produced irregular motion, which was a violation of the ancient principle that all celestial motion must be uniform and circular. Beginning in the thirteenth century, this led to the proposal of alternative models by several Islamic astronomers, dubbed collectively the Marāgha School, whose purpose was to replace certain suspect Ptolemaic models and devices (such as the equant) using various combinations of uniformly rotating orbs. Copernicus, somehow aware of this late tradition of non-Ptolemaic astronomy, began

* I wish to thank Steven Livesey, Sally Ragep, A. I. Sabra, and Julio Samsó for helpful suggestions. It should be noted that several of the texts discussed in this article are only available in manuscript and have yet to be edited or translated. This will, I hope, be rectified in a forthcoming publication.

[1] "Late" medieval astronomy refers here to the period beginning in the early thirteenth century. This is after the main translation movement from Arabic into Latin had occurred.

his work to reform astronomy under its influence. Eventually, in dealing with aspects of planetary motion for which he had no Islamic precedent (in particular the motion of certain epicycles that produce "the second anomaly"), he was led to transform his system from a geocentric to a heliocentric one.[2]

Since medieval Islamic astronomy remained geocentric, it has been assumed that the reasons for Copernicus's decision to embrace a heliocentric cosmology and put the Earth in motion are to be found within a European context. There is, however, some tantalizing evidence that links Copernicus's discussion of the Earth's motion with a long and increasingly sophisticated discussion of the Earth's possible rotation that occurred amongst a number of Islamic astronomers and philosopher/theologians. In what follows, I will discuss the evidence linking Copernicus to this Islamic tradition, indicate some of the main issues that animated this debate in Islam, and speculate about the possible implications of this debate for Copernican astronomy.

2. The Evidence

In chapter 8 of Book I of *De Revolutionibus*, Copernicus attempts to refute some of the classical arguments for a stationary Earth at the center of the Universe. After discussing a number of problems with attributing the daily motion to the celestial region as a whole, he then moves on to justify the main alternative, namely the Earth's daily rotation. For this purpose he quotes a verse from the *Aeneid* meant to show that from a ship on a calm sea one could not tell whether the ship or the land was in motion, thus concluding with the widely dispersed idea that the "impression that the entire universe is rotating" could be produced by the Earth's rotation. The problem, of course, was how to explain, given the rotation of the Earth, the corresponding – and empirically necessary – rotation of bodies that were near to but detached from the Earth. This was certainly one of the thorniest issues related to the assumption of the Earth's rotation, and Ptolemy had drawn particular attention to the absurd (and non-observed) consequences that he assumed would occur to objects in the air if the Earth were rotating. Copernicus's answer is as follows:

> Then what should we say about the clouds and other things suspended in the air in whatever way or things that fall down or conversely things that rise up to the upper regions? [We would say] that not only the earth with the watery element conjoined with it moves in this way, but also not a small part of the air and whatever in the same way has a natural connection (*cognatio*) to the earth. Either the nearby air, mixed with the matter of earth or water, should conform to (*sequatur*) the same nature as the earth, or the motion of the air, which has been acquired by the contiguity of the earth,

[2] Admittedly this is a grossly simplified version of a fuller and much more careful exposition that one may find in Swerdlow and Neugebauer 1984, esp. part 1, 41–64. Although there are parts of this story that need revision, I will leave that for another occasion.

> participates in a perpetual rotation without resistance. On the other hand, it is no less remarkable that the upper region of the air conforms to (*sequi*) the motion of the heavens, which is indicated by those suddenly-appearing stars that the Greeks call "comets" and "bearded stars". It is maintained that they are generated in that place and, furthermore, like the other stars, they rise and set. We can say that that part of the air is unaffected by the terrestrial motion on account of its great distance from the earth. The air closest to the earth, and the things suspended in it, will appear still unless they are moved about by the wind or some other impulse (*impetus*). For what else is the wind in the air but a wave in the sea? (Copernicus 1543, 6a, lines 16–34)[3]

Let us compare this with a passage from Naṣīr al-Dīn al-Ṭūsī's *Tadhkira*, a work on theoretical astronomy (*hayʾa*), whose first edition was completed in 1261. This passage occurs in Book II, chapter 1, which is concerned with establishing the general cosmology of the celestial realm according to Ptolemaic principles. As such Ṭūsī wishes to prove that the Earth is at rest. But unlike the other "proofs" in this chapter that for the most part follow Ptolemy's recourse to observational evidence for establishing such things as the sphericity of the heavens and Earth, Ṭūsī here rejects Ptolemy's empirical approach in a manner strikingly similar to that taken by Copernicus:

> It is not possible to attribute the primary motion to the Earth. This is not, however, because of what has been maintained, namely that this would cause an object thrown up in the air not to fall to its original position but instead it would necessarily fall to the west of it, or that this would cause the motion of whatever leaves the [Earth], such as an arrow or a bird, in the direction of the [Earth's] motion to be slower, while in the direction opposite to it to be faster. For the part of the air adjacent to the [Earth] could conceivably conform (*yushāyiʿu*) to the Earth's motion along with whatever is joined to it, just as the aether [(here) = upper level of air] conforms (*yushāyiʿu*) to the orb as evidenced by the comets, which move with its motion. Rather, it is on account of the [Earth] having a principle of rectilinear inclination that it is precluded from moving naturally with a circular motion. (Ragep 1993, vol. 1, 106–107)

What originally struck me about these two passages was the use of comets by both Ṭūsī and Copernicus to bolster their case for the view that the Earth might be moving but we would not be able to tell this simply by observing objects that occurred or were thrown in the air. But in examining the texts more closely, I became aware of other similarities, such as the use of the concept of "following" or "conforming" used by both men to describe what occurs in the lower as well as the

[3] I owe this fairly literal translation to my colleague Steven Livesey, who also helped me gain a deeper understanding of the passage. He is, of course, absolved of any shortcomings and peculiarities in the interpretation.

upper atmosphere (*sequi* in Latin and *yushāyiʿu* in Arabic).[4] I also saw that the structure of the argument itself – making the case for objects in the lower air conforming to the Earth's motion and then bringing forth comets to somehow clinch the matter – was similar in both cases. Though highly suggestive, these passages alone are not decisive in proving influence or transmission. For one thing, it has been known for some time that similar discussions concerning the possibility of the Earth's motion exist in the medieval European scholastic tradition. Here an understanding of the intellectual contexts in each case can not only help elucidate similarities and differences of corresponding passages but also help answer questions regarding influence and transmission.

In what follows, I will attempt to deal with some of these contexts by examining three issues: (1) the use of comets to bolster the case for the Earth's rotation; (2) the problem of observational tests; and (3) the debate over the use of natural philosophical premises in mathematical astronomy.

3. The Use of Comets to Bolster the Case for the Earth's Rotation

As mentioned above, one of the most striking similarities between the passages by Ṭūsī and Copernicus is the appeal to comets. Both use them to provide an analogous case that would make plausible the notion that the air, and whatever is in it, might participate in the Earth's rotation. To follow this argument, one must first understand the underlying Aristotelian doctrine regarding comets. According to Aristotle, comets are a sublunar phenomenon and, as such, one might thereby assume that they would not participate in the daily rotation of the Universe. But Aristotle maintained in the *Meteorology* that the "outermost part of the terrestrial world which falls below the circular motion. . . and a great part of the air that is continuous with it below is carried round the earth by the motion of the circular revolution." Aristotle then proceeded to relate this to the production of comets, which he, and most medieval writers, took to occur in the upper atmosphere. Indeed it is presumably comets that led him to conclude that the upper atmosphere was somehow a party to the daily motion (Aristotle 1984, *Meteorology* I.vii, esp. 344a5–23). From the point of view of Ṭūsī and Copernicus, the fact that Aristotle could argue that the upper part of the atmosphere could participate in – or "conform" to – the daily motion of the orbs provided a physical justification for the idea that the lower atmosphere – that is, the air – could follow the motion of a rotating Earth if the orbs were not the source of the daily motion. It is worth noting here that Aristotle's theory of comets was well-known, and widely accepted, in both medieval Islam and Christendom. This is nicely illustrated by a passage from Albertus Magnus's (ca. 1193–1280) commentary on Aristotle's *Meteorology* in which he cites both Avicenna (Ibn Sīnā: 980–1037) and

[4] E. Rosen in his translation (1978, 16) uses "conform" in the first instance, "accompany" in the other.

Algazal (al-Ghazālī: 1058–1111) as purportedly supporting the Aristotelian view of comets, which is also Albert's belief.[5]

The question that then arises is whether anyone before Ṭūsī in the Islamic tradition, or before Copernicus in the medieval Latin tradition, had used the Aristotelian theory of comets to bolster the case (but not necessarily argue) for the Earth's possible rotation. Though I have thus far been unable to find anyone who did so, there is a rather similar argument (minus the comets) in *Le Livre du ciel et du monde* by Nicole Oresme (ca. 1325–1382). As he puts it, "I should like to present an example taken from nature, which, according to Aristotle, is true." As does Ṭūsī and Copernicus, Oresme uses the alleged circular motion of the fiery upper atmosphere as part of his evidence for the Earth's possible rotation (Oresme 1968, 524–527; trans. repr. Grant 1974, 505–506).

The evidence from Oresme shows that the essential components of the argument we find in Copernicus were already present and had been put together in fourteenth-century Europe. It might then seem that this is all that is needed to make the case that Oresme, not Ṭūsī, was the immediate source for Copernicus. (This argument would also work even if we were to claim that Oresme were somehow influenced by Ṭūsī's argument.) But as Grant has noted, there is "no evidence that Copernicus … derived his arguments from medieval sources" (1994, 648). But, of course, neither is there documented evidence that he derived them from Ṭūsī. However, given the strong evidence of Copernicus's use of Ṭūsī's astronomical devices, and also the appeal to comets by both (which is absent in Oresme), one could conceivably claim that Copernicus was somehow more influenced by his Islamic rather than his European predecessor, despite the lack of linguistic and cultural affinities.

This possibility is further strengthened by the evidence of a continuing and long-lived discussion in the Islamic world of the relevance of comets for determining the Earth's possible rotation, and the seeming lack of such evidence in Europe prior to Copernicus.[6] For example, in his *al-Tuḥfa al-shāhiyya fī al-hayʾa,* Quṭb al-Dīn al-Shīrāzī (1236–1311) disputed his onetime master Naṣīr al-Dīn al-Ṭūsī, not to mention Aristotle, and stated that if comets did indeed move "by conformity" (*bi-'l-mushāyaʿa*) with the daily motion of the moon's orb, "then they would remain parallel to the celestial equator; however, they move from north to south, which is due to a soul connected to them that moves them sometimes parallel and sometimes not in parallel [to the equator]" (bāb II, faṣl 4: Mosul MS, f. 17a = London MS, f. 10b). This dispute was taken up in a number of commentaries on Ṭūsī's *Tadhkira*, including one

[5] For the English translation, see Thorndike 1950, 68; repr. Grant 1974, 544. The source of Albert's reference to Ibn Sīnā's view may be from the latter's book on meteorology in the *Shifāʾ* (1965, 73–74), where a similar but not exact passage may be found. Albert's more extensive quotation is from al-Ghazālī's *Logica et philosophia* (1506, Liber II, Trac. III, Spec. iiii; for the original Arabic see idem 1960–1, 343).

[6] I make no claim to having gone through all the possible European medieval sources. But I find no references to any such discussion in Thorndike 1923–1958, Hellman 1944, Thorndike 1950, Jervis 1985, or Grant 1994. If such a discussion existed, it also escaped the keen gaze of Pierre Duhem.

of the most famous and widely read, that of al-Sayyid al-Sharīf al-Jurjānī (1339–1413), who was sympathetic to Shīrāzī's position on comets but who also noted its irrelevance when judging whether or not the air might be in conformity with the Earth's motion (Jurjānī, f. 20a-b).

This issue was also discussed by ʿAlī al-Qūshjī (d. 1474), who played a prominent intellectual role in the court of the Timurid Prince Ulugh Beg in Samarqand and was later invited to establish a school devoted to the sciences by the Ottoman Sultan Mehmet the Conqueror in the newly Islamized city of Constantinople. Writing in his commentary on Ṭūsī's theological work, *Tajrīd al-ʿaqāʾid*, Qūshjī disputed Shīrāzī's dismissal of the daily motion of the comets in conformity with the orbs by citing the comet of 837 hijra (= 1433 A.D.), which he claimed to have personally observed. As he says:

> From what we have witnessed, there is clear proof that the sphere of fire (*kurat al-athīr*) moves with the daily motion. But it is said [*viz.* by Shīrāzī] that if this were the case then the motion of comets would be parallel to the celestial equator; however, this is not so since sometimes they [move] north from the equator, sometimes south from it. There is, though, nothing to this [objection by Shīrāzī]. For according to what we have witnessed, they do indeed move thusly with their proper motion. But all the planets move this way – they move with the daily motion while they have their own proper motions, which may sometimes be to the north of the equator, sometimes south. (Qūshjī 1890, 194)[7]

This dispute regarding the relevance of the cometary evidence continues into the sixteenth century. ʿAbd al-ʿAlī al-Bīrjandī, who died in 1525 or 1526 (and thus was a contemporary of Copernicus), was yet another commentator on the *Tadhkira* who brought up this controversy.[8] He noted that the question of the conformity of air to the Earth's motion would not depend on whether or not the comets moved with the orb since this was only brought up by Ṭūsī as a supporting argument whose resolution would not be decisive one way or another (Bīrjandī, f. 37b).

The point that needs to be stressed here is that this question regarding comets and their relevance for the problem of the Earth's rotation was hotly debated for a number of centuries in the Islamic world. Though Oresme's argumentation is clearly similar, he does not use comets directly. As far as I have been able to tell, Copernicus was the first person in Europe to discuss this matter in a way that so closely follows (or parallels) the Islamic tradition. Here again, understanding the context (and tradition) of the debate within each cultural context is important, I believe, for indicating the

[7] Though he does not mention Shīrāzī by name, the quotation is taken directly from the *Tuḥfa* and would have been readily recognized by many of Qūshjī's readers. The comet of 1433 was described by the Italian Paolo Toscanelli (Jervis 1985, 56–58).

[8] It is worth noting that though Bīrjandī was Iranian, he, along with a number of other Sunnī intellectuals, fled the new Shīʿī regime of Shah Ismāʿīl and went to areas controlled by the Ottomans. Bīrjandī himself went to the Ottoman cities of Trebizond and later Istanbul, where contact with European scholars, either directly or indirectly, would have presumably been easier (Ihsanoğlu et al. 1997, vol. 1, 101).

most likely lines of transmission and influence. In the next two sections, we turn to other aspects of the intellectual and historical contexts that underlie the arguments of Ṭūsī and Copernicus.

4. The Problem of Observational Tests

One of the critical issues arising from the Earth's possible rotation concerned observational tests. Put simply, were there observations that could determine whether the Earth were at rest or in motion? In the *Almagest*, Ptolemy implicitly assumed that such observations were possible. There are several aspects to his argument, which we summarize as follows (Toomer 1984, I.7, 44–45):

1) Because of the speed that one would need to assume for a rotating Earth, objects not actually standing on the Earth would quickly be left behind and appear to move toward the west.
2) One might counter (1) by claiming that the air could be carried with the Earth in its rotation; Ptolemy answers by stating that in this case objects thrown into the air would still be left behind.
3) One might then claim that the objects were somehow "fused" in the air; Ptolemy counters that if such were the case these objects would "always appear still" which flies in the face of our experience.

Clearly both Ṭūsī and Copernicus, in the passages quoted in Section 2 above, are reacting against Ptolemy, maintaining that his cited observations are not decisive in determining whether or not the Earth is at rest. This question has a long and intricate history in Islam whose details could well shed light on European discussions.[9] An indication of the early history of this problem in Islam can be gleaned from *al-Qānūn al-masʿūdī*, completed in 1030 by the great polymath Abū ʾl-Rayḥān al-Bīrūnī (973–1048). In it, he reports that some unnamed person held that a heavy body in the air could have two motions: one circular, which results from being part of the rotating whole, and the second linear, which is a result of its natural motion downward. As a consequence of these two motions, a body thrown straight upward would stay aligned with the point from which it was thrown. The path of the body would not, contrary to what one observes, be straight up and down but rather a line curving toward the east (Bīrūnī 1954–56, vol. 1, 50–51).[10] Under such circumstances, Ptolemy's type of observational tests would not be decisive in determining whether the Earth were rotating. Bīrūnī himself disputes this view. Pointing to the great speed of the Earth that would need to be assumed (which in typical Bīrūnī fashion he proceeds to

[9] Unfortunately, as noted above, the vast majority of these texts have not been edited or translated.
[10] Though Bīrūnī names the followers of the Hindu astronomer Āryabhaṭa as holding that the Earth is in motion in both his *India* (1887, 139; 1888, 276) and *Qānūn* (1954–56, vol. 1, 49), the "unnamed person" is probably not of Indian origin since he is said to be a distinguished scholar of "*ʿilm al-hayʾa*", which no doubt indicates an Islamic personage. Cf. S. Pines (1956), who came to the same conclusion.

calculate), he claims that an object such as an arrow shot with a violent (i.e. forced) motion eastward would have its motion combined with that of the air traveling with the great speed of the Earth while one shot westward would resist it; thus one should be able to tell the difference if the Earth's motion existed (Bīrūnī 1954–56, vol. 1, 51–53).[11]

After Ṭūsī, the question was taken up with renewed vigor, in large part because of the above passage from the *Tadhkira*. Once again his student, Quṭb al-Dīn al-Shīrāzī, took a contrary stance, in this case claiming that if the air conformed to the motion of the Earth, then a large and a small rock thrown, say, along the meridian should return to Earth at different locations since the air would move the larger less than the smaller. In general he seems to have agreed with Ptolemy that observation could determine the question of the Earth's rotation.[12] (We will return to the significance of this stance below.)

Shīrāzī's position, however, was itself soon under attack. Niẓām al-Dīn al-Nīsābūrī (writing in 1311) and the noted theologian/scientist al-Sharīf al-Jurjānī (in 1409) both criticized Shīrāzī on the matter of the two rocks; they held that they would in fact have the same quantity of motion as that of the rotating Earth. Hence they upheld Ṭūsī's view that this conceivable motion of the Earth could not be decided on empirical grounds.[13]

This discourse became increasingly sophisticated as various writers attempted to understand the implications of a rotating Earth and to analyze such ideas as the "conformity" of the air, and things in the air, with a rotation of the Earth. Al-Qūshjī, for example, attempted to counter Shīrāzī by claiming that "what is intended by conformity of the air is its conformity [with a rotating Earth] along with all that is in it whether it be a rock or something else, whether small or large" (Qūshjī 1890, 195). Earlier, Jurjānī had dealt with the notion of "conformity" by invoking the important distinction between accidental and forced motion. "There would be no difference between the moving [by the air] of the two rocks by an accidental motion since it would be in the amount of the [air's] proper motion whether the accidentally moved thing were small or big. Any difference between them would only be in the forced motion" (Jurjānī, f. 20b).[14] Bīrjandī elucidated this further by stating that one may argue against Shīrāzī as follows: "the small or large rock will fall to the Earth along the

[11] In order to make sense of the argument, one should change the text on page 52, line 9 from *mi'at alf* (one hundred thousand) to the variant *bi-thalāthat ālāf* (three thousand).

[12] Shīrāzī's discussion can be found in maqāla II, bāb 1, faṣl 4 (ff. 46a–47b) of his *Nihāyat al-idrāk fī dirāyat al-aflāk*, which was completed in 1281, and in bāb II, faṣl 4 (Mosul MS, ff. 15a–18a = London MS, ff. 9b–11a) of his *al-Tuḥfa al-shāhiyya fī al-hay'a*, which appeared in 1284. This section of the *Nihāya* was translated into German by E. Wiedemann 1912.

[13] In both cases, the discussion occurs in the context of their commentaries on the above-cited passage from Ṭūsī's *Tadhkira*, i.e. Bk. II, Ch. 1, Para. 6. Cf. Ragep 1993, vol. 2, 384; for information on these commentaries, see ibid., vol. 1, 60, 62. Most of the other commentators on this passage also sided with Ṭūsī.

[14] This passage is also quoted by Bīrjandī, f. 37a.

path of a line that is perpendicular to the plane (*saṭḥ*) of the horizon; this is witnessed by experience (*tajriba*). And this perpendicular is away from the tangent point of the Earth's sphere and the plane of the perceived (*ḥissī*) horizon. This point moves with the motion of the Earth and thus there will be no difference in place of fall of the two rocks" (Bīrjandī, f. 37a). Thus Bīrjandī makes the case with a concept very close to what would later be called circular inertia.[15]

This question of observational tests is of central importance in the work of two fourteenth-century Frenchmen, namely Jean Buridan and Nicole Oresme, the latter of whom has been mentioned previously. In his *Quaestiones* on Aristotle's *De Caelo*, Buridan (ca. 1300–1358), philosopher and sometime rector of the University of Paris, maintained that there is an observation that negates the possibility of the Earth's rotation. This would be an arrow shot straight upward which should, if the Earth were rotating, not return to the same point from which it was projected since "the violent impetus of the arrow in ascending would resist the lateral motion of the air so that it would not be moved as much as the air." This would then counter the supporters of the Earth's rotation who had claimed that "the air, moved with the Earth, carries the arrow, although the arrow appears to us to be moved simply in a straight line motion because it is being carried along with us."[16] As can be seen, both Buridan's position and that of his antagonist is quite close to what we have seen above in Bīrūnī's *Qānūn*. Like Bīrūnī, Buridan holds that an observation can settle the matter.

A very different perspective is presented by Oresme.[17] He holds that no observation can be decisive since given the scenario outlined by Buridan, in which an arrow or stone is thrown up into the air, such an object would participate in the Earth's hypothetical rotation; thus just as various motions inside a ship would "seem exactly the same as those when the ship is at rest," so one could not tell from the action of the arrow whether or not the Earth were rotating (Oresme 1968, 524–525; trans. repr. Grant 1974, 505). Oresme thus holds a view that is virtually identical with that of Ṭūsī and many (but not all) of Ṭūsī's Islamic successors as well as Copernicus. But Oresme argues a further position that puts him at considerable odds with Ṭūsī, namely that "no argument is conclusive,"[18] that is, that neither observations nor rational arguments from natural philosophy nor even theological arguments could conclusively show that the Earth was – or was not – moving. On the other hand, Ṭūsī and many of his followers were willing to accept the proofs of natural philosophy on this matter, which observation and mathematical astronomy, they maintained, could

[15] This is not the place to compare Bīrjandī's view with those of early modern European scientists such as Galileo, but one hopes such a comparison will not be dismissed out of hand.
[16] Translation due to M. Clagett 1959, 596; repr. Grant 1974, 502.
[17] As my colleague Steve Livesey pointed out to me, one should keep in mind that Oresme was professionally a theologian whereas Buridan was a philosopher mainly concerned with the works of Aristotle. We will return to the possible significance of this difference below.
[18] Oresme 1968, 520–521; trans. repr. Grant 1974, 504. Since this is the main point Oresme is making, it is repeated several times throughout the passage.

not decide. Arguing as a theologian, Oresme could afford to be sceptical; as astronomers, Ṭūsī and his successors needed, indeed demanded, some conclusive proof concerning a matter of such basic importance to astronomy. But this became an issue of considerable controversy in Islam as we shall see in the next section.

To conclude this section, we can see that both in Islam and in medieval Europe we find a comparable range of opinion regarding the matter of the relevance of observational tests for determining the Earth's rotation. It should be noted, though, that the context is rather different; in Islam the discussions mainly occurred within the astronomical tradition of *hayʾa* whereas in Europe they are to be found within the commentary tradition on Aristotle's natural philosophy. And the extent of the discussion, both in terms of time and participants, would seem to have been much greater in Islam than in medieval Europe. Again such considerations cannot "prove" that Copernicus's arguments on the Earth's rotation were influenced by Islamic astronomy. But taken with other considerations, they are certainly suggestive.

5. The Debate over the Premises of Astronomy

In Islam, this debate over the question of the Earth's possible rotation became intricately tied to another question, namely the nature of the premises of astronomy (*hayʾa*) and their connection with natural philosophy (*al-ṭabīʿiyyāt*, i.e. physics). In many ways, this latter question was a continuation of a debate that had begun in antiquity. It was generally agreed that astronomy was both mathematical and physical, but the debate centered on the extent to which principles based upon natural philosophy (as opposed to purely mathematical techniques and observational data) were needed.[19] In the *Tadhkira*, Ṭūsī was quite explicit in maintaining that one needed, at least occasionally, the results from natural philosophy, which were based on the *a priori* methods of the natural philosophers rather than the mathematics and observations of the mathematical astronomers (Ragep 1993, vol. 1, 38–46). A good case in point was the question here under consideration: because, according to Ṭūsī, one could not determine by observation whether or not the Earth was in motion, an astronomer must have recourse to the natural philosophers, who had shown using other methods that the Earth must be at rest at the center of the Universe (Ragep 1993, vol. 1, 106–107 and vol. 2, 383–385).[20]

[19] Much of the literature on this problem has portrayed the two sides as being on the one hand mathematical and instrumentalist (interested only in "saving the phenomena") and on the other physicalist and realist (interested in the "true" nature of the universe). There have been a number of correctives to this view in recent years – especially as regards ancient authors – and it has become increasingly clear that physical considerations were of concern even to someone like Ptolemy or the writers of astronomical tables (*zīj*es). Cf. Ragep 1993, vol. 1, 24–53, where one may also find references to other discussions of this problem; for how a *zīj* writer such as al-Battānī (ca. 858–929) was influenced by physics in dealing with questions of mathematical astronomy, see Ragep 1996, 267–303.

[20] Ṭūsī does not put the matter as explicitly as stated here, but this was the universal understanding of the passage, which is quoted in section 2 above.

Though this issue became much more explicit and important for Ṭūsī and his successors, it certainly predates him. In his work on the astrolabe, written sometime before 1000 A.D., Bīrūnī implies that the question of the rotation of the Earth cannot be decided by observation and, somewhat surprisingly, declares that this is a difficult matter whose "resolution should be entrusted (*mawkūl*) to the Natural Philosophers."[21] But in his later work, *al-Qānūn al-masʿūdī*, completed in 1030, he claims, as we have seen, that there is an observational test. One way to interpret Bīrūnī's change of position is to see him as having become more closely tied to a mathematical approach to astronomical problems and less sympathetic to a philosophical encroachment upon science. Indeed, in the *Qānūn* he tells us that for these matters mathematical investigation is more appropriate than that of natural philosophy since the latter is "persuasive" (*iqnāʿī*) (Bīrūnī 1954–56, vol. 1, 49) and thus does not attain certitude.[22] One is tempted to view this change in Bīrūnī as somehow a reaction to his contemporary and long-term rival Ibn Sīnā, who at about the same time Bīrūnī was writing the *Qānūn* was completing his summary of the *Almagest*, which would become part of his monumental *Shifāʾ*. There, contrary to Bīrūnī, he seems to dispute Ptolemy's reliance on observational tests by stating that "his [i.e. Ptolemy's] amazement at their portrayal of something of this heaviness [*viz.* the Earth] having such a fast motion … is not something one should put much stock in for it would only be amazing if they had made it move by compulsion and it were not in its natural place whereby it had an inclination by nature for another motion." Ibn Sīnā ends the discussion by stating that "we have shown the impossibility of this motion in the section on Natural Philosophy" (Ibn Sīnā 1980, 25–26).[23] This gives a clear indication that he, unlike Bīrūnī, thinks the best basis for proving that the Earth does not move is through the rationalist procedures of Natural Philosophy rather than the observational tests of mathematical astronomy.

Once again, this debate is given new life and intensity by Ṭūsī's *Tadhkira*, and the main players will be familiar from the above discussion of observational tests. As we shall see, this is not coincidental. Shīrāzī once more gets the ball rolling. In a way that hearkens back to Bīrūnī, he insists, as we have seen, that observation can determine the Earth's state of rest. And lurking behind this assertion, again like Bīrūnī, is the need to establish the science of astronomy (i.e. *ʿilm al- hayʾa*) without recourse to natural philosophy. Shīrāzī calls upon the Ancients for support:

[21] This work, *Istīʿāb al-wujūh al-mumkina fī ṣanʿat al-asṭurlāb*, is unedited, but the above passage may be found in the Persian edition of Bīrūnī's *al-Tafhīm* (1367 H. Sh., 297).

[22] This difficult passage is far from clear, but the interpretation given here is reinforced elsewhere in the *Qānūn* (vol. 1, 27) where Bīrūnī tells us that Ptolemy's physical proofs for the sphericity of the heavens are persuasive (*iqnāʿī*), not necessary (*ḍarūrī*). He also chides Ptolemy for mixing natural philosophy and metaphysics with astronomy in the latter's *Planetary Hypotheses* (Bīrūnī 1954–56, vol. 2, 634–635; for a translation, see Ragep 1993, vol. 1, 40).

[23] Ibn Sīnā is most likely referring here to chapter 7 of his *De Caelo*, which forms part of the *Shifāʾ* (Ibn Sīnā 1969).

> If one asks: why did the Ancients [probably Ptolemy] disprove the Earth's motion toward the East with what you have stated and they did not disprove it by [resorting to] its having the principle of rectilinear inclination and thus is prevented by nature from moving circularly? We answer: This is: (1) either because it does not follow from a denial of a natural circular motion of the Earth that one reaches the desired end since it is possible that it might move in a circle by compulsion; or (2) because this proof is natural philosophical not mathematical and they [i.e. the Ancients] avoided using non-[mathematics] in their inquiries. For this reason, to establish the circularity of the simple [elements] they relied upon matters based upon observation and testing (*al-raṣad wa-'l-iʿtibār*) and not upon that which is bound to natural [philosophy] – for example, that a form other than a sphere would entail a dissimilarity of parts. (Shīrāzī, *Tuḥfa*, Mosul MS, f. 17b = London MS, f. 11a)

The reason for this aversion to natural philosophy is made explicit by Shīrāzī in his introduction to the *Nihāya*. There he paraphrases a famous and controversial passage from the introduction of Ptolemy's *Almagest*: "Astronomy is the noblest of the sciences… its proofs are secure – being of number and geometry – about which there can be no doubt unlike the proofs in physics and theology" (Shīrāzī, *Nihāya*, preface, f. 34b).[24]

Despite this skepticism, Shīrāzī still retained, as had Ṭūsī, sections on the natural philosophical principles needed in astronomy both in his *Nihāya* and in the *Tuḥfa*. But in the next century Qūshjī would take the bold step of declaring that astronomy does not depend upon natural philosophy and metaphysics and can dispense with them. In his commentary on Ṭūsī's theological work, the *Tajrīd al-ʿaqāʾid*, Qūshjī, responding to attacks by certain theologians who had attempted to discredit astronomy by associating it with astrology and Aristotelian natural philosophy and metaphysics, claimed:

> That which is stated in the science of astronomy (*ʿilm al-hayʾa*) does not depend upon physical (*ṭabīʿiyya*) and theological (*ilāhiyya*) premises (*muqaddamāt*). The common practice of authors to introduce their books with them is by way of following the philosophers; this, however, is not something necessary and it is indeed possible to establish [this science] without basing it upon them. For of what is stated in [this science]: (1) some things are geometrical premises which are not open to doubt; (2) others are suppositions (*muqaddamāt ḥadsiyya*) as we have stated; (3) others are premises determined by (*yaḥkumu bihā*) the mind (*al-ʿaql*) in accordance with the apprehension (*al-akhdh*) of what is most suitable and appropriate; … and (4) other premises that they state are indefinite (*ʿalā sabīl al-taraddud*), there being no final determination (*al-jazm*). Thus they say that the irregular speed in the sun's motion is either due to an eccentric or to an epicyclic hypothesis without there being a definitive decision for one or the other. (Qūshjī 1890, 187)[25]

[24] For the comparable passage in the *Almagest*, see Toomer 1984, 36.
[25] A lengthy section from Qūshjī that contains this passage is quoted by al-Tahānawī (1862, vol. 1, 48–49). For a translation of the entire passage see Ragep 2001, appendix.

It is worth noting that Qūshjī was true to his principles; in his elementary *hayʾa* work *Risāla dar ʿilm-i hayʾa*, he took the highly unusual step of dispensing with a section on natural philosophy with which almost all other similar treatises began.[26]

What this has to do with the Earth's rotation becomes clear later in his commentary when Qūshjī deals with Shīrāzī's views. He first states that "it is not established that what has a principle of rectilinear inclination is prevented from [having] a circular motion," which is in answer to the view that the Earth cannot rotate since its natural motion is rectilinear. Secondly, as we have seen above, he counters Shīrāzī by asserting that the "conformity of the air [with a rotating Earth] would be its conformity along with all that is in it, whether it be a rock or something else, whether small or large." He then ends with a startling conclusion: "Thus nothing false (*fāsid*) follows [from the assumption of a rotating Earth]" (Qūshjī 1890, 195).

Qūshjī's conclusion needs to be taken in context with his earlier discussion of the premises of astronomy. By rejecting the need for Aristotelian natural philosophy, he has made the determination of the Earth's rotation (or lack thereof) dependent upon observational evidence. But contrary to Shīrāzī, he insists that such evidence is not to be had since the possibility of the "conformity" of the air with such a rotation makes the two-rock experiment irrelevant. This leaves him in the rather surprising position of being apparently an agnostic as regards this question. His position is therefore quite close to that of Copernicus; given a more compelling physics – one based upon his four types of premises and in conformity with observational evidence – he would seem prepared to accept a rotation of the Earth. This makes him almost unique among medieval astronomers and philosophers.[27]

It should therefore not surprise us that other astronomers would find such a position intolerable. Bīrjandī for one paraphrases the above passage from Qūshjī (regarding the premises of astronomy),[28] and then gives his response:

> This is contestable. For many of the questions of this science are based upon the orbs being simple [bodies], the impossibility of [their] being penetrated and so on, which are based upon the two sciences [i.e. natural philosophy and metaphysics]. The restriction to what he has stated is unacceptable, as will become clear in the investigations of this book. (Bīrjandī, f. 7a–7b)

Exactly how this will become clear is made clear when we reach Bīrjandī's discussion of the possible rotation of the Earth. As we have seen, Bīrjandī, like Qūshjī,

[26] This work was originally in Persian and, given the evidence of the extant manuscripts, quite popular. It was translated by Qūshjī himself into Arabic and dedicated to Mehmet, the Conqueror (*Fātiḥ*) of Constantinople, whence it was called *al-Risāla al-fatḥiyya*. Cf. Haidarzadeh 1997, 24, 30–32, 41; Ihsanoğlu et al. 1997, vol. 1, 27–35; and Pingree 1996, 474.

[27] Other possibilities (as indicated above) are Bīrūnī's "unnamed astronomer" and the "followers of Āryabhaṭa".

[28] Curiously, Bīrjandī does not mention Qūshjī by name but simply refers to him as "one of the eminent scholars" (*baʿḍ al-afāḍil*).

argued against the view that observations could determine whether or not the Earth rotates. But unlike Qūshjī, he was willing to depend upon the standard Aristotelian natural philosophy to decide the issue. In reacting to Shīrāzī's explanation (cited above) of why the ancient astronomers did not use Natural Philosophy to prove the Earth's state of rest, Bīrjandī reasserts his previous position: "As mentioned above, natural philosophy is among the principles of astronomy (*hay'a*), so it is not improper to determine a question of astronomy with premises that are proven in natural philosophy" (Bīrjandī, f. 38a).

Later in this chapter, Bīrjandī again brought the issue of the use of Natural Philosophy in astronomy to the question of the Earth's rotation. He admits that, in general, Natural Philosophy strives to prove the "why" of nature (the "reasoned fact") whereas astronomy simply proves the fact of a thing. However, "the proof of the lack of rotational motion of the Earth is. . .of the 'reasoned fact' (*limmī*)" (Bīrjandī, f. 39b).[29] Whereas Shīrāzī, as well as Qūshjī, wished to avoid such a conclusion, Bīrjandī, following Ṭūsī, is willing to accept that astronomy must on occasion defer to Natural Philosophy.

We can now return to a comparison with Buridan and Oresme. Buridan notes that some astronomers hold that since either hypothesis (a stationary or a rotating Earth) can save the appearances, they "posit the method which is more pleasing to them" (Clagett 1959, 595; repr. Grant 1974, 501). Underlying this position is a certain view, which he reports but also seems to agree with, namely "that it suffices astronomers that they posit a method by which appearances are saved, whether or not it is so in actuality" (ibid.).[30] Buridan thus takes for himself the role of determining the actual nature of things, in this case whether or not the Earth moves. He therefore must do this as a natural philosopher, not as an astronomer, which in any event is evident since his discussion occurs within the context of Aristotle's *De Caelo*, a part of the Natural Philosophy corpus. This then is in marked contrast to several of the Islamic writers we have been dealing with who took it upon themselves, in an *astronomical* context and as *astronomers*, to determine by factual (*innī*) proofs mainly based on empirical evidence whether or not the Earth moved. Even astronomers such as Ṭūsī and Bīrjandī, who were willing to defer to Natural Philosophy in this one case, were nevertheless careful to delineate those matters in which the astronomer can determine the true state of affairs (by mathematics and observations) from those very few he cannot. This simply is not an issue for Buridan; by claiming that astronomers are not interested in reality, he as natural philosopher can use both observational facts

[29] For a discussion of the significance of the fact/reasoned fact dichotomy for distinguishing astronomy from natural philosophy, see Ragep 1993, vol. 1, 38–41 and vol. 2, 386–388.

[30] This extreme version of the "saving the phenomena" thesis is, in fact, something of a distortion of what the ancient Greek astronomers actually did. Someone like Ptolemy, for example, was quite obviously interested in the reality of his system as is made clear not only from his cosmological *Planetary Hypotheses* but also from his more mathematical *Almagest*. Islamic astronomers were, for the most part, also considerably interested in the physical reality of their models. Cf. Lloyd 1991 and Ragep 1990.

(the domain held closely by Islamic astronomers) as well as rational arguments (the traditional realm of the natural philosophers).[31]

A more striking comparison can be made with Oresme. Since his purpose is to show that "no argument is conclusive" in determining whether it is the Earth or the heavens that move, he obviously does not believe that astronomy can be put on an absolute foundation, whether by astronomical, physical or metaphysical arguments. One might compare this attitude with that of Qūshjī, who also allows for some uncertainty in the premises of astronomy, but the context is quite different. Qūshjī, like all the Islamic writers mentioned in this paper, was committed to the importance of astronomy not only as a way to reach truth but also as a way to glorify God.[32] Many, if not most, Islamic astronomers would agree with Shīrāzī that the mathematical science of astronomy was the most sure way to obtain knowledge of cosmological matters, i.e. God's creation. Despite Oresme's incisive and subtle argumentation, which cannot but elicit our admiration, Oresme's purpose in this passage from *Le Livre du ciel et du monde* is not to establish the foundation of astronomy. If anything, it is the exact opposite. At the end of the passage he tells us his exercise can "serve as a valuable means of refuting and checking those who would like to impugn our faith by argument" (Oresme 1968, 538–539; trans. repr. Grant 1974, 510). However one interprets this, it is clear that Oresme believes the lesson to be drawn is theological rather than astronomical or physical.[33]

The point that needs to be made here is that, despite their brilliance, Buridan and Oresme were simply not arguing within an astronomical context, at least as it was understood in Islam and as it would later be understood in Europe. The question of

[31] The use of observations to *support* propositions in natural philosophy, rather than to *prove* them (which ideally should be done using rational arguments rather than observational evidence), goes back to Aristotle himself, who in *De Caelo* states at one point that "our theory [of the unchanging aether] seems to confirm experience and to be confirmed by it" (I.3, 270b4–5). That Buridan's decisive argument is based upon observations rather than *a priori* premises perhaps indicates a continuing overlap (and confusion) of natural philosophy and astronomy (which both Bīrūnī and Qūshjī deplore), but it does not in itself make Buridan's argumentation "astronomical" any more than Ptolemy's occasional recourse to "physical" arguments makes the *Almagest* a work of natural philosophy. But one should not draw too fine a line; the main point that I wish to make here is that Buridan is arguing in the mode of a natural philosopher and using whatever arguments seem appropriate.

[32] Qūshjī 1890, 187: "Whoever contemplates the shadows on the surfaces of sundials will bear witness that this is due to something wondrous and will praise the [astronomers] with the most laudatory praise." A similar sentiment was expressed by al-Sharīf al-Jurjānī in his commentary on the *Mawāqif*, a famous theological work by al-Ījī; cf. Sabra 1994, 39–40 and Ragep 2001.

[33] E. Grant has interpreted this to mean that "by showing that it was impossible to know which alternative is *really* true, Oresme, the theologian, succeeded in using reason to confound reason" (1974, 510, note 61). E. Sylla has taken a somewhat different view and argued, rightly in my opinion, that Oresme was not seeking to "humble reason" but rather to establish that an apparently "unreasonable" tenet, whether the Earth's rotation or one of the articles of Christian faith, "may in fact be quite defensible by rational argument" (1991, 217–218). Grant has defended his interpretation against that of Sylla and insisted that "Buridan arrived at his conclusion on the basis of rational argument and the senses" whereas Oresme "decided the issue on the basis of scripture and faith" (1994, 647). But whichever interpretation is correct, it should be clear that Oresme was not writing to establish the proper premises of astronomy.

the proper foundation of astronomy would therefore not arise. On the other hand, the Islamic writers quoted above were struggling with just this question since they identified themselves so closely with the mathematical traditions they had inherited from antiquity.[34] For this reason, I believe it is easier, and more natural, to associate Copernicus's argumentation with his Islamic astronomical predecessors rather than with his European scholastic ones. Though admittedly Copernicus's arguments were not conclusive but "make it more likely that the earth moves than that it is at rest" (Rosen 1978, 17), it is noteworthy that just such an alternative was made theoretically possible by Qūshjī, who opened up the possibility of the Earth's rotation if a coherent (but not necessarily proven) alternative to Aristotelian physics could be put forth. In view of his theoretical position, Qūshjī for one might well have found Copernicus's alternative, that the whole Earth could have a circular natural motion different from the rectilinear motion of its parts (Rosen 1978, 17), compelling.[35] This simply does not seem to be an actual possibility that could be maintained by either Buridan or Oresme.

6. Conclusion

In seeking to understand the possible connection between the passages in Ṭūsī's *Tadhkira* and Copernicus's *De Revolutionibus*, it is crucial to understand the intellectual contexts in which those passages were produced. As we have seen, the Islamic discussion regarding the possible rotation of the Earth spans more than 600 years. Ṭūsī is one of a large number of astronomers, philosophers, and theologians who dealt with this issue in increasingly sophisticated terms as each generation added new insights into the problem. Thus someone like Bīrjandī, writing in the sixteenth century, could quote and react to many of the main players, including Ṭūsī, Shīrāzī, Jurjānī, and Qūshjī, who themselves were well aware of their predecessors. As we have seen, the latest members of this debate, Qūshjī and Bīrjandī, had already anticipated the main lines of argument that would animate the debate in Europe that began with Copernicus's bold assertion of a rotating Earth, an assertion that Qūshjī had tentatively suggested in the previous generation.

None of this, of course, proves that Copernicus was indebted to his Islamic predecessors and contemporaries on this point. What it does show is that one of the crucial arguments used by him had already been debated extensively in the adjoining cultural area. And furthermore, it had been debated as part of an ongoing *astronomical* debate and not simply as a scholastic, philosophical, or theological exercise as was the case in fourteenth-century Europe. Indeed, the question of the Earth's rotation

[34] This is true even when they were writing in a theological context as Qūshjī was in the remarks quoted above. In other parts of the *Sharḥ Tajrīd*, he was at some pains to defend astronomy from those theologians who would disparage it (Ragep 2001).

[35] We should recall that Qūshjī also raised the possibility that something could have both a rectilinear inclination and a natural circular motion (see above).

became a staple of the larger question of the role of Aristotelian physics in mathematical sciences, a question whose resolution would have such profound consequences for the history of science in sixteenth- and seventeenth-century Europe.

It is thus not only the similarity of the arguments used by Ṭūsī and Copernicus, but the intellectual contexts that make the case for influence and transmission so strong. Added to the overwhelming evidence that Copernicus also used Islamic astronomical models, the case becomes, in my view, compelling. One is still, though, left with the conspicuous lack of textual evidence in the form of translations to cinch the case. This indeed is a puzzle and, perhaps, forces us to look much more seriously at the possibility of oral transmission and contemporary interaction. For those accustomed to dealing with the early thirteenth century as the terminal point of Islamic influence on Europe, this suggestion will seem extreme and unwarranted. But given the increasing evidence of untranslated Islamic scientific products showing up in early modern Europe, it is time to rethink the cultural and geographical boundaries of this crucial period in the history of science.[36]

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[36] Obviously it is not possible to deal with this complex question in a footnote, but the following literature gives tantalizing hints of continuing transmission and influence of Islamic science during the late medieval and early modern periods. For the possible influence of Ibn al-Nafīs's (Damascus, thirteenth century) discovery of pulmonary circulation on Michael Servetus (Spain, sixteenth century), see Meyerhof 1935. For some compelling evidence of the influence of Islamic mathematics from the circle of the Marāgha observatory (thirteenth century) upon Levi ben Gerson (France, fourteenth century), see Lévy 1992. Another indication of the importance of Jewish scholars in the transmission of Islamic science to early modern Europe is given by Y. Tzvi Langermann, who has discovered Jewish writers, including Mordecai Finzi (Italy, fifteenth century), who knew of the Ṭūsī couple; see Langermann 1996, 34–35. Another article that investigates the possibility that "Marāgha" astronomy was known in fifteenth-century Europe is Dobrzycki and Kremer 1996. Further evidence that Ṭūsī's *Tadhkira* was known during this period is provided by the Latin annotations found in an Arabic manuscript of the work currently in the Vatican (ar. 319). And finally, the question of the influence of late Islamic observatories upon those of early modern Europe is explored by Sayili 1960, esp. chaps. 9–10.

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Ibn al-Haytham and Eudoxus: The Revival of Homocentric Modeling in Islam

F. JAMIL RAGEP*

David Pingree has written the book (actually many) on the transmission of science between cultures. So it seems appropriate to present him with a Persian text for his seventieth birthday, a text describing an astronomical model whose roots go back to fourth-century B.C. Athens, one that managed to get criticized in second-century A.D. Alexandria, that somehow influenced an eleventh-century Cairene transplanted from Basra, that was picked up in thirteenth-century Iran, and then made its way—through channels unknown—to Europe. A convoluted and still puzzling tale to warm the heart of the master!

1 *Introduction*

Ibn al-Haytham (965–ca. 1040) wrote a number of works on astronomy.[1] Historically he was best known for his *Maqāla fī hay'at al-ʿālam* (Treatise on the Configuration of the World), which gained for him considerable renown in both Islamdom and in Europe.[2] In particular, Islamic sources often accorded him the role of having taken Ptolemy's imaginary circles and turning them into solid, spherical bodies. Though this is, at best, dubious history, it did provide a nice story for Islamic astronomers that con-

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[1] Sabra [1972], 197–9, 205–8; Sezgin [1978], 251–61.

[2] Langermann [1990] provides an edition and translation along with a discussion of the influence of the work.

trasted their work with that of their Alexandrian predecessor.[3] It also provided a backdrop for another of Ibn al-Haytham's works, namely his *al-Shukūk ʿalā Baṭlamyūs* (Doubts About Ptolemy).[4] In it, he criticized Ptolemy for certain models (for example those employing the equant) that allowed irregular motions to occur in the heavens.

It is often stated that Ibn al-Haytham (as well as other early critics of Ptolemy) 'did not propose new models to replace those to which they were objecting...' (Saliba [1994], 21) but this turns out not to be the case. A number of years ago, A. I. Sabra [1979] published Ibn al-Haytham's reply to an anonymous critic of a work of his called *Maqāla fī ḥarakat al-iltifāf*, in which Ibn al-Haytham proposed a model to account for the latitudinal motions of the planetary epicycles. This work was evidently written before the *Doubts About Ptolemy*, which is promised at the end of his reply (Sabra [1979], 398 = 207 (Arabic numeration)). Unfortunately, the *Maqāla fī ḥarakat al-iltifāf* itself seems to be irretrievably lost. Its contents, however, can be reconstructed, thanks to a number of later accounts, the most extensive of which turns out to be in Persian. Naṣīr al-Dīn al-Ṭūsī (1201–1274) wrote this exposition in a supplement that he appended to his *Risālah-i Muʿīniyya*, written in 1235 while he was part of the court of the Ismāʿīlī governor of Qā'in in eastern Iran. This Appendix, variously entitled *Ḥall-i mushkilāt-i Muʿīniyya, Sharḥ-i Muʿīniyya*, or *Dhayl-i Muʿīniyya* ('The Solution of Difficulties', 'A Commentary' or 'An Appendix' for the *Muʿīniyya*), is important for a number of reasons, not the least of which is that it is the first instance in which Ṭūsī presents the rectilinear version of his famous 'couple' by which he used an oscillation on a straight line to deal with a number of difficulties in Ptolemaic planetary theory. But it is clear that he had not yet figured out how to produce a curvilinear oscillation on a great circle arc (which he later used in the *Tadhkira* to deal with such problems as Ptolemy's latitude theory), so in the Appendix he instead presented Ibn al-Haytham's model from the *Maqāla fī ḥarakat al-iltifāf* as one possible way to

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[3] See, for example, Ibn al-Akfānī (d. 1348), *Irshād al-Qāṣid*, in Witkam [1989], 57–8 (= 408–407), who is probably following astronomers such as al-Khiraqī (d. 1138–9) ; cf. Ragep [1993], 1: 30–3.

[4] The work has been edited by A. I. Sabra and N. Shehaby [1971]. An English translation is in D. L. Voss [1985].

deal with the problem (Ragep [1993], 1: 65-70 and Ragep [2000]). It is this chapter of the Appendix that is presented below in a Persian edition and English translation.

That Ṭūsī was not satisfied with Ibn al-Haytham's proposal was made abundantly clear in his *al-Tadhkira fī ʿilm al-hay'a*, where he notes that such a model, by retaining Ptolemy's small circles, will produce motion in longitude as well as latitude, thus altering the correct positions of the apex and perigee. He also was dissatisfied with the fact that motion on the small circles is uniform about a point other than the center of the circle, thus making the motion analogous to the irregular motion of the epicycle center on the deferent due to its uniformity with respect to the equant (Ragep [1993], 1: 214-7, 2: 450-2). It is interesting that Ṭūsī indicates similar dissatisfaction with Ibn al-Haytham's model in the *Risālah-i Muʿīniyya*, where he says that 'even with this postulation the irregularity is not ordered, and in addition several other corruptions come into being. But this is not the place to explain them' (Ragep [2000], 125). Nevertheless, he presents Ibn al-Haytham's model without criticism of any sort in the Appendix, thus clearly signaling that, despite having devised and introduced the rectilinear couple in the Appendix, he has yet to produce a model of his own (i.e. the curvilinear couple of the *Tadhkira*) to account for Ptolemy's latitudinal motions.

The model itself is Ibn al-Haytham's attempt to give a physical representation to Ptolemy's 'small circles', which the latter introduced in *Almagest*, XIII.2 to account for latitudinal variations in the epicycles, the so-called deviation and slant (Toomer [1984], 599-600). What is of great interest is that this model turns out to be essentially the same as one that is attributed to Eudoxus of Cnidus (fourth century B.C.). In particular, Ibn al-Haytham proposes to produce the small circles by means of two homocentric spheres that have different axes of rotation, each rotating at the same speed but with opposite angular rotations (see Figure C1). One rotating sphere (viz. KL), of course, could cause a given point to describe the needed circle, but in such a case the entire epicycle would also rotate, thus seriously disrupting the position of the planet. The second sphere (MN), which is contained inside the first and has an axis that always goes through the apex and perigee of the epicycle, would return the epicycle to its correct position. At least this is the theory. In fact, these two orbs of Ibn al-Haytham correspond to orbs three and four of Eudoxus's planetary models, i.e. the ones that are meant to account for the retrograde motion by means of the 'hippopede' (Figure C2).[5]

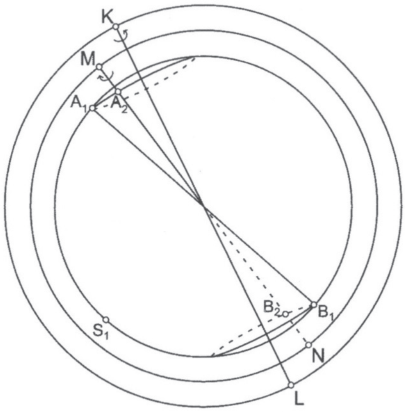

Figure C1

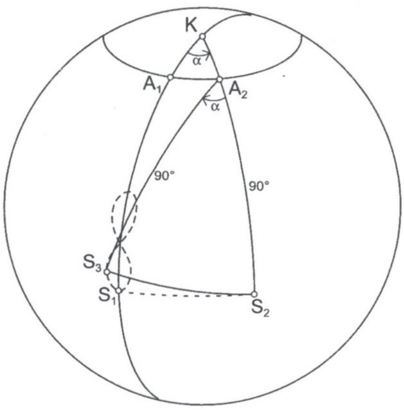

Figure C2

But this consequence of the Eudoxan model plays no role in Ibn al-Haytham's theory and indeed the hippopedal motion of the mean distance S between the epicyclic apex A and perigee B, if acknowledged, would have been an unwelcome complication of an already complex theory, since Ibn al-Haytham's goal is to have sphere MN return all points—other than the apex and perigee—of the epicycle, including S, to the eccentric plane.

[5] For a lucid account of the Eudoxan system, see Heath [1913], 190–211.

Has Ibn al-Haytham been somehow influenced by Eudoxus, most likely through the intermediation of Aristotle who presented Eudoxan planetary theory in Book XII, Chap. 8 of his *Metaphysics*? Ibn al-Haytham himself in his reply to a critic of his original *Iltifāf* treatise states that there is a generic affiliation between Aristotle's *iltifāf* and the *iltifāf* motion that he discusses in his own treatise. But the two are distinctly different, according to Ibn al-Haytham, since 'the latter is not the former, it resembles it only. The proof of this is that the mathematicians do not use it nor mention it, i.e. that which is referred to by Aristotle, since they do not need it and Aristotle does not employ the motion of the epicycle orb, nor does he [even] have a word for it' (Sabra [1979], 401=204 (Arabic numeration)). It is interesting that Ibn al-Haytham does not cite the obvious connection between his model and that of Eudoxus's hippopedal model but instead merely refers to 'Aristotle's' solar model that employs orbs with different axes for the daily motion, ecliptic motion and latitudinal motion (Sabra [1979], 403–402=202–203 (Arabic numeration)). Thus the 'generic' affiliation seems to be that homocentric orbs with different axes are employed both by himself and Aristotle to achieve an *iltifāf* motion. But for Aristotle, this is an 'accidental' result and the end product is a spiral motion with endpoints at the solstices whereas Ibn al-Haytham's *iltifāf* is based on 'specific bodies' that are designed to produce a circular motion on the epicycle orb (Sabra [1979], 401=204 (Arabic numeration)). From this discussion, it is clear that Ibn al-Haytham has not explicitly connected what he is doing with Eudoxus's planetary orbs three and four; there remains, though, the indirect influence from Aristotle of using connected homocentric spheres with different axes to produce a desired result. In the *Doubts* Ibn al-Haytham criticizes Ptolemy's abandonment of the 'small circle' latitude model in the *Almagest* in favor of a two-sphere arrangement in the *Planetary Hypotheses*.[6] But it is curious that he refrains from referring to his own treatise on *Iltifāf*, which undoubtedly was written earlier; this tends to underscore that the *Doubts* is a work of criticism rather than constructive engage-

[6] For an account of the latitude model of the *Planetary Hypotheses*, see Neugebauer [1975], 2: 908–9, 924–5. For the Arabic text of Ibn al-Haytham's criticisms, see Sabra and Shehaby [1971], 43–58; Voss [1985] provides an English translation, 61–78 and discussion, 147–71.

ment.

What is the significance of Ibn al-Haytham's *Iltifāf*? Ultimately, it was a minor work that nevertheless possesses a certain historical significance. For one thing, it is one of several works by early (pre-thirteenth century) Islamic astronomers that proposed new models to deal with a variety of ills they felt they had inherited from the Ancients.[7] It also evidently played an important role in the thinking of Naṣīr al-Dīn al-Ṭūsī , who not only summarized it for his Persian readers but also would later give it a certain prominence by transforming it in the *Tadhkira* into his own curvilinear version of the Ṭūsī couple.[8]

Finally Ibn al-Haytham's text marks an important step in the revival of homocentric modeling in the Middle Ages. The latter is best known in Islam through the work of the twelfth-century Andalusian Nūr al-Dīn al-Biṭrūjī, who sought to abandon the eccentrics and epicycles of Ptolemaic astronomy in favor of a purer, homocentric system in which all the orbs were geocentric. Whether this was inspired by the Eudoxan system has been a matter of some controversy.[9] Perhaps it would help to distinguish between a homocentric model and a homocentric system. An astronomical model (*aṣl* in Arabic; *hypóthesis* in Greek) is a device that can be used for a particular purpose within a larger system (Arabic: *hay'a*). Thus one may speak of an eccentric or epicyclic model but not of an eccentric or epicyclic system. On the other hand, one may refer to either a homocentric model or a homocentric system. The latter would include the systems of Eudoxus and al-Biṭrūjī. One might say that Biṭrūjī's *system* is doubtless inspired by an Aristotelian view that all motion in the heavens must be about a single center. Since Biṭrūjī also knows the Eudoxan system through Aristotle, it does not take much of a leap to conclude, as has E. S. Kennedy, that Biṭrūjī's system owes much to fourth-century B.C. Greek astronomy. On the other

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[7] Examples include the various early theories of trepidation and Jūzjānī's eleventh-century attempt to deal with the equant; cf. Ragep [1993], 2: 452.

[8] It is of great interest that Ṭūsī presents his model as a transformation of Ibn al-Haytham's; see Ragep [1993], 1: 216–17, 2: 452–5.

[9] See, for example, Kennedy [1973] and Goldstein [1964] and [1971]. The former held that Eudoxus was the ultimate source of Biṭrūjī's astronomical system, whereas the latter insisted upon an origin in the various Islamic trepidation theories.

hand, this does not necessarily imply that Biṭrūjī knew or understood all the details of Eudoxan astronomy per se; as was the case with Ibn al-Haytham, he could have simply understood that the systems described in Aristotle's *Metaphysics* were homocentric and based upon a series of embedded, interconnected orbs with different axes of rotation. As B. Goldstein has noted, there could have been other sources that Biṭrūjī drew upon for the details of his *models*, and he pointed to several theories of trepidation that employ homocentric models. But as we now know, there are other astronomers before Biṭrūjī, such as Ibn al-Haytham, who were using homocentric modeling for purposes other than trepidation. And we know that this aspect of Ibn al-Haytham's astronomical corpus was influential both in Islam and the Latin West.[10]

2 *Edition and Translation of Chapter 5 of Ṭūsī's Ḥall-i mushkilāt-i Muʿīniyya.*

The edition is based upon 3 manuscripts:

1) F Istanbul, Fatih 5302/4, ff. 204a-205a (722 H./1322 A.D.)
2) K Oxford Or. 208 (Bodl.), ff. 145b-148a (n.d.)
3) M Tehran, Malik 3503, pp. 14–17 (658 H./1260 A.D.); published in facsimile in al-Ṭūsī 1956

Abbreviations used in Apparatus:

F_{ab} = above in MS F
F_{cr} = crossed out in MS F
F_{mr} = in margin of MS F
F_{rp} = repeated in MS F

The Persian text is part of a collaborative effort by Professor Wheeler Thackston, Ms. Sally Ragep and myself to establish the entire *Risālah-i Muʿīniyya* and its *Ḥall*. Thackston has established a preliminary edition of both works based upon MSS F and M, and S. Ragep has been collating this preliminary edition with another nine manuscripts. It has been my responsibility to then establish a 'final' apparatus, text and commentary. In the case of this particular chapter, only the three listed manuscripts

[10] For evidence of the influence of his homocentric models in the Latin West, see Mancha [1990].

of the eleven we have been using contained all of Chapter 5 of the *Ḥall*.

The English translation generally follows the procedures and vocabulary used in my translation of Ṭūsī's *Tadhkira* (Ragep [1993]). This represents a revised version of an earlier translation done a number of years ago by Prof. Thackston and myself. But, as with the edition, all remaining shortcomings are the responsibility of the present author alone.

Glosses are keyed to the translation and are given as footnotes. Only problematic readings or interpretations (such as the mix-up in the order of the orbs) are discussed; a more detailed commentary on Ibn al-Haytham's model and its relation to that of Ṭūsī can be found in Ragep [1993], 2: 450-5.

Figures 1 and 2 have been established from the three manuscripts, which exhibit minor variations. Figure 2 follows the medieval convention of rendering a 3-dimensional effect by 'flipping' the circles horizontally onto the page. (See Ragep [1993], 1: 218-9 for another example.) Figure 2c is an attempt to present the diagram using a more modern perspective.

Figure 2C

حلّ مشكلات معینیه در هیئه لنصیر الدین طوسی

فصل ۵

در هیأت افلاك تداویر سیارگان[1]
بر مذهب ابو علی بن الهیثم[2]

این ابو علی[3] از مبرزان علم ریاضی بوذه است و هیأت افلاك بر وجه تجسیم بیشتر مستفاد از سخن اوست . و اورا رسالهٔ است در بیان افلاك تداویر کواکب بر وجهی که این حرکات مختلف از آن صادر شوذ . می‌گویذ هر یکی از[4] کواکب علوی سه فلك تدویر دارند بیکدیگر محیط .

فلك اول که در میان دو فلك دیگر باشد ، فلکی بوذ مصمت که کوکب[5] بر یك طرف او باشد . و آن کره متحرك بوذ بحرکت خاصهٔ کوکب . و منطقهٔ او توهم کنیم که از سطح منطقهٔ خارج مرکز خارج بوذ و با او مقاطع[6] در دو[7] بعد اوسط . پس قطری که بدو بعد اوسط بگذرذ در سطح منطقهٔ خارج بوذ ، و قطری که بذروه و حضیض[8] بگذرذ یك نیمه در جهتی و دیگر نیمه در جهتی دیگر[9]. و چون خطی از مرکز مایل[10] بمرکز تدویر کشند و اخراج کنند تا بر تدویر بگذرذ ، هر آینه با این قطر[11] که بذروه و حضیض گذشته است بر مرکز تدویر تقاطع کنذ . و چون این خط در سطح فلك خارج باشد ، بعد میان این خط و قطر تدویر در ذروه و حضیض بقدر میل ذروه و حضیض باشد برین صورت :[12]

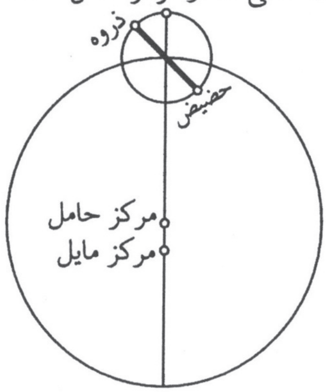

Figure 1A

پس فلك دوم توهم كنيم كه بذين فلك محيط باشد و مركز هر دو فلك[13] يكى بوذ . و اين فلك[13] بر دو قطب ازين خط كه[14] از مركز مايل آمذه است ، متحرك باشد بحركتى شبيه بحركت مركز تدوير بر[15] محيط مركز معدل مسير . و لامحالة چون اين فلك حركت كند ، و فلك اول را با خوذ ببرذ ، ذروه و حضيض را دو[16] مدار حادث شوذ كه مركز هر يكى از آن دو مدار بر خطى بوذ كه از مركز مايل آمذه است . و آن دو دايرهٔ خرد[17] بوذ كه سطح هر يكى با سطح فلك خارج متقاطع بوذ بر زواياى[18] قايمه ، مانند هدفى كه قطرش بر محيط سپرى نهند . پس دو موضع ازين دايره[19] در سطح فلك حامل بود . و چون ذروه و حضيض بر محيط اين دايره[19] حركت كنند ، هرگاه كه بذين دو نقطه رسند در سطح فلك خارج باشند . و چون بر منتصف هر دو نقطه باشند در غايت ميل باشند از سطح خارج .

لیکن[20] ازین حرکت فسادی لازم آید . و آن چنان بوذ که چون همه تدویر بذان حرکت متحرك بوذ ، قطری که بدو بعد اوسط گذشته باشد از سطح حامل خارج شوذ و دوری[21] بکند که در اثنای آن نصف شرقی از تدویر غربی شوذ و نصف غربی شرقی . پس از جهت دفع این فساد ، فلکی[22] دیگر توهم کنیم که آن فلك سوم باشد محیط باین هر دو فلك چنانکه مرکزش مرکز هر دو فلك بوذ[23] ، و دو قطب او بر دو طرف قطر فلك تدویر بوذ که مارّ باشد [24] بذروه و حضیض ، و حرکت او در خلاف جهت حرکت فلك دوم بوذ ، و هم بمقدار آن حرکت تا آن قدر که منطقهٔ فلك تدویر بحرکت فلك دوم از وضع خوذ زایل شوذ ، این فلك اورا با وضع خوذ برد[25] و قطر بعد اوسط همیشه در سطح فلك خارج بماند ، اما دور[26] ذروه و حضیض بر مدارهای[27] مذکور بر قرار بماند از جهت آن که قطبهای[28] این فلك بر دو طرف قطر فلك تدویر است و قطبهای[29] فلك دوم غیر این[30] دو قطب است ، و میان هر دو قطبی[31] ازین چهار قطب[32] بقدر نصف قطر مدار ذروه یا حضیض[33] . پس ازین حرکات لازم آیذ که نصف ذروه دایما در جهتی بوذ و نصف حضیض در جهتی دیگر مخالف جهت اول ، و در هر[34] دوری از ادوار دو نوبت[35] منطقهٔ تدویر در سطح منطقهٔ خارج آیذ و بگذرذ بر[36] وجهی که جهات متبادل شوذ .

و چنانکه مرکز تدویر محیط مایل را[37] بحرکتی قطع می کنذ[38] بنسبت با مرکز مایل غیر متشابه است بنسبت[39] با

مرکز معدل مسیر متشابه تا در دو ربع که در نصف اوج افتد بطئ باشد[40] و در دو ربع دیگر سریع . همچنان[41] ذروه و حضیض این دو مداررا قطع کنند[42] بحرکتی که بنسبت با مرکز مدار[43] غیر متشابه بوذ و بنسبت با نقطهٔ دیگر غیر مرکز[44] مدار و داخل مدار که بجای مرکز معدل مسیر باشد متشابه بوذ[45] تا سیر ذروه بر محیط این مدار در دو ربع بطئ بوذ و در دو ربع سریع . و هم چنین سیر حضیض نا[46] متشابهست تا سیر مرکز محفوظ بوذ .

و این دو دایرهٔ خرد آنست[47] که صاحب منتهی الادراك در اثنای[48] اثبات اجسامی که مبادی حرکات باشند ایراد[49] کرده است و بر آن اقتصار[50] نموذه . و هر چند باول اثبات این دو دایره بطلمیوس کرده است در مجسطی ، اما چون بطلمیوس در همه احوال بر دوایر اقتصار[51] کرده است ، این باب مناسب دیگر ابواب باشذ . و کسی که در دیگر مواضع اثبات اجسام کند و اینجا بر[52] ایراد دوایر اقتصار کند ، شرط مناسبت رعایت نکرده باشد .

و اما در دو کوکب[53] سفلی همین دو فلك زاید[54] بر فلك تدویر جهت میل ذروه و حضیض اثبات می کند . [55]
و جهت حرکت انحراف دو فلك دیگر اثبات می کند . فلك اول ، که فلك چهارم بوذ ، از افلاك تداویر ایشان محیط[55] بوذ[56] بذان سه فلك . و دو قطب این فلك[57] دو نقطه بوذ از خطی که بمرکز تدویر بگذرذ در سطح فلك حامل و با خطی که از مرکز مایل آمذه باشد بر زوایای[58] قایمه متقاطع بوذ . و چون[59] فلك حرکت کند قطری‌را[60]

که مارّ بوذ بدو بعد اوسط لامحالة بر حوالی این دو قطب حرکت بایذ کرد . پس حرکت انحراف حادث شوذ ، الا آنکه چون همه منطقهٔ تدویر[61] حرکت کند ، ذروه و حضیض از وضع خوذ زایل شود و ذروه بجای[62] حضیض آیذ و حضیض بجای[63] ذروه . پس فلکی پنجم برین چهار فلك محیط بوذ که دو قطبش دو طرف خطی بوذ که بدو بعد اوسط گذشته باشد و حرکت او مخالف حرکت فلك چهارم و مساوی او تا آنچه از موضع خود[64] زایل شوذ با وضع اول آیذ . و دو بعد اوسطرا از[65] حرکت[66] فلك چهارم دو مدار لازم آیذ ، و آن دو دایرهٔ خرد بوذ که با سطح[67] فلك حامل بر زوایای[68] قایمه متقاطع باشذ ، مانند هدفی که بر سپری نهند ، چنانکه[69] هر دو محیط بر نقطهٔ مماس باشند و سطح با سطح متقاطع بر زوایای قایمه[70] . و حرکت این دو بعد بر محیط این دو دایره مختلف در یك نیمه سریع و دیگر نیمه بطئ[71] ، مانند[72] سیر مرکز بر فلك[73] مایل . و چون ذروه در مدار خوذ در غایت میل بوذ از سطح مایل ، بعد اوسط در سطح مایل بوذ . و چون بعد اوسط از[74] سطح مایل در غایت میل بوذ ، ذروه در سطح مایل بوذ تا این دو عرض بر سبیل تبادل لازم آیذ .

و صورت افلاك تداویر این دو کوکب[75] بر حسب[76] آنچه بر سطح توان کشید اینست . و صورت افلاك کواکب علوی هم از اینجا معلوم شوذ چون بر سه فلك[77] اقتصار[78] کنند .

اینست بیان این مقالت ، و الله اعلم بالصواب[79] .

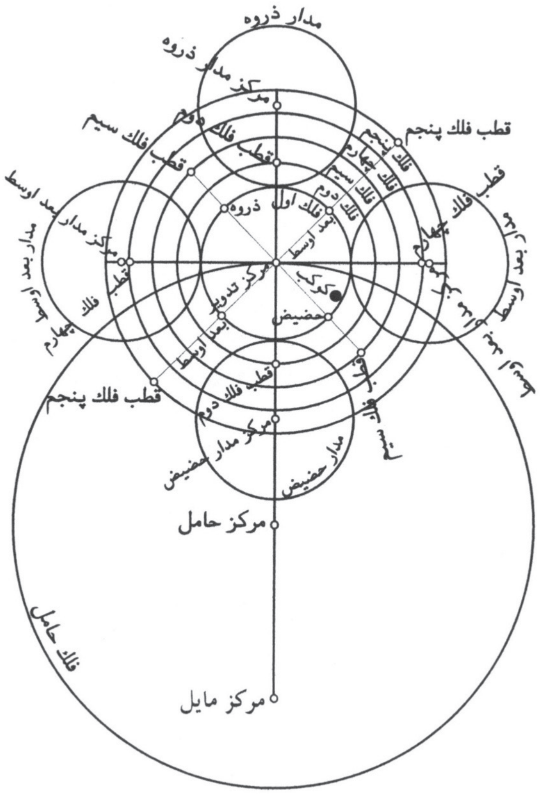
مدار ذروه
مركز مدار ذروه
قطب فلك پنجم
فلك سيم
فلك اول
ذروه
بعد اوسط
مركز تدوير
كوكب
حضيض
مدار بعد اوسط
قطب فلك پنجم
مركز مدار حضيض
مدار حضيض
مركز حامل
فلك حامل
مركز مايل

Figure 2a

Text Variants

1 سیارگان] ستارکان : F . 2 الهیثم] +رحمه الله : F . 3 ابو علی]
مرد : F . 4 از] ازین : K . 5 که کوکب] که ~~فلك~~ کوکب : K .
6 مقاطع] K_{mr} , K_{ab} = مطالع : K_{cr} . 7 در دو] + بروج :
M_{cr} . 8 و حضیض] حضیض: K . 9 دیگر] + افتد : K . 10
مایل] حامل : K . 11 قطر] قدر : F . 12 این] آن : K . 13 یکی بوذ
و این فلك] –F . 14 که] –M . 15 بر] –K . 16 دو] هر دو : M .
17 خرد] F_{mr} . 18 زوایای] زوایا : M . 19 در سطح ... محیط این
دایره] –M . 20 لیکن] لکن : M . 21 و دوری] دوری : K .
22 فلکی] فلك : K, M . 23 بوذ] –F . 24 باشد] باشند : M . 25 برد]
–M = اورد : K . 26 دور] در : F . 27 مدارهای] مدارها : M . 28
قطبهای] قطبها : M . 29 قطبهای] قطبها : M . 30 این] ان : K . 31
قطبی] قطب : K . 32 قطب] قطر : M . 33 یا حضیض] تا
حضیض : F = با حضیض : M = + باشد : K . 34 در هر] در هم
: M . 35 دو نوبت] و نوبت : M . 36 بر] و بر : M .
37 مایل‌را] حامل‌را : K . 38 کنذ] + که : F, K . 39 بنسبت] و
بنسبت : F, K . 40 بطئ باشد] بطیست : M . 41 همچنان] و
همچنان : K . 42 کنند] کند : K . 43 مدار] K_{rp} . 44 مرکز]
مروکز : M . 45 بوذ] باشد : F . 46 نا] تا : F . 47 آنست]
است : F . 48 اثنای] اثنا : F . 49 ایراد] این او : M . 50 اقتصار]
اقتضا : M . 51 اقتصار] اختصار : M . 52 بر] –K . 53 دو
کوکب] کوکب : K . 54 زاید] M_{rp} . 55 و جهت ...ایشان
محیط] و بجهت میل طرف قطر بعد اوسط همجنین فلکی چهارم
توهم کنند که محیط : K . 56 بوذ] –F . 57 این فلك] این

~~دو~~ فلك : K . [58] زوايای] زوايا : K, M . [59] و چون] جون : K
= + اين : F . [60] قطری‌را] قطری : F, K . [61] تدوير] + نيز : F .
[62] بجای] بحالی : M . [63] بجای] بحالی : M . [64] خود] –M . [65] را از]
راز : M . [66] حركت] M_{ab} . [67] با سطح] با ~~دو~~ سطح : F .
[68] زوايای] زوايا : M . [69] چنانكه] F_{rp} . [70] زوايای قايمه] زوايا :
M . [71] و ديگر] و در ديكر : K = ديكر : M . [72] مانند] باشد :
F . [73] بر فلك] –K . [74] از] در : K, M . [75] دو كوكب]
كواكب : F . [76] حسب] سبيل : F . [77] فلك] + تدوير : F .
[78] اقتصار] اختصار : K . [79] بالصواب] –F, –K .

The Solution of Difficulties in the Muʿīniyya [Treatise] on Astronomy by Naṣīr al-Dīn al-Ṭūsī

Chapter Five: 'On the Configuration of the Epicycle Orbs of the Wandering Planets According to the Theory of Abū ʿAlī b. al-Haytham'

This Abū ʿAlī was a prominent mathematician, and the configuration of the orbs as solid bodies is mostly taken from his work.[11] He has a treatise[12] explaining the orbs of the planets' epicycles in such a way that the various motions result from them. He states that each of the upper planets has three epicycle orbs, one enclosing another.

The first orb, which is inside the two other orbs, is a complete solid orb on one side of which is the planet.[13] That sphere moves with the proper motion of the planet. Let us conceive that its equator is in a plane different from that of the eccentric equator, intersecting the latter at the two mean distances. Now the diameter passing through the two mean distances is in the plane of the eccentric equator and the diameter passing through the epicyclic apex and perigee will be in one half in one direction and in the other half in the other direction. If a line is drawn from the center of the inclined [orb] to the center of the epicycle and extended until it reaches the epicycle, then it necessarily intersects the diameter passing through the apex and the perigee at the center of the epicycle. Since this line is in the plane of the eccentric orb, the distance between this line and the diameter of the epicycle [passing] through the apex and perigee is equal to the inclination of the apex and the perigee according to this illustration.[14]

---

[11] As is common among Islamic astronomers and encyclopedists starting at least as early as al-Khiraqī (d. 1138-9), Ṭūsī reiterates the view that Ibn al-Haytham is responsible for putting forward the basic solid configuration (*hay'a*) of the celestial orbs. No doubt this is due to Ibn al-Haytham's *Maqāla fī hay'at al-ʿālam* (Treatise on the Configuration of the World). That Ptolemy had attempted this in the *Planetary Hypotheses*, a work known to Ibn al-Haytham and Ṭūsī, seems not to have made much of an impression (cf. Ragep [1993], 1: 30-3 and the introduction above).

[12] This is Ibn al-Haytham's work whose title was most likely *Maqāla fī ḥarakat al-iltifāf*; see Sezgin [1978], 260 (no. 25) and Sabra [1979], 390.

[13] This first orb, of course, is the 'original' epicycle.

[14] Apparently the apex and perigee in Fig. 1 are meant to be above and below the plane of the paper at the maximal inclination of the epicyclic

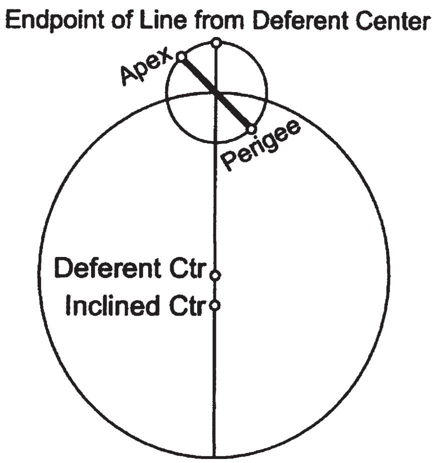

Figure 1B

Let us now conceive the second orb as enclosing this [first] orb, and each of these two orbs shares the same center. This orb, which is on two poles on the line coming from the center of the inclined orb, moves with a motion like that of the epicycle center on the circumference of the equant center [circle].[15] There is no doubt that when this orb moves and carries the first orb with it, then [both] the apex and the perigee describe two circuits each of whose centers is on the line which comes from the center of the inclined [orb]. And these two small circles are such that each of their planes is perpendicular to the plane of the eccentric orb, like a stud whose diameter is on the circumference of a shield. Now two places on [each] circle are in the plane of the deferent orb. When the apex and perigee move along the circumference of [each] circle, they will be in the plane of the eccentric orb whenever they reach these two points. At the mid-point between these two points, they are at the maximum inclination from the plane of the eccentric.

There follows, however, from this motion a distortion, namely that since the entire epicycle moves with this motion, the diameter passing through the two mean distances goes out of the plane

'deviation' (cf. Ragep [1993], 1: 190–3).

[15] This is somewhat misleading. In fact Orb 2 moves the apex and perigee with a motion correlated with the actual irregular motion of the epicycle center on the deferent, not with the idealized uniform motion on the imaginary equant circle. Cf. note 17 below.

of the deferent. As it moves around, the eastern half of the epicycle becomes western and the western half becomes eastern. Then in order to rectify this distortion, let us conceive another orb, which is the third orb enclosing these [previous] two orbs, in such a way that its center is the center of both orbs. Its two poles are at the two end-points of the diameter of the epicycle orb that passes through the apex and perigee. Its motion is in the opposite direction of the second orb's motion but equal to it, so that by however much the equator of the epicyclic orb is displaced from its proper place by the motion of the second orb, this orb brings it back to its proper place, and the diameter of the mean distance always remains in the plane of the eccentric orb. However the revolutions of the apex and the perigee on the above-mentioned circuits remain fixed inasmuch as the poles of this orb are at the two end-points of the diameter of the epicycle orb. The poles of the second orb are different from these two poles. The distance between each two of these four poles is equal to the radius of the circuit of the apex or of the perigee. Therefore from these motions it follows that the apex half is always in one direction and the perigee half is in another direction opposite that of the first. In every revolution the equator of the epicycle passes twice through the plane of the eccentric equator in such a way that the directions interchange.[16]

As the epicycle center traverses the circumference of the inclined [orb] with a motion that is nonuniform with respect to the inclined center, it is uniform with respect to the equant center, so that in the two quadrants that fall in the apogee half it is slower while in the other two quadrants it is faster. Similarly, the apex and the perigee traverse [their] two circuits with a motion that is nonuniform with respect to the circuit's center but uniform with respect to a point other than the circuit's center that is within the circuit in a position [corresponding to that of] the

[16] There is a serious error in the order of the orbs. For the model to actually work, Orb 3 should be contained inside Orb 2 and not the reverse, as is presented here and in Figure 2; otherwise the apex and perigee will not remain on the small circle and the diameter connecting them will not stay aligned with the poles of Orb 3. Whether this was a careless error due to Ibn al-Haytham or Ṭūsī is not clear. Ṭūsī does correct the mistake, without comment, in the *Tadhkira* where he places Orb 3 between Orb 2 and the epicycle (Ragep [1993], 1: 214–5 and 358, Fig. C23).

equant center, so that the movement of the apex on the circumference of this circuit is slower in two quadrants and faster in [the other] two quadrants. And likewise the movement of the perigee is nonuniform so that the motion of center is preserved.[17]

These two small circles are those that the author of *Muntahā al-idrāk* introduced when positing the bodies that are the principles of motion, and he limited himself to that. Even though these two circles were first posited by Ptolemy in the *Almagest*, this chapter is consistent with other chapters inasmuch as Ptolemy limited himself in all cases to circles. [However] someone who in other places posits bodies but here limits himself to circles is not observing the condition of consistency.[18]

Moreover for the two lower planets, he [Ibn al-Haytham] likewise posits two orbs in addition to the epicycle orb to account for the inclination of the apex and the perigee; to account for the motion of the slant, he posits two other orbs. The first orb, which is the fourth orb of their [i.e. Ibn al-Haytham's] orbs for the epicycles, encloses the other three orbs. The two poles of this orb are two points on a line passing through the epicycle center in the plane of the deferent orb and intersecting at right angles the line that comes from the center of the inclined [orb]. When

[17] Though Ptolemy claims that the small circle 'revolves with uniform motion, with a period equal to that of the motion in longitude' (Toomer [1984], 599), it is actually, as Ṭūsī notes here, nonuniform since the motion must be correlated with the motion of the epicycle center on the deferent, which is uniform with respect to the equant center but nonuniform with respect to the actual deferent center. Ṭūsī himself presented this in the *Tadhkira* as part of his criticism of Ptolemy's model (and by implication Ibn al-Haytham's) since this results in the apex and perigee moving nonuniformly on the small circle (Ragep [1993], 1: 212–7). One might well interpret what he says in the *Muʿīniyya* itself as also being a criticism of this aspect of Ibn al-Haytham's model: 'Yet even with this postulation the irregularity is not ordered, and in addition several other corruptions come into being' (Ragep [2000], 125). But here he presents Ibn al-Haytham's model without explicit criticism.

[18] The author in question is Shams al-Dīn abū Bakr Muḥammad b. Aḥmad al-Khiraqī (d. 533 H./1138–9), who, in addition to the *Muntahā al-idrāk fī taqāsīm al-aflāk*, wrote *al-Tabṣira fī ʿilm al-hay'a*, both of which were very influential in the development of the *hay'a* (mathematical cosmography) tradition in Islam; see Ragep [1993], 1: 33, 36. Note the criticism of Khiraqī's inconsistency in presenting circles in some places but physical bodies in others; at least, according to Ṭūsī, Ptolemy was consistent in always using circles in the *Almagest*. (This represents a rather rare apology for the much maligned Alexandrian astronomer.)

the orb moves, the diameter, which passes through the two mean distances, must necessarily move around these two poles. Thus there results the motion of the slant except that since the entire epicyclic equator moves, the apex and the perigee will become displaced from their proper places; the apex goes to the perigee's place and the perigee to the place of the apex. Therefore a fifth orb encloses these four orbs; its two poles are the two end-points of the line that passes through the two mean distances. Its motion is opposite and equal to the motion of the fourth orb so that whatever is displaced from its proper place will return to its original position.[19] Now two circuits for the two mean distances result from the motion of the fourth orb, and these are the two small circles that intersect the plane of the deferent at right angles, like a stud on a shield, such that each of the two circumferences are tangent at a point and one plane intersects the other at right angles. The motion of these two [mean] distances on the circumference of these two circles varies in each half, being faster in one half, slower in the other, corresponding to the motion of center on the inclined orb. When the apex on its own circuit is at the maximum inclination from the inclined plane, the mean distance is in the inclined plane. And when the mean distance is at the maximum inclination from the inclined plane, the apex is in the inclined plane. Thus it follows that these two latitudes are inverses of one another.

The illustration of the orbs of the epicycles of these two planets, to the extent they can be drawn in a plane, is as follows. The illustration of the orbs of the upper planets may also be known from [the following] since they are limited to three orbs.

This is the exposition of this treatise, and God is the Knower of Truth.

[19] The same criticism regarding the ordering of Orbs 2 and 3 also applies here to Orbs 4 and 5, which should be reversed. See note 16 above.

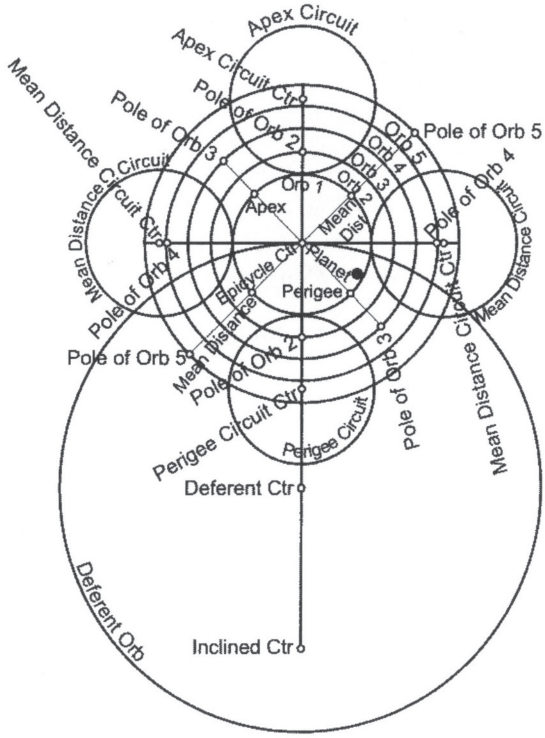

Figure 2B

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AL-BATTĀNĪ, COSMOLOGY, AND THE EARLY HISTORY OF TREPIDATION IN ISLAM*

F. Jamil Ragep

I. *Introduction*

In recent years, there has been a renewed interest in Islamic theories of trepidation.[1] Most of these studies have focused on later manifestations and developments, i.e. ones that seem, in some way or another, to derive from the work of the noted mathematician Ibrāhīm b. Sinān (908-946 A.D.),[2] who was a grandson of the Ṣābian astronomer, mathematician, and translator Thābit ibn Qurra (d. 901).[3] Ibrāhīm's work shows a notable sophistication inasmuch as his model attempted to deal simultaneously with reported variations in the rate of precession, the obliquity of the ecliptic, and the longitude of the solar apogee. His work was known and commented on, but generally rejected, in Eastern Islam, but it would seem to have had a much more profound, though indirect, influence through its probable but obscure connection with *De motu octave spere* ("On the Motion of the Eighth Sphere"), an Arabic work extant only in Latin translation that has, until recently, usually been ascribed to Thābit.[4] It was this latter work that was particularly important both for

* As always, I thank Sally P. Ragep for her help, which was invaluable and incalculable for all phases of the preparation of this paper.

[1] Trepidation is here used to translate *ḥarakat al-iqbāl wa-'l-idbār* (motion of accession and recession, i.e. direct and backwards motion). In general, any motion that results in an oscillation of either the vernal equinox or, alternatively, the fixed stars will be called trepidation. When combined with a unidirectional, constant motion of precession, the result will be either a unidirectional, variable precession, or else an oscillatory, variable trepidation of the vernal equinox or fixed stars.

[2] All dates, unless otherwise noted, are A.D.

[3] Ibrāhīm's known work on trepidation is his *Kitāb fī ḥarakāt al-shams*, which is treatise 6 of *Rasā'il ibn Sinān*, edited by Aḥmad S. Saʿīdān, pp. 273-304.

[4] For a preliminary discussion of Ibrāhīm ibn Sinān's treatise, its possible connection with *De motu octave spere*, and its historical role in eastern Islamic astronomy, see F. J. Ragep, *Naṣīr al-Dīn al-Ṭūsī's* Memoir on Astronomy *(al-Tadhkira fī ʿilm al-hay'a)*, 2: 400-

Spanish developments in trepidation, in particular the work of al-Zarqāllu (eleventh century), and for European ones down to the time of Copernicus.[5]

These rather complicated models, though, are far removed from what is generally taken to be the original theory of trepidation, which is described in Theon of Alexandria's *Small Commentary to the Handy Tables* (fourth century).[6] The historical issue that then needs to be addressed is the process by which this rather simpleminded theory came to be transformed into these later, rather formidable models. My purpose in this paper is to argue for the central role played by al-Battānī (ca. 858-929)[7] in this process as well as for the importance of cosmological considerations. My claims can be summarized as follows: (1) Battānī, following earlier Islamic astronomers of the ninth century, has made a rather drastic reinterpretation of the theory that we find presented by Theon; (2) physical cosmology played an important role in this reinterpretation; (3) though he rejected the earlier theory, Battānī was not unsympathetic to variable precession (which runs counter to what has usually been claimed in modern accounts) and has used the occasion of his

408; also evidence is there presented against Thābit's authorship of *De motu* and a case is made, admittedly speculative, that it is by Ibrāhīm. For references to the substantial literature on *De motu*, see ibid. as well as the sources listed in note 5 below.

[5] On Spanish trepidation, see now J. Samsó, "Trepidation in al-Andalus in the 11th Century," which contains extensive references to the literature. On Copernicus's trepidation, and the tradition that he inherited, see the important discussion in N. M. Swerdlow and O. Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus*, 1: 42-43, 61, 72-74, 127-172.

[6] The Greek text can be found in A. Tihon, *Le "Petit Commentaire" de Théon d'Alexandrie aux tables faciles de Ptolémée*, pp. 236-237 (French translation, p. 319). An English translation is in O. Neugebauer, *A History of Ancient Mathematical Astronomy* [hereafter *HAMA*], 2: 632.

[7] His full name is Abū ᶜAbdallāh Muḥammad ibn Jābir ibn Sinān (or perhaps ibn Sinān ibn Jābir) al-Ḥarrānī al-Battānī; his roots were in the pagan, Ṣābian community of Harrān, but he himself was a Muslim and lived most of his life in Raqqa (along the Euphrates in northern Syria). He is most noted for his *Zīj*, which was the subject of a masterful edition, translation (into Latin), and study by Carlo Nallino (*Kitāb al-Zīj al-Ṣābi' (Opus astronomicum)*, 3 vols. (Milan, 1899-1907); hereafter Nallino, *Battānī*).

rejection of ancient trepidation to present an alternative conceptualization to explain discrepancies in reported year-lengths, precessional rates, and solar motions; and (4) this alternative provided the framework within which later models were developed.

II. *Theon's account of trepidation*

Before proceeding further, we will need to summarize Theon's account of trepidation from his *Small Commentary to the Handy Tables*, which is currently the only known ancient source having an exposition of this theory.[8] He states that certain ancient astrologers (*οἱ παλαιοὶ τῶν ἀποτελεσματικῶν*) assume that the solstitial points have a back and forth motion at a rate of 1°/80 years and an amplitude of 8°; their maximum forward motion (i.e. in the direction of the zodiacal signs) occurred 128 years before the beginning of Augustus's reign at which point they began a backward motion (i.e. opposite the direction of the signs) which will last 640 years before again reversing direction. (See Figure 1; note that for simplicity the reference point is the vernal equinox rather than the solstice.)

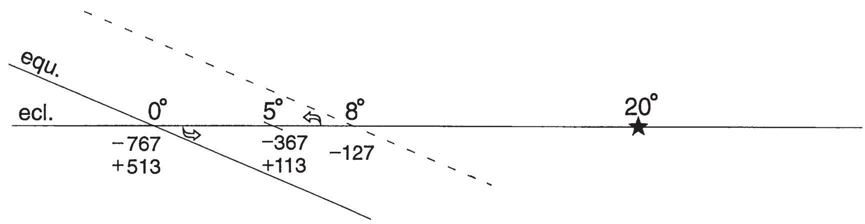

Figure 1

According to this theory, the vernal equinox, i.e. the point from which longitude is measured, will have an oscillatory motion; thus during accession (direct motion of the vernal equinox), the longitude of a given

[8] For references to the text and translation, see note 6. Besides Theon, there is a brief allusion to trepidation in Proclus's *Hypotyposis* (Neugebauer, *HAMA*, 2: 633). Islamic authors sometimes seem to refer to sources other than Theon; see Section IV.A below.

star will decrease from its original value while during recession (backward motion of the vernal equinox), the longitude of a given star will increase until it reaches its original value (see Table 1). Let us take an example. Two hundred forty years after −127 (in +113 Era Augustus), the vernal equinox will have shifted 3°; one subtracts this from 8° obtaining 5° for the new position of the equinox. To get the new longitude of a given star (let us say one whose base longitude was 20° occurring when the vernal equinox was at 0° in −767), one subtracts 5° from 20° to obtain 15°.[9]

Table 1

	Theon		Battānī	
Year of Augustus	Position of Ver. Equinox	Long. of Given Star	Position of Ver. Equinox	Long. of Given Star
−767	0°	20°	0°	20°
−367	5°	15°	0°	25°
−127	8°	12°	0°	28°
+113	5°	15°	0°	25°
+513	0°	20°	0°	20°

[9] Because Theon takes this theory to require a correction to a "standard" longitude computed with the vernal equinox being at 0°, the correction should always be subtractive. However Theon says that one *adds* the correction (*τὰς λοιπὰς ὡς τῆς τότε μεταβάσεως τῶν τροπικῶν προστιθέασι ταῖς* καταλαμβανομέναις *διὰ τῶν εἰρημένων ψηφοφοριῶν ἡλίου καὶ σελήνης καὶ τῶν πέντε πλανωμένων ἐποχαῖς*). This might be interpreted (as apparently done by Neugebauer, *HAMA*, 2: 632) to mean that the correction is added to the positions of the sun, moon, and five planets, presumably from their longitudes when the vernal equinox was at 8°. But this cannot be the case since for years after −127, Theon instructs us to add not the degrees traveled but rather the remainder, obtained by subtracting those degrees from 8°, and this can only be with reference to 0°. One way of saving Theon is to understand his correction as being to the position of the vernal equinox, which is "obtained by the said computations of the sun, the moon and the five planets"; or perhaps Theon is forgetting that the theory is with reference to 0° and is making the clumsy point that the longitudes will increase during recession from their minimum values. At any rate, this ambiguity is not sufficient to call into question the overall interpretation.

III. *Battānī's account of ancient trepidation*

In Chapter 52 of his *Zīj*, Battānī gives what at first sight appears to be simply an account of Theon's description of trepidation (although the attribution is to Ptolemy, a point to which we shall return).[10] Careful examination reveals, however, some important differences that indicate that the trepidation theory we know from Theon (which we will refer to as "ancient trepidation") has been systematically transformed from a purely mathematical theory of the Babylonian type into a theory that has been given a geometrical and cosmological interpretation. It has also been understood by Battānī as a theory that combines trepidation with a precessional motion. As we shall see in Section V, it is this transformed model that Battānī claims is not viable on both physical and observational grounds.

From nos. [1]-[3] and [6]-[11] of Battānī's account,[11] we see that the numerical details regarding the theory are identical to those of Theon (accession culminates in −127 Era Augustus at which time recession begins; the motion is at the rate of 1°/80 yrs; amplitude is 8°). However, the way in which these parameters operate lead to significantly different results. For Battānī has interpreted this trepidation as being due to a motion of an orb that causes the stars themselves to oscillate rather than being due to an oscillation of the equinoxes and solstices. For him, the vernal equinox will remain fixed, and accession and recession will be manifested in the motion of the stars. Thus if we take our previous example, namely a star with a 20° longitude in −767, its longitude will increase during the 640-year accession (rather than decrease as before) until it culminates at 28° in −127 (see Figure 2). It will then decrease during the recession until it returns to its initial value in +513. Note that in Theon's version, the longitude decreases during accession and increases during recession; the opposite holds in Battānī's version (see Table 1).

Battānī transforms Theon's version of trepidation, at least as it has usually been understood by modern historians, in yet another way: he evidently conceives that trepidation is an *additional* motion of the ecliptic

[10] Nallino, *Battānī*, 3: 190-192 (Arabic text), 1: 126-128 (Latin translation), 1: 298-304 (commentary). An English translation is contained in the *Appendix* below.

[11] Numbers in brackets refer to sentences of the translated text in the *Appendix*.

orb, i.e. over and above the precessional motion.[12] This would require, however, that the ecliptic orb (i.e. the eighth orb containing the fixed stars) move with two motions, that of precession as well as that of trepidation, which for him presents a problem of celestial physics that we will return to in Section V below. Depending on the rate of precession, the combination of trepidation and precession would either lead to 2 different rates of forward precession during accession and recession or else to a forward and retrograde (i.e. trepidational) motion of the stars.[13] In other words, Battānī's understanding of trepidation would not then constitute a substitute for precession but rather a motion additional to it that would lead to either variable precession or variable trepidation.[14]

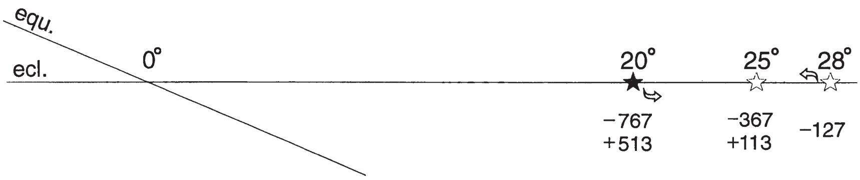

Figure 2

[12] This is quite clear from nos. [3] and [4] (see *Appendix*) since he says that during the accessional (west to east) motion the orb moves with (*maᶜa*) the west to east (precessional) motion of the fixed stars while during recessional (from east to west) motions, the orb still must move "with (*bi-*) the motion of the fixed stars that is from west to east." Delambre (*Histoire de l'astronomie du Moyen Age*, p. 54), basing himself on the medieval Latin translation, correctly understood what Battānī was getting at here; however, Duhem (*Système du monde*, 2: 231), who did not think that a combination of precession and trepidation was at work, disputed Delambre and even went so far as to modify Nallino's Latin translation (without bothering to concern himself with the underlying Arabic) to get the interpretation he wanted. On the matter of interpreting Theon, note that Neugebauer calls his version a "substitute...for Ptolemy's precession" (*HAMA*, 2: 633).

[13] For a numerical example of how this might work, see Section IV.B below; cf. Ragep, *Naṣīr al-Dīn*, 2: 399.

[14] Here variable trepidation would mean that the accessional and recessional rates are different.

IV. *Sources and motivations for Battānī's reinterpretation*

Do these departures from Theon represent an innovation on Battānī's part? Not at all; in fact, they are in line with an interpretation that had become more or less "standard" by the ninth century. But then we are faced with the questions of when and for what reasons early Islamic astronomers chose to see this theory in the way they did. And even more fundamentally, why were they interested in trepidation in the first place? Although our sources for early Islamic trepidation are, to say the least, murky, we can at least glean a general outline of the answers to these questions.

A. The Physicalization of Ancient Trepidation

As we have seen, Battānī takes trepidation to be due to the motion of the orb of the fixed stars, which differs in that important detail from the mathematical theory Theon is describing. He also tells us that his information comes from Ptolemy,[15] which raises the question whether Battānī (or perhaps one or more earlier Islamic astronomers upon whom he depended) had access to ancient sources other than Theon's *Small Commentary* that contained a report along the lines we find in Battānī. This seems unlikely for a number of reasons. First and foremost, there remains the fact that Theon represents our only substantial source for this theory from antiquity.[16] Next, it should be recalled that Theon, writing two centuries after Ptolemy, presented the theory in a nonphysical way; if Ptolemy or someone else had physicalized or changed it in some other manner, one would assume that Theon would somehow have reflected this. As for Battānī claiming Ptolemy as his source, this could simply be a misattribution of Theon's works to Ptolemy, which is not uncommon.[17]

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[15] No. [1] in the *Appendix*.

[16] The only other ancient reference I know of is due to Proclus; see note 8 above.

[17] See O. Neugebauer, "The Early History of the Astrolabe," pp. 242-243; Bīrūnī in his *Chronology* (Arabic, pp. 325-326; English, p. 322) cites Ptolemy's "Introduction to the Spherical Art" as his source for information about the "Chaldean" norm of beginning the seasons 8° after the equinoxes and solstices and then associates this with the 8° amplitude

We are then left to conclude that Battānī's interpretation of ancient trepidation reproduces a standard as well as purposeful reading (or misreading) by Islamic translators and/or readers of what they found in Theon. That it is standard is indicated by the virtual unanimity of Islamic sources in making the ecliptic orb the cause of this motion;[18] that it is purposeful is shown by the fact that even when Theon's *Small Commentary* is cited as a source, it is the orb that moves, not the solstices.[19]

Of course, it is certainly conceivable that a Greek author might have interpreted ancient trepidation in physical terms, especially in view of what we know more generally of the process by which Babylonian mathematical models were physicalized by Greek astronomers.[20] Indeed,

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of trepidation. But this association is his own hypothesis (*aḥsibu*) rather than Ptolemy's; and as Nallino has shown (following Suter), this information concerning the Babylonian norms comes not from "Ptolemy" but from Geminus's *Introduction to the Phenomena* (*Battānī*, 2: XIX), which Manitius (in his edition of Geminus) speculated may be the basis of the work that circulated as Ptolemy's "Introduction" in Latin (translated by Gerard of Cremona from the Arabic); cf. F. Sezgin, *Geschichte des arabischen Schrifttums* [hereafter *GAS*], 5: 157-158 and 6: 96 (who lists the title of the nonextant Arabic work without further identification) and R. Lemay, "Gerard of Cremona," p. 178 (no. 23). Bīrūnī cites Theon as his source for trepidation in his *Elements of Astrology*, p. 101, which was written after the *Chronology*. (This note corrects my earlier speculation in *Naṣīr al-Dīn*, 2: 397-398.)

[18] See, for example, Thābit ibn Qurra, our earliest source, in his letter to Isḥāq ibn Ḥunayn (preserved in Ibn Yūnus, *Ḥākimī Zīj*, p. 117; translated by R. Morelon, "Ṯābit," p. 131, to which one should add "orb" after ecliptic); Ṣāᶜid al-Andalusī, *Ṭabaqāt*, p. 40 (Cheikho ed.; correct Biyūn to Thiyūn), p. 109 (Bū-ᶜAlwān ed.), and p. 86 (Blachère trans.) (cf. Qifṭī, *Ta'rīkh*, p. 260; Funūn is obviously Thiyūn); Bīrūnī, *Chronology*, p. 326 (Arabic ed.), p. 322 (English trans.); Bīrūnī, *Elements of Astrology*, p. 101; B. Goldstein, *Biṭrūjī*, 1: 90 and 2: 177-179 (f. 45r-v); and Ragep, *Naṣīr al-Dīn*, 1: 124-125. It is curious that in the latter work Ṭūsī states that the orb is moving and also that the vernal equinox shifts position, which is clearly nonsensical but may reflect some knowledge of the actual version in Theon. Cf. ibid., 2: 399.

[19] See Ṣāᶜid al-Andalusī, ibid., and the twelfth-century author Ẓahīr al-Dīn al-Ghaznawī (Sezgin, *GAS*, 6: 102). The Arabic translation of the *Small Commentary* is no longer extant but Ṣāᶜid's description of "Theon's *Qānūn*," which includes the information on trepidation, makes it clear that this is the work that is meant.

[20] For a recent overview of this process, see A. Jones, "The Adaptation of Babylonian Methods in Greek Numerical Astronomy."

there is an interesting, and relevant, ancient precedent for just the kind of transformation we are talking about, namely reinterpreting what is essentially a purely mathematical device[21] in terms of a motion of a spherical orb. Ptolemy has done just this in his reinterpretation of Hipparchus's precession. Hipparchus seems to have understood precession as the westward motion of the tropics and equinoxes (similar to what is depicted in Figure 1 for years after −127) whereas Ptolemy took the precessional motion to be the result of a slow rotation of the sphere of the fixed stars eastward about the poles of the ecliptic (as in Figure 2 for years after −767).[22]

But Theon did not choose to make an analogous recasting of an earlier theory, which he was simply presenting as a historical curiosity. On the other hand, Islamic astronomers seem to have been unable even to describe a purely instrumentalist astronomical theory of the Babylonian type without reinterpreting it in physical terms. The reason, I believe, is that from an early period in Islamic science there was an attempt to understand all celestial motions as resulting from the motion of solid, celestial orbs. Of course, this was also true of Greek cosmology, but Islamic astronomers put more passion into this project than had their Greek predecessors, even if this meant the loss of historical accuracy.

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[21] Neugebauer refers to the principle underlying ancient trepidation as a "linear zigzag function" (*HAMA*, 2: 632).

[22] See G. J. Toomer, *Ptolemy's Almagest*, Bk. VII, chs. 1-3, esp. pp. 321 (and n. 2) and pp. 327-329. Cf. N. M. Swerdlow, "Hipparchus's Determination of the Length of the Tropical Year and the Rate of Precession," p. 304 (n. 28). Note, however, that in Book III, p. 131, Ptolemy states that "Hipparchus comes to the idea that the sphere of the fixed stars too has a very slow motion...towards the rear..." But that Hipparchus is also reported by Ptolemy to have entertained the idea (as "first hypothesis") that only stars near the zodiac had a rearward motion (p. 322), that he had questions about whether this motion was about the ecliptic poles (p. 329), and, most importantly, that he titled his book "On the Displacement of the Solsticial and Equinoctial Points" leads one to conclude that he was not committed to Ptolemy's cosmological interpretation of precession. It is worth noting here that Neugebauer suggests that Hipparchus may have been the inventor of the trepidation theory reported by Theon (*HAMA*, 1: 298, 2: 633), which would provide further evidence that he conceived of changes in stellar longitudes as the result of shifts in solstices and equinoxes rather than as a motion of a stellar orb.

B. Trepidation Plus — or Minus — Precession

But what of Battānī's other "reinterpretation," namely that Theon was combining trepidation with precession? Here one might argue that Theon himself provides the necessary indication since he refers to it as "an additional [term]" (τῆς προσθέσεως) without which "the said computations, made with [Ptolemy's] tables, agree with the instrumental data."[23] If one takes these computations to include precession, then trepidation could be seen as additional to it rather than a substitute. Admittedly, this has not been the usual way of interpreting Theon among modern historians;[24] but most Islamic writers understood him in this way.[25] J. Samsó has recently called attention to Spanish sources that make just this interpretation, namely Ṣāᶜid and Zarqāllu (both eleventh century) and Biṭrūjī (twelfth century).[26] Ṣāᶜid's remarks merely hint at this, but Zarqāllu and Biṭrūjī are completely explicit. To quote the latter:

> Theon of Alexandria maintained (*zaᶜama*) that the fixed stars have a movement by which they sometimes accede and sometimes recede, each being 8°. But it also has in addition a motion in the direction of the signs of 1°/100 yrs.[27]

According to this interpretation, the ecliptic would then have two speeds: about 1°/44 yrs (1°/80 yrs + 1°/100 yrs) in the forward direction, and 1°/400 yrs (1°/80 yrs − 1°/100 yrs) during the backwards motion; this then we can call variable trepidation.

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[23] Neugebauer's translation, *HAMA*, 2: 632.

[24] Neugebauer calls it a "substitute...for Ptolemy's precession" (*HAMA*, 2: 633).

[25] Thābit, however, in his letter to Isḥāq apparently also considered trepidation a substitute for precession (see below).

[26] Samsó, "On the Solar Model and the Precession of the Equinoxes in the Alfonsine Zīj and its Arabic Sources," p. 178. For Ṣāᶜid and Biṭrūjī, see note 18 above. For Zarqāllu, see Millás Vallicrosa, *Estudios sobre Azarquiel*, pp. 275-276; Zarqāllu's work is "Treatise on the Motion of the Fixed Stars," which is extant in a Hebrew translation that Millás here translated into Spanish.

[27] Goldstein, *Biṭrūjī*, 2: 177, 179 (f. 45r-v); cf. 1: 90.

But other Islamic authors do not attribute a combination of trepidation/precession to ancient trepidation. Indeed our earliest source, Thābit ibn Qurra, tells us:

> One group mentioned by Theon (and others), whom he referred to as astrologers, believed that the ecliptic orb has a motion by which it advances 8° and then goes backwards the same amount. The amount of this motion is one degree every 80 years. From this they set forth a calculation that sometimes resulted in 4°, sometimes more, [sometimes] less. It would follow, if the situation were as they stated, that the fixed stars would sometimes be seen as stationary or retrograding.[28]

Although the interpretations of Zarqāllu and Biṭrūjī also result in stations and retrograde motions of the fixed stars, Thābit's report seems to imply a simple oscillatory motion without precession.[29]

Yet another astronomer implies that the result will not be variable trepidation but variable precession. In his *Elements of Astrology* (p. 101), Bīrūnī tells us that Theon's trepidation will cause a speeding up of the stars during accession and a slowing down during recession. What he seems to be indicating is a variable precession resulting from a combination of trepidation and a constant precession. If we take the value that he gives for the rate of precession of the "moderns," namely 1°/66 yrs,[30] and combine this with ancient trepidation, we will indeed have a variable precession (about +1°/36 yrs during accession, +1°/377 yrs during recession) rather than the variable trepidation of Zarqāllu and Biṭrūjī. The problem, of course, is that if this is what Bīrūnī has in mind, it is based upon a precessional rate not used by Theon.

Naṣīr al-Dīn al-Ṭūsī (Persia; 1201-1274) in his *Tadhkira fī ᶜilm al-hay'a* seems to be describing a similar version of trepidation, namely one that is in combination with a precessional motion and results in a variable precession that "becomes slower due to the recession...faster due to the accession." Although the passage is not entirely clear, Ṭūsī seems to be

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[28] Ibn Yūnus, *Ḥākimī Zīj*, pp. 117, 119; cf. Morelon, "Ṯābit," p. 131.

[29] We will return below to the significance of the 4°, which is not in Theon's account.

[30] Bīrūnī, *Elements of Astrology*, p. 101.

attributing this combined trepidation/precession to a "practitioner of this discipline" [i.e. astronomy] (*baᶜḍ ahl hādhā al-ᶜilm*) who learned of trepidation (*samiᶜa dhālika*), apparently in its simpler, noncombined form, from the "astrologers" (*ahl al-ṭalismāt*, literally "practitioners of the talismans"[31]), who held that the orb simply undergoes "accession and recession."[32] Though Ṭūsī does not reveal the identity of this astronomer, Theon seems unlikely since for him, at least according to Zarqāllu and Biṭrūjī who assume he is using Ptolemy's rate of precession, the result of a combined motion is variable trepidation (with different rates of speed for accession and recession) rather than a variable, unidirectional precession.

If Theon is not the person who is meant, then who did propose this theory of variable precession? My guess is that we are dealing with an early Islamic astronomer, who, as we shall see in the next section, was probably motivated by discrepancies between the Ptolemaic and Ma'mūnī (i.e. the *Mumtaḥan*) rates of precession. The attribution to Theon then becomes a retroactive reading helped along by early Islamic attempts to take such a combined motion seriously. Thus it would not be difficult for someone like Battānī to assume that this is what Theon (or, as he thought, Ptolemy) had in mind when he referred to trepidation as "additional."

C. Motivations for Trepidation before Battānī

But what can we say about this pre-Battānī period of Islamic trepidation theories? The sources could certainly be better, but I think there is enough evidence to provide a context within which to understand Battānī's discussion and criticism of trepidation.

Al-Hāshimī (ca. 890) in his *Kitāb fī ᶜilal al-zījāt* (Book of Explanations of the *zīj*es[33]), which provides quite a bit of information on the early history of Islamic astronomy, tells us that what he calls *irtifāᶜ al-falak wa-inkhifāḍuhu* (the elevation and depression of the orb) is dealt with in the works of Theon, al-Fazārī (ca. 770), Yaḥyā ibn Abī Manṣūr

[31] This is clearly a rendering of οἱ παλαιοὶ τῶν ἀποτελεσματικῶν that we find in Theon.

[32] Ragep, *Naṣīr al-Dīn*, 1: 124-125. It is likely that Ṭūsī, influenced by Bīrūnī, understood these "astrologers" to be "Babylonians" or "Chaldeans." Cf. ibid., 2: 397-398.

[33] A *zīj* is an astronomical handbook.

(d. ca. 830), Abū Maᶜshar (787-886), and Ḥabash al-Ḥāsib (ca. 850) as well as in the *Arkand*, an Indian *zīj*.[34] Now E. S. Kennedy and D. Pingree in their commentary have taken this to mean trepidation,[35] and though this is possible, especially in view of Theon's association with it, it is also possible that Hāshimī is referring to any type of ecliptic motion, including simple precession; for "elevation and depression" could simply pertain to increases and decreases in declination brought on by motion, of whatever sort, of the ecliptic orb.[36]

Though Hāshimī here, as elsewhere, leaves us with more questions than answers, we do have other sources that provide us with additional information.[37] Regarding Indian trepidation, we are told by Ṣāᶜid that a certain Muḥammad ibn Ismāᶜīl al-Tanūkhī went to India and left it with all manner of strange teachings including motion of accession and recession (i.e. trepidation). He does not give us his dates, though, and I can offer little more than a suspicion that he is early.[38] Zarqāllu also mentions that a trepidation theory, presumably similar to Theon's, could

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[34] ᶜAlī ibn Sulaymān al-Hāshimī, *The Book of the Reasons Behind Astronomical Tables* (*Kitāb fī ᶜilal al-zījāt*), f. 97r, lines 1-3.

[35] Ibid., p. 225.

[36] In ibid., f. 96v, lines 8-9, Yaḥyā is said to have "established the reason for the elevation (*irtifāᶜ*) of the orb" and further it is claimed that the "observation(s) differed [from Ptolemy's?] because of the elevation and depression of the orb (*irtifāᶜ al-falak wa-inkhifāḍihi*). In their commentary (p. 225), Kennedy and Pingree suggest that this may "refer to the model, known from Thābit ibn Qurra's *De motu octave spere*." But this cannot be correct since this model dates from at least a century after Yaḥyā (see note 4 above). Though some simple trepidation model may be at work here, it seems just as reasonable to understand Yaḥyā's explanation of the difference in observations as being based upon a monotonic precession. It should also be noted that Yaḥyā does not seem to be associated with trepidation in any other source; see Ragep, *Naṣīr al-Dīn*, 2: 400, esp. n. 29.

[37] I have, however, been unable to find anything regarding trepidation for either Yaḥyā or Fazārī.

[38] Ṣāᶜid al-Andalusī, *Ṭabaqāt*, p. 56 (Cheikho ed.); p. 144 (Bū-ᶜAlwān ed.), p. 112 (Blachère trans.); cf. virtually the same report in Qifṭī, *Ta'rīkh al-ḥukamā'*, p. 281. See also Millás, *Estudios sobre Azarquiel*, p. 320.

be found in the *Sindhind*.[39]

As for Abū Maᶜshar, Zarqāllu informs us that one may also find a trepidation theory similar to that of Theon in Abū Maᶜshar's "Tables." Exactly what work is meant is not clear,[40] but there is a discussion of ancient trepidation in Abū Maᶜshar's *Kitāb al-qirānāt* (Book of Conjunctions).[41] There he even gives an example for Year 265 of Yazdegerd.[42] Zarqāllu, however, tells us that though Abū Maᶜshar put trepidation in his "Table," he also offered doubts concerning it and, unlike Theon, did not combine it with precession.[43] This seems in line with the comments of both Duhem and Millás, who note that the eclectic Abū Maᶜshar also gives Ptolemy's precessional rate but does not explain how or whether to combine it with trepidation.[44]

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[39] Millás, *Estudios sobre Azarquiel*, p. 275; it is possible that Zarqāllu here has Ḥabash's *Sindhind zīj* in mind, which is mentioned by Ṣāᶜid (see below). For information on early Indian theories of trepidation, see D. Pingree, "Precession and Trepidation in Indian Astronomy before A.D. 1200." What role, if any, Indian trepidation played in Islamic astronomy has yet to be explored.

[40] Ṣāᶜid, an associate of and source for Zarqāllu, lists 2 *zījes* for Abū Maᶜshar (*al-kabīr* and *al-ṣaghīr*, the latter also known as *Zīj al-qirānāt*) [*Ṭabaqāt*, p. 57 (Cheikho ed.), p. 145 (Bū-ᶜAlwān ed.), p. 113 (Blachère trans.)]. The former would seem to correspond with *Zīj al-hazārāt* (Pingree, "Abū Maᶜshar," p. 36 [no. 3]) and the latter to no. 30 of ibid., p. 38. Cf. Pingree, *The Thousands of Abū Maᶜshar*, p. 2, n. 3.

[41] I have not seen the Arabic text, which is extant (Sezgin, *GAS*, 7: 146-147 [no. 15] and Pingree, "Abū Maᶜshar," p. 36 [no. 8]). The Latin translation by John of Seville was printed in 1489 and 1515. Duhem (*Système du monde*, 2: 503-504) partially translated into French the section on trepidation from the Latin, which occurs in Tract. II, differentia 8ª. I am not sure of the relation of this work to the *Zīj al-qirānāt* mentioned in the previous note.

[42] Unfortunately the example doesn't quite work since he comes up with 5°12′45″ for motion of accession and this should have occurred in 269 Yazdegerd (= +900 Julian). Since the amount of motion results in an integral number of years (417), I suspect that the error is in the year (265 Yazdegerd) that we find in the Latin text. Curiously, Abū Maᶜshar would not have been alive for either date, at least according to our sources.

[43] Millás, *Estudios sobre Azarquiel*, p. 276. This, at least, is what I think Zarqāllu is getting at though I am not at all certain.

[44] Duhem, *Système du monde*, 2: 504; Millás, *Estudios sobre Azarquiel*, p. 276, notes 1 and 4.

The last person on Hāshimī's list I would like to turn to is Ḥabash. Ṣāᶜid tells us that Ḥabash used "the motion of accession and recession of the ecliptic orb" in his first *zīj* (i.e. the one that followed the methods of the *Sindhind*) "according to the theory of Theon in order to correct the position of the stars in longitude."[45] One gets the sense from Ṣāᶜid's account that this was unusual since he says that this was one of the things distinguishing this early *zīj* (the first of three attributed to him) from those of Fazārī and Khwārazmī.[46] Now Zarqāllu mentions a work called the "*al-ᶜAlūmī* Tables," which Millás takes to be Ḥabash's *al-Mumtaḥan zīj*.[47] But if *al-Mumtaḥan* were meant, one would assume that the Hebrew translator would have transcribed it as such, which is actually done later in the treatise.[48] Could this then be another of Ḥabash's *zīj*es, perhaps the *Sindhind zīj* mentioned by Ṣāᶜid? This seems to me a real possibility, especially in view of the close association of Ṣāᶜid and Zarqāllu. At any rate, Zarqāllu tells us that in this *ᶜAlūmī* text the quantity of motion for trepidation was distinct from that of other authors.[49] The important point then is that we are dealing with an early attempt to modify ancient trepidation for some reason or other.

What were the motivations for these early Islamic forays into trepidation theory? For Abū Maᶜshar, the reason seems to be mainly astrological.[50] But for Ḥabash and perhaps others, I strongly suspect

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[45] *Ṭabaqāt*, p. 54 (Cheikho ed.), pp. 140-141 (Bū-ᶜAlwān ed.; correct *fa-alafa (?) minhu* to *khālafa fīhi*), and pp. 109-110 (Blachère trans.); cf. Qifṭī, *Ta'rīkh*, p. 170.

[46] *khālafa fīhi al-Fazārī wa-'l-Khwārazmī fī ᶜāmmat al-aᶜmāl wa-istaᶜmālihi li-ḥarakat iqbāl falak al-burūj wa-idbārihi*.

[47] Millás, *Estudios sobre Azarquiel*, p. 276.

[48] Ibid., p. 338 and f. 21a. I owe this insight to my friend and colleague Tzvi Langermann.

[49] Ibid., p. 276.

[50] Trepidation, according to Abū Maᶜshar, can magnify the effect Saturn has on terrestrial events if it passes from one sign to another at the same time an 80-year period is being completed. The culmination of accession or of recession is, of course, particularly important in portending major changes in our sublunar world (Duhem, *Système du monde*, 2: 503-504).

there were actual astronomical reasons.[51] Here Thābit's testimony from his letter to Isḥāq is instructive. He states that he wishes to attribute the differences between the *Mumtaḥan* results (those in the *zīj* of Yaḥyā ibn Abī Manṣūr?) and those of Ptolemy to a generalized phenomenon affecting all the stars (*shay' ᶜāmm fī jamīᶜ al-kawākib*);[52] as such he suggests that trepidation, as reported by Theon and others, could be a possible cause since it is a motion of the ecliptic orb, which, presumably, he understands as affecting all the motions below it in addition to all the fixed stars. Now one important difference between Ptolemy and the Ma'mūn astronomers (ca. 830) was the rate of precession; the former had a value of 1°/100 yrs while the latter's was 1°/66 yrs. Over the 700 years or so separating them, the different rates would yield a difference in prediction of 3.6°. It is certainly interesting and suggestive that Thābit in his letter uses the puzzling 4° as the amount predicted by trepidation and then adds that sometimes it is "more, [sometimes] less."[53] Of course, this is the medial value, but I strongly suspect that there is more involved. Thābit (or perhaps someone else) has probably tried to use trepidation to account for the 3.6° discrepancy (nearly 4° by the end of the ninth century). Now with the traditional dates (recession beginning in −157 Julian, accession +483 Julian)[54] simple trepidation would bring the stars at the time of Ma'mūn back to approximately the same positions they had at the time of Ptolemy. But by changing the beginning of an accessional period to −11 Julian and the beginning of recession to +629 Julian, the stars at the time of Ma'mūn would have computed positions 3.6° ahead of where they would have been by simply using a precessional rate of 1°/100 yrs (see Figure 3). Although I have no actual evidence that anyone reconfigured trepidation in this way, it would certainly be consistent with the type of experimentation needed if someone (such as Ḥabash or the author of the

[51] That trepidation was transformed from an astrological to an astronomical theory also seems to be the point Ṭūsī is making in his comments discussed above.

[52] Ibn Yūnus, *Ḥākimī Zīj*, pp. 114-117; Morelon, Ṯābit," p. 131.

[53] See above, Section IV.B.

[54] See Figure 2; note that the Julian dates are 30 years less than the years of Augustus shown in the figure.

ᶜAlūmī tables) were to try to use trepidation to explain the discrepancies between the Ma'mūn astronomers and Ptolemy.

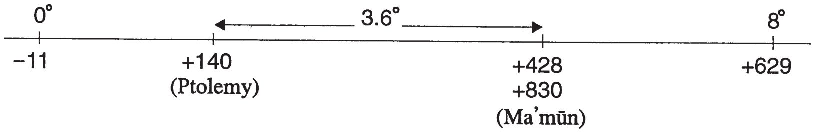

Figure 3

To his credit, Thābit was not ready to commit himself to trepidation without further observations.[55] Indeed one of Thābit's reasons for writing Isḥāq was to ascertain if he knew of any observations of the sun between Ptolemy and their own time. Presumably he would then have been able to test whether a revised trepidation model such as we have outlined was consistent with such an observation. Now this choice of observation is an interesting one since, as we shall see, solar observations (or more precisely year-lengths) are precisely the type of observation used by Battānī in his own analysis.

Let us now summarize what we can say about the situation of trepidation in Islam before Battānī. First the ancient theory of trepidation, which according to Theon's description was strictly mathematical, has been transformed in Islamic accounts into a model in which the motion is brought about by one or more physical bodies. Second it has also been interpreted by a number of sources as being additional to precession, resulting in either variable trepidation or variable precession. Third there is evidence that one or more ninth-century astronomers used (or experimented with) combinations of simple trepidation and precession to account for the discrepancies between predicted values of Ptolemy and Ma'mūn's astronomers. Finally there is no evidence for the eighth or ninth centuries of any sort of complex trepidation or variable precession models of the type we encounter in the tenth and later centuries (e.g. in

[55] As he says, "the reason for this error (*ghalaṭ*, i.e. between *Mumtaḥan* and Ptolemy) is obscure."

De motu octave spere).

It is with this background in mind that we now turn to Battānī for his criticism of trepidation and his suggested alternative.

V. *Battānī's criticism of ancient trepidation*

It has been known for some time that Islamic astronomers were not hesitant to criticize their ancient predecessors, and Ptolemy was a favorite target. These objections were of several types and included criticisms of observations, mathematical models, and the physical cosmology resulting from those models.[56] It has become increasingly clear that far from being a manifestation of later Islamic astronomy (often associated with the so-called "Marāgha school"), they began quite early.[57] Battānī's critique of the physical and observational basis of ancient trepidation, as well as his raising the possibility of instrumental error in the observations of the ancients, should also be understood as part of this tradition of early Islamic criticism.

Battānī's first objection to Theon's trepidation model (at least as Battānī has interpreted it) is that it cannot be given a physical model (*lā yatahayya'u*, literally "it cannot be structured").[58] The problem here is that a single orb would be required to have two independent motions, namely trepidation and precession, and that during the period of recession the two motions would be in opposite directions. Battānī notes that this is impossible unless one were to propose another mover for the ecliptic orb or unless one were to have the fixed stars somehow move upon (*ʿalā*) the

[56] For a brief discussion of these criticisms, see Ragep, *Naṣīr al-Dīn*, 1: 48-51.

[57] One quite remarkable example can be found in the early ninth-century work "On the Solar Year," which has been edited, translated, and commented on by R. Morelon in his *Thābit ibn Qurra: Œuvres d'astronomie*, esp. p. 61. (Note that Morelon argues convincingly that this work is not by Thābit, but it does date from the ninth century.) See also Morelon, "Ṯābit b. Qurra," esp. pp. 127-129, and G. Saliba, "Early Arabic Critique of Ptolemaic Cosmology: A Ninth-century Text on the Motion of the Celestial Spheres" (cf. Ragep, *Naṣīr al-Dīn*, 2: 389-390).

[58] Nos. [4]-[5] in the *Appendix*. It is interesting to note that the word he uses here has the same root as *hay'a*, which came to mean specifically scientific cosmography and more generally astronomy; see Ragep, *Naṣīr al-Dīn*, 1: 33-41.

ecliptic. (Presumably he means move upon the surface of the ecliptic orb while the ecliptic performed its motion of trepidation; I doubt that he means that one consider this a serious alternative since stars and planets cannot simply move on their own in ancient and medieval cosmology but need celestial bodies as movers.) What is somewhat surprising is that he does not make an explicit objection to the fact that the orb would be required to change directions, thus violating the cardinal requirement that celestial motion be uniform; perhaps he thought the absurdity of an orb moving in opposite directions simultaneously was criticism enough.[59]

Table 2
Reported Values

	Yrs. since Nabonassar (Julian Yr.)	Length of Tropical Year (days)	Motion of Sun per Egyptian Yr.
Babylonians	0 (−746)	365¼ + $^1/_{120}$ (=365;15,30)*	359°44′43″
Hipparchus	600 (−146)	365¼ (=365;15)	359°45′13″
Ptolemy	885 (+139)	365¼ − $^1/_{300}$ (=365;14,48)	359°45′25″
Battānī	1628 (+882)	365¼ − ($3^2/_5$)/360 (=365;14,26)	359°45′46″

*Battānī takes this to be a sidereal year in Chapter 27 (3: 61, 1: 40).

In addition to criticizing trepidation on physical/cosmological grounds, Battānī uses the available observational data (in the form of tropical year-lengths) to undermine this ancient form of trepidation.[60] But as we shall see, even here cosmological considerations play an important role. Table 2 contains a summary of this material (nos. [12]-[20]). His main argument is that there is no pattern in this data that would support trepidation since the tropical year-lengths over a long period of time (more

[59] One might wish to read sentence no. [5] to imply a criticism based on uniformity, but the addition of *maʿan* (simultaneously) would seem directed at the problem of having the ecliptic orb move in opposite directions due to precession and recession. That Battānī does not suggest getting around this problem by simply combining the motions reflects an insistence on having individual motions be represented by separate movers. For another example of this, see Ragep, *Naṣīr al-Dīn*, 2: 439.

[60] Nos. [12]-[23] in the *Appendix*.

than 1,600 years if one takes the Babylonian data seriously) do not show a variability in the tropical year that is predicted by this theory of trepidation (nos. [21]-[22]). His specific example is that in about 300 years, Ptolemy corrected Hipparchus's value by about 1 day while he corrected Ptolemy by 4¼ days in about 750 years (no. [23]).[61] What I think he is getting at here is that according to his interpretation of the ancient theory of trepidation, the tropical year should be constant for the 640 years of accession and also constant, but longer, during the 640 years of recession. (This will be irrespective of whether or not there is also a constant precession taking place.) To see this, we can refer to Figures 4 and 5. In the tradition of Islamic mathematical cosmology (*hay'a*), generally one uses a ninth orb above the eighth orb of fixed stars as a fixed system of reference with the vernal equinox marking the zero point (Figure 4).[62] The motion of precession in the eighth orb would then take place against this fixed coordinate system. But the eighth orb also moves everything below it (as noted by Thābit in his letter) and therefore the entire system of solar orbs will move with whatever motion the eighth orbhas (Figure 5);[63] from a calculation standpoint, this means that the solar apogee is sidereally fixed (assuming that it does not have an independent motion) and thus a constant precession will lead to a constant

[61] Battānī's correction of Ptolemy is actually closer to 4½ days. Ptolemy tells us in the *Almagest* that he did not correct but rather accepted Hipparchus's value for the tropical year, and Battānī admits in at least two places in his *Zīj* that he knows that a value less than 365¼ days was attributed to Hipparchus. His claim here is therefore quite puzzling; see note 76 below.

[62] Ragep, *Naṣīr al-Dīn*, 1: 110-117. The ninth orb was also responsible for the daily motion. There was not unanimity, however, concerning the need for such an orb; see ibid., 2: 389-390, and Saliba, "Early Arabic Critique."

[63] This is the point of the author of "On the Solar Year," for which see Morelon, *Thābit ibn Qurra*, pp. 27-67.

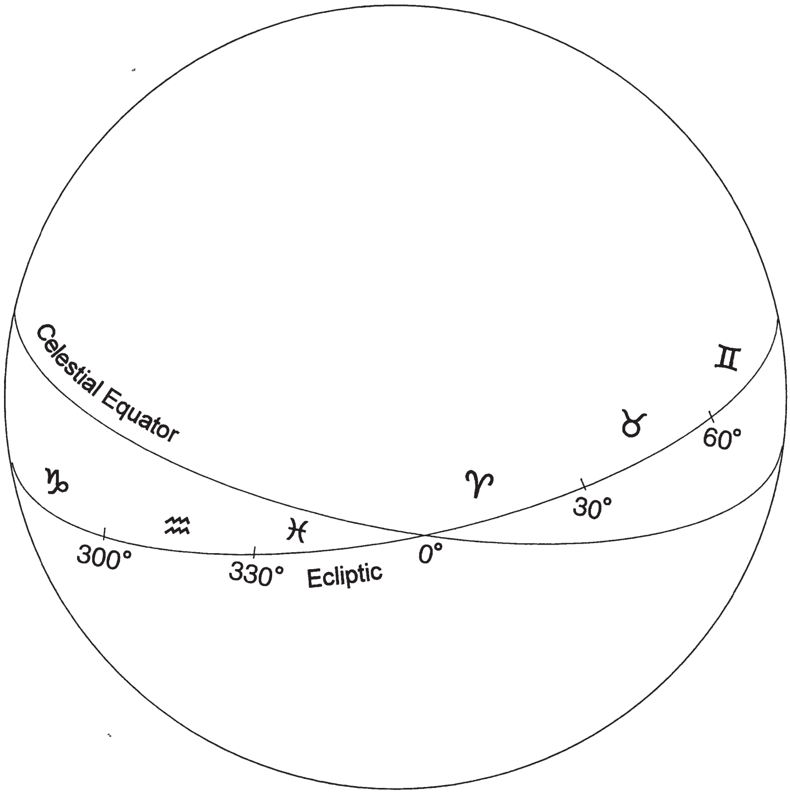

Figure 4. The 9th Orb

tropical year-length.[64] But a constant accession or recession (whether or not combined with a constant precessional term) will also lead to constant tropical years during each period of motion. Thus if Battānī's version of ancient trepidation were a reality, then one would expect a constant

[64] Strictly speaking, this is not correct as is pointed out by the author of "On the Solar Year." But the discrepancy, brought on by the difference between precession in the eighth orb and the resulting arc on the solar eccentric, is not large enough to be a factor for the differences in tropical years with which we are here concerned; see Morelon, *Thābit ibn Qurra*, pp. 59-61, 208-210.

tropical year-length during accession, and a constant, but longer, year-length during recession; instead, as we read Battānī's data in Table 2, we find that there is a steadily decreasing year-length over time. Thus Battānī feels that he can reject this ancient trepidation on both cosmological and observational grounds.

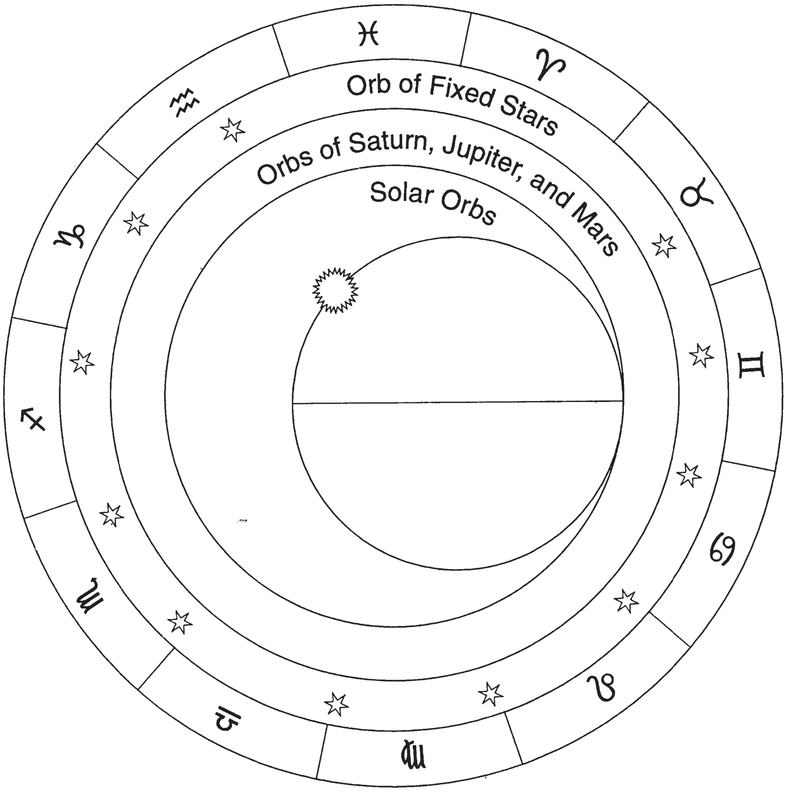

Figure 5

VI. *Battānī's alternative to ancient trepidation*

But then how does one interpret the data that would seem to indicate a steadily decreasing tropical year-length? Battānī offers two suggestions. The first (no. [24]) is that this could be due to observational error. If Ptolemy's and Hipparchus's figures had been skewed due to instruments of poor workmanship or due to warping occurring in them over time, then observers during a later period (presumably with better instruments) would correct their values and this would account for the differences. On the other hand, this difference could be due to a motion in the orb. If I understand his point in nos. [25]-[26], he is saying that the motion is so minute that its true character and amount could not be discovered during a single time period but needs longer spans so that comparisons can be made. If this interpretation is correct, then he is leaving open the possibility of discovering some variable precession that might account for these differences. This is an important point, I believe, that will help us understand the last part of this chapter.

Battānī ends with a discussion of precession. Why? He basically repeats the point that Thābit made in his letter, which is fundamental for understanding the cosmological basis for relating year-lengths and precession, namely that any increase in speed affecting all the stars and planets (including the sun) must be due to a motion of the ecliptic orb. We can remind ourselves of what he has in mind by looking again at Table 2, where decreasing year-lengths are associated with increasing solar speeds. Now what he is saying in nos. [31] and [32] is that the increase in precession can account for the increased speed in all the celestial motions, in particular that of the sun. Though he does not go into detail, I believe that we can do some simple calculations that should make the basis for his remarks clear. If one takes the difference in precessional rates of Ptolemy and Battānī, which comes to about 18.55″/yr, and then one divides that over the 743 years separating them, one comes up with around 1.5‴/yr, which represents the average yearly increase in the rate of precession from Ptolemy's time to Battānī's. Now if one starts with Battānī's rate of precession and calculates earlier precessional rates based on the assumption of a steady decrease, one can then also find solar motions and year-lengths for earlier periods. This is done in Table 3. To say the least, these results are suggestive when one compares them with the reported data of Table 2. The problem is that this also suggests that the ecliptic orb is steadily increasing its speed which, from a cosmological perspective that demands uniform circular motion, is unacceptable. I believe that this is why Battānī leaves the matter unresolved, but this problem, as well as his analysis, would provide a starting point for his successors.

Table 3
Calculated Values According to Battānī's Variable Precession

	Precession 1°/yrs*	Precession seconds/yr†	Tropical Yr. (days)†	Motion of Sun per Egyptian Yr.†
Babylonians	1°/261 yrs	14″/yr	365;15,8	359°45′5″
			(365;15,22 = sid. yr.)	
Hipparchus	1°/125 yrs	29″/yr	365;14,53	359°45′20″
Ptolemy	1°/100 yrs	36″/yr	365;14,45	359°45′27½″
Battānī	1°/66 yrs	54½″/yr	365;14,26	359°45′46″

*Rounded to the nearest year.
†In general, rounded to the nearest second.

VII. *The significance of Battānī's proposed alternative*

One of these successors seems to be Ibrāhīm ibn Sinān, who, in his *Kitāb fī ḥarakāt al-shams*, makes explicit the relationship between a variable precession (brought on by an additional orb that moves the ecliptic) and variable tropical years.[65] But the importance of Battānī is made more obvious in the *De motu octave spere*. For there the author, in the context of a discussion of precessional rates and tropical year-lengths, paraphrases nos. [22] and [25] in which Battānī states that the increase in solar motion does not proceed according to a slowing down and speeding up and then seems to be calling for more observation;[66] but then the author of *De motu* states:

> Then, after this statement of his [i.e. nos. [22] and [25]], he returned to what was more proper and suitable and more like the method of evaluation, in order that his [al-Battānī's] procedure might be good and

[65] For a discussion of Ibrāhīm's model, see Ragep, Naṣīr al-Dīn, 2: 402-405.

[66] Millás, *Estudios sobre Azarquiel*, pp. 500-501, and Neugebauer, "On the Motion of the Eighth Sphere," p. 294. Cf. Ragep, *Naṣīr al-Dīn*, 2: 401.

> that he himself might have perfected something.[67]

In short, the author of *De motu* seems to understand that Battānī is doing something that is admirable following his statement casting doubt on a motion of trepidation. Considering that *De motu* puts forth a theory of variable precession, it does not seem at all farfetched to understand its author as praising Battānī's explanation of variable year-lengths as due to an increasing rate of precession. Biṭrūjī in his *Kitāb fī al-hāy'a* also seems to confirm our interpretation since he says that:

> Battānī showed that the fixed stars move away from the vernal equinox point with different speeds in equal times. He then left [reading the variant *taraka* instead of *nazala*] the matter there.[68]

Thus it seems very likely that Islamic astronomers understood that Battānī was suggesting, though not advocating, variable precession as a way to explain the differences between the Ptolemaic and ninth-century observations.

VIII. *Conclusion*

There are several points that we can make regarding Battānī's chapter on trepidation. First, I think that it served as an important starting point for later investigations into variable precession; I am convinced that Battānī's influence extended to his younger contemporary and countryman Ibrāhīm ibn Sinān, whose work was seminal for later developments in

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[67] "Deinde reuersus est post hunc suum sermonem ad id quod dignius fuit et conuenientius et similius secundum semitam estimationis, ut bona foret eius operatio et quod ipse aliquid perfecisset" (Millás, *Estudios sobre Azarquiel*, p. 501). I owe the translation, for which I am most grateful, to Gerald Toomer; it corrects Neugebauer's in "On the Motion of the Eighth Sphere," p. 294.

[68] Goldstein, *Biṭrūjī*, 2: 179 (f. 45v); cf. 1: 91. As Goldstein notes (p. 23), Zarqāllu may well be the source for Biṭrūjī's remarks; see his "Treatise on the Fixed Stars" in Millás, *Estudios sobre Azarquiel*, pp. 276-277, where Zarqāllu has virtually the same report on Battānī's variable precession.

Islamic trepidation.[69] Second, and no less important, are the lessons we can learn regarding the relationship of physical cosmology and mathematical astronomy. It is customary, both for modern historians and medieval cataloguers, to differentiate the *zīj*es (astronomical handbooks) from *hay'a* (cosmographical) works. The former are usually taken to be mainly computational and practical, while the latter are understood to be physical (or "philosophical") and theoretical in their orientation. Though this categorization has its place, it would be a mistake to understand it as implying different philosophical commitments regarding the nature of astronomy.

Battānī is a good example of someone often taken to be in the "computational" camp, but, as we have seen, Battānī weaves cosmology, mathematics, and observation into a virtually seamless web; the observations are placed within the framework of a physical cosmography without ado, and the mathematical analysis makes numerous cosmological assumptions. It is also worth noting here that in other parts of his *Zīj* he takes up matters usually considered to be part of *hay'a*. In Chapter 50, he deals with the "distances of the stars and their diameters, their volumes (*ᶜuẓm ajrāmihā*), and the width (or extent: *saᶜa*) of their orbs, and their discussion (*dhikr*) presented according to what the ancients (*al-qudamā' wa-'l-awā'il*) have stated." Sizes and distances, of course, is a fundamental part of ancient and medieval cosmology. Battānī also gives himself leave to discuss the nature of the fifth element.[70] These forays of a "practical" astronomer into theory should not surprise us since, after all, he was working within the framework of Ptolemaic astronomy, which was based upon both physical and mathematical principles. Islamic astronomers, however, took this dual aspect much more seriously than did their ancient predecessors;[71] the effects of this attitude can be seen in the "mathematical astronomy" of someone like Battānī. Taking these into account can, I am convinced, yield valuable insights into the development of Islamic astronomy.

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[69] I hope to return to this matter in a future publication.

[70] Nallino, *Battānī*, 3: 182, lines 7-8 (Arabic text), 1: 120 (Latin trans.).

[71] For a preliminary discussion of this point, see Ragep, *Naṣīr al-Dīn*, 1: 46-48, which has references to other works dealing with this issue.

Appendix

Translation of Chapter Fifty-Two of Battānī's *Zīj* (1: 126; 3: 190)[72]

On Understanding What the Masters of Talismans Have Stated, Namely That the Orb Has a Shifting Motion of Accession and Recession, and What Is Clearly Their Error

[1] Ptolemy stated in his book that the masters of Talismans claimed that the orb has a slow, shifting motion over time, being one degree in every 80 years. [2] They have stated that this motion, culminating at 8°, accedes and then recedes. [3] The meaning (1: 127) of their words is that the ecliptic orb moves 8° from west to east with the motion of the orb of the fixed stars that is also in this direction and then also moves 8° from east to west, this being opposite the first motion. [4] Despite this it must also move with the motion of the fixed stars[73] that is from west to east; but this cannot be nor can [the orb] be structured (*lā yatahayya'u*) unless something else moves it or unless the fixed stars themselves move upon it. [5] This is because a single body cannot move simultaneously with two motions in opposite directions.

[6] They have stated that the culmination of the accession occurred before King Augustus by 128 Egyptian years, this being the 166th year of Alexander the Macedonian. [7] For determinations after that time, one calculates a degree for every 80 years; the result of this, until it reaches 8°, [is subtracted from 8°].[74] [8] The remainder is then added to the (3: 191) forward motion of the stars. [9] When 8 is reached, there is no longer [a remainder (*al-baqiyya*)?]. [10] For amounts exceeding 8, [the

[72] Numbers in parentheses refer to Nallino's text (vol. 3) and translation (vol. 1).

[73] Deleting *al-ḥaraka al-ūlā* (the first motion), which is both awkward and not very meaningful. It may have been a marginal gloss meant to explain that the fixed stars are in the direction of the accessional ("first") motion, or it may be simply a copyist's mistake (repeating the previous occurrence of this phrase in no. [3]). This seems to be the phrase that led Duhem astray in his interpretation by which he denied that a combined precession and trepidation is meant (*Système*, 2: 231); cf. note 12 above.

[74] Something is clearly missing from this sentence, and Nallino has added *yunqaṣ* (is subtracted), but one also needs what "the result" is subtracted from.

excess] itself is taken and this is then added to the positions of the stars until [the excess] reaches 8. [11] Thereupon the initial situation is reverted back to.

[12] The period of the year that those who held this doctrine worked with was greater than 365¼ days by the amount of $^1/_5$ of an hour, approximately.[75] [13] The mean motion of the sun will therefore come to 359°44′43″ in an Egyptian year. [14] As for Hipparchus, he came after these [people] and operated with a year whose period was 365 days and one-quarter only.[76] [15] The motion of the sun in the period of an Egyptian year would then be 359°45′13″. [16] And it was claimed that he had taken the position that it was less than a quarter-day.[76] [17] Then Ptolemy observed after Hipparchus by 285 years, and he found the period of a year that he worked with to be 365 days and less than a quarter-day by one part in 300. [18] According to that, the motion of the sun in an Egyptian year would be 359°45′25″. [19] We ourselves have made observations 743 years after Ptolemy and have found the period of the year to be 365 days and less than a quarter-day by $3^2/_5$ parts of 360. [20] According to this, the motion of the sun in an Egyptian year then becomes 359°45′46″.

[21] All of these motions have been increasing since the time of Nabonassar [Bukhtanaṣṣar], or thereabouts; therefore this that they have described [concerning year-lengths] is incapable of conforming to anything they [i.e. the originators of trepidation] have stated concerning the amount

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[75] In Chapter 27 (3: 61, 1: 40), Battānī attributes this same year-length (365¼ + $^1/_{120}$ days) to the "Egyptians and Babylonians" (*ahl Miṣr wa-Bābil*), who he evidently thinks are the source of ancient trepidation. He also thinks this is the sidereal year Ptolemy is complaining about in the *Almagest* III.1, p. 132. Neugebauer (*HAMA*, 1: 530) does not think much of this "Babylonian" attribution. (He apparently means al-Battānī rather than al-Bīrūnī here.) Cf. Nallino, *Battānī*, 1: 204-209, 304.

[76] These are curious statements (nos. [14] and [16]) since Battānī shows in Chapter 27 (3: 61, 1: 40) that he knows Hipparchus held that the year was "in truth" (*bi-'l-ḥaqīqa*) less than 365¼ days and paraphrases *Almagest* III.1, p. 139 [H207] to this effect. However here he repeats what he also states in Chapter 27, namely that Hipparchus "worked with" (*ᶜamila ᶜalā*) a 365¼-day year. This may be a reference to remarks made by Ptolemy, who reports that Hipparchus said that his observations were in agreement with a 365¼-day year (p. 134), but I am far from certain. It is noteworthy that Battānī needs Hipparchus to have a longer year than Ptolemy (rather than the same length of 365¼ − $^1/_{300}$ days as is usually understood from Ptolemy's remarks) in order to make his point in nos. [21] and [23] below.

of the parts [or degrees], the magnitude of the motion, or the increase and decrease. [22] Rather we find this increase not to be according to a slowing down and speeding up. [23] For Ptolemy corrected Hipparchus in some 300 years approximately a day and we ourselves have corrected Ptolemy in some 750 years by the amount of 4¼ days besides the day with which he had corrected Hipparchus.

[24] If this increase had, though, arisen due to an error in the observing instruments on account of their divisions or a change in them over time, then necessarily it will enter into our observations after some period of time since comparisons made in our observations are [with respect] to those observations. [25] [But] if that were due to a motion in the orb, its reality would not be apparent to us and neither we nor someone from among the ancients would have been able to ascertain it; for the search and pursuit (reading *ittibāᶜ*) of true reality requires that observing be done during each time period and then whatever is found and corrected will be improved just as it was improved in earlier times.

[26] That upon which there is doubt will call for (reading *fa-yūjibuhu*) measurement [or comparison]. [27] Since this increase includes [or comprehends] the totality of motions of all the stars, that will occur due to the motion of the orb of the fixed stars. [28] Concerning that Ptolemy stated that this motion, inasmuch as he found it with his observations and according (3: 192) to what was done prior to him as well, is one degree in every 100 years. (1: 128) [29] But between the observations of Ptolemy and those he compared them with, there was not enough time to cause an obvious change to appear in this type of motion. [30] This is because between the observation that he made and that with which he made his comparison was only about 200 years.[77] [31] But since the time between him and us is longer, an increase in this motion has become apparent so that it was found to be one degree in every 66 solar years. [32] It is on account of the difference between these two speeds that there has occurred the increase in all [the motions].

[77] Actually 285 years.

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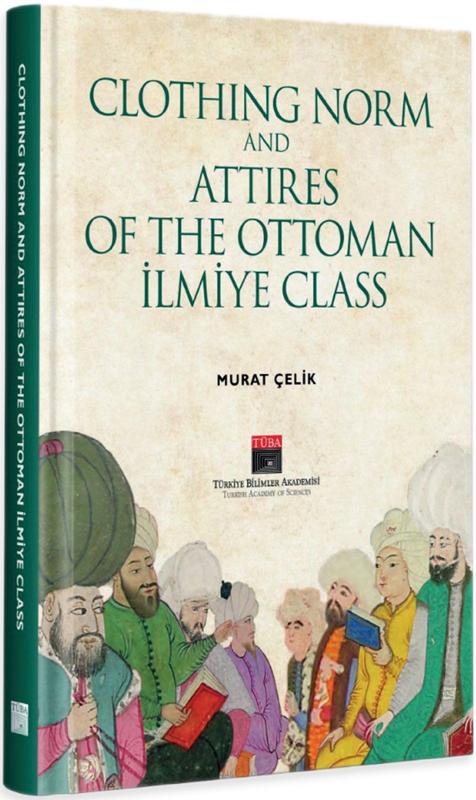
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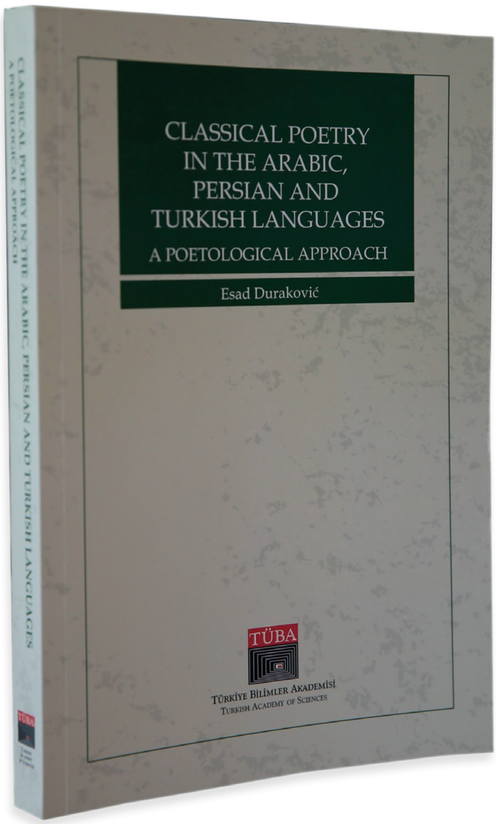
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Türkiye Bilimler Akademisi
Turkish Academy of Sciences
Vedat Dalokay Caddesi No: 112, 06670 Çankaya/ANKARA
Tel: +90 (312) 442 29 03 Faks: +90 (312) 442 72 36

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