The Scientific Revolution and the Transmission Problem

Abstract
Recent dialogical histories of science propose that the Scientific Revolution of the seventeenth century was shaped by contributions from different astronomical traditions of the Eurasian region – especially the Maragha School of Arabic astronomy, the Chinese infinite empty space cosmology and the Indian Kerala School of astronomy. Such narratives are based on many discoveries in these traditions which antedate similar discoveries made in Europe during the Scientific Revolution. These views have generated intense objections from critics of the dialogical perspective who maintain independent discovery in Europe of these parallel achievements by repudiating claims for transmission as lacking documentary evidence or acknowledgment. This paper explores these debates using transmissions from the Maragha tradition as a case study. It proposes that a plausible case for transmissions can be made on the basis of circumstantial evidence even in the absence of direct documentary evidence.

Keywords
Maragha School, Scientific Revolution, transmission problem, Copernicus, dialogical histories.

1 Toward Dialogical Histories

In recent years there has emerged an increasing interest in articulating dialogical histories of modern science motivated to a large extent by the turn to global histories of the modern world that look at how interactive processes across civilizations mediated by trade, religion and trans-regional empires have shaped local, regional civilizations and intra-national developments.[1] Modern science itself, although it developed largely within Europe, has also been seen as influenced by the global linkages in which much geographical, medical, botanical, and technological knowledge exchanges took place between wide geographical regions of the world. However, the Scientific Revolution which played such a critical role in the emergence of the modern world has often been seen as a phenomenon largely insulated by such global dialogues since most historians see it as built on the achievements of ancient and early modern Europeans alone.

However, there is an increasing number of writers who have proposed that non-European contributions played a seminal role in the Scientific Revolution, especially the astronomical revolution associated with Copernicus, Kepler, Galileo and Newton. The pioneer in this area is Joseph Needham who documented the significant contributions made by Chinese cosmology, especially the importance of the Chinese infinite empty space theory in transforming European thinking from the closed world of the medieval era. Paralleling these studies is George Gheverghese Joseph's efforts to identify and delineate the impact of the mathematical discoveries of the Kerala School of Indian astronomy on the Scientific Revolution. At the same time George Saliba has emphasised the role that the Arabic Maragha School of astronomy played in developing mathematical techniques and tools that rendered possible the Copernican revolution. In his study *The Dialogue of Civilisations in the Birth of Modern Science* Arun Bala shows how Chinese, Indian and Arabic traditions of astronomy came to be integrated within the Scientific Revolution that led to modern science.[2]

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[1] For an overview of recent surveys of global historical approaches see the essays written by leading international scholars in P. Duara, V. Murthy and A. Sartori (eds.), *A Companion to Global Historical Thought,* Wiley Blackwell, 2014.

[2] See: J. Needham, *Chinese Astronomy and the Jesuit Mission: An Encounter of Cultures,* London: The China Society, 1958; G. Saliba, *Islamic Science and the Making of the European Renaissance,* Cambridge, Mass.: The MIT Press, 2007; G. G. Joseph, *The Crest of the Peacock: Non-European Roots of Mathematics,* Princeton: Princeton University Press, 2011; A. Bala, *The Dialogue of Civilizations in the Birth of Modern Science,* New York: Palgrave Macmillan, 2006. These dialogical histories have begun to put into question orthodox histories concerned with identifying influences only within Europe that came to impact the Scientific Revolution. By contrast: H. Butterfield, *The Origins of Modern Science 1300–1800,* New York: The Free Press, 1957; R. Hall, *The Scientific Revolution 1500–1800: The Formation of the Modern Scientific Attitude,* London: Longmans, 1954; E. J. Dijksterhuis, *The Mechanization of the*

These dialogical historians of the Scientific Revolution demand that we need to radically revise our notions of the historical and geographical contexts of the event. Historically they regard the period from 500 to 1500, often seen as the dark ages of science in Europe[3], as a time of great advances in astronomy in the Arabic, Chinese and Indian civilisations. Paralleling these differences in historical understanding is a different geographical conception. In contrast to histories that see this geography as ranging only over the European continent, dialogical historians see the circulation of knowledge across the Eurasian region as integral to both comprehending and making possible the Scientific Revolution.[4]

However, the emerging dialogical approaches to the Scientific Revolution have generated intense controversy. Although some writers question the notion of a scientific revolution at the dawn of the

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World Picture, (Oxford: Oxford University Press, 1961) emphasise the ancient Greek influence; F. A. Yates, *Giordano Bruno and the Hermetic Tradition,* (Chicago: University of Chicago Press, 1964) the role of neo-Platonic hermeticism; P. Duhem, *Medieval Cosmology: Theories of Infinity, Place, Time, Void and the Plurality of World,* (R. Ariew, trans., Chicago: University of Chicago Press, 1985) and E. Grant, *The Foundations of Modern Science in the Middle Ages,* (Cambridge: Cambridge University Press, 1996) the impact of medieval scholasticism. The recent study by R. S. Westman, *The Copernican Question: Prognostication, Skepticism and Celestial Order,* (California: University of California Press, 2011) which highlights the role that astrology played in the Scientific Revolution, continues this trend of not taking into account contributions from other civilizations.

[3] Some medieval historians disagree with this claim. See Duhem (1985) and J. Hannam, *The Genesis of Science: How the Christian Middle Ages Launched the Scientific Revolution*, Regnery Publishing, 2011.

[4] Such dialogical connections were also important in the constructions of premodern Arabic, Chinese and Indian traditions of science which cannot also be treated as purely endogenous. An early account of such connections written in the eleventh century by S. Al-Andalusi, *Science in the Medieval World: Book of the Categories of Nations,* (S. I. Salem and A. Kumar, trans., Austin: University of Texas Press, 1991) serves to emphasise this point. Maragha tradition influences on Chinese astronomy have been noted by W. Hartner, ›The Astronomical Instruments of Cha-ma-lu-ting, Their Identification, and Their Relations to the Instruments of the Observatory of Marāgha‹, *Isis,* Vol. 41, No. 2, 1950, pp. 184–194. We also have evidence for Greek impact on Indian astronomy in texts such as *Yavanajataka* by Yavanesvara (»Ruler of the Greeks«), and Varahamihira's translation of the *Paulisa Siddhanta* (»Treatise of Paul«), and the *Romaka Siddhanta* (»Treatise of the Romans«). D. Pingree, ›The Recovery of Early Greek Astronomy from India‹, *Journal for the History of Astronomy,* Vol. 7, 1976, pp. 109–123 has noted that many Greek astronomical models antedating Ptolemy can only be known through Indian translations.

modern era[5], and others trace its roots into the Scholastic age, the concept of such a radical disjuncture continues to have traction. Critics of the dialogical approach have noted the problem of establishing whether ideas, theories, techniques or practices from non-Western astronomical traditions were transmitted to Europe. Generally such critics do not dispute that the non-Western discoveries antedated those of the Scientific Revolution, but they insist that what dialogical historians often claim to be transmissions were really independent discoveries made in Europe. Hence the issue of establishing transmission or independent discovery has become central to the controversies that surround dialogical historians and their adversaries.

2 Copernicus and the Maragha Parallels

The most intense and extended debates concerning this issue are associated with the Maragha School, and serve to illustrate the epistemological concerns surrounding claims for transmission and independent discovery. Dialogical histories argue that the Maragha School anticipated many mathematical techniques and devices used by Copernicus in the formulation of his heliocentric model. Although the heliocentric theory of Copernicus was revolutionary and went far beyond Maragha School geocentrism, they deem his epistemological motives and mathematical techniques were not simply inspired by Ptolemy without the significant mediation of the Arabic tradition, *contra* the position adopted by Dijksterhuis when he writes »barring the application of trigonometric methods of computation one finds nothing in [*De Revolutionibus*] that might not just as well have been written in the second century CE by a successor of Ptolemy« (1961: 288).

Both Maragha School astronomers and Copernicus actually introduce new techniques motivated by attempts to remove the notion of an equant deployed by Ptolemy, who had introduced it as a device to make possible a more mathematically precise description of observed phenomenon. Despite their different heliocentric and geocentric conceptions of planetary motions both see Ptolemy as introducing a hypothesis that violated physical plausibility, since it made

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[5] See S. Shapin, *The Scientific Revolution*, Chicago: University of Chicago Press, 1996; Duhem (1985).

the assumption that a celestial sphere can rotate uniformly about an axis that did not pass through its centre. Indeed objections to the use of the equant were raised by the Arabic polymath Ibn al-Haytham (965–1040 CE) in his study *Doubts Concerning Ptolemy* in the eleventh century. His critique inspired Arabic astronomers to look for new mathematical techniques that would eliminate the need to deploy the equant in astronomical theory.[6]

The first step in solving the problem of the equant was the discovery of a mathematical theorem now labelled the ›Tusi couple‹ by Nasir al-Din al-Tusi (1201–1274). The theorem considers two spheres, with one having half the radius of the other and inside the larger sphere in contact with it at one point. If the larger sphere rotates about an axis, and the smaller sphere rotates at twice the speed about a parallel axis in the opposite direction, then the theorem states that the original point of contact would oscillate back and forth along a diameter of the larger sphere. The Tusi couple allowed astronomers to enlarge and shrink the size of the epicycle radius using only combinations of uniform circular motion (Saliba 2007: 158).

The second step is the discovery of the Urdi lemma by Muyyad al-Din al-Urdi (1200–1266), as a development of the Apollonius theorem. According to this lemma if we draw two lines of equal length from a given straight line that make equal angles either internally or externally with it, and connect their tops to each other, the straight line which results will be parallel to the original line. What makes the Urdi lemma so useful for planetary model building is that it allows astronomers to transform eccentric models into epicyclic ones (*ibid.*: 202).

Taken together the Tusi couple and Urdi lemma gave enough flexibility for model building so that Ibn al-Shatir (1304–1375) was able to formulate his non-Ptolemaic geocentric model of the universe without using the equant. Moreover the predictive power of his model was not inferior to that of Ptolemy's. This was the culmination of the Maragha School project for eliminating the equant begun in response to Ibn al-Haytham's critique of Ptolemy (*ibid.*: 162–165).

What is striking is that Copernicus's theory, despite the revolutionary heliocentric model it adopts in contrast to the geocentric mod-

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[6] For a comprehensive survey of these responses see Saliba (2007), especially Chapter 4. Saliba also notes that Ibn al-Haytham not only influenced the Maragha tradition but also shaped European Renaissance science (*ibid.*: 109).

el of al-Shatir, nevertheless exhibits what can be termed ›structural parallels‹ to it. Such structural parallels are very common in the history of science when new theories replace earlier ones in revolutionary situations. The philosopher of science John Worrall makes this point by illustrating it with the example of electromagnetic theory:

> There was an important element of continuity in the shift from Fresnel to Maxwell – and this was much more than a simple question of carrying over the successful empirical content into the new theory. At the same time it was rather less than a carrying over of the full theoretical content or full theoretical mechanisms (even in »approximate« form) […] There was continuity or accumulation in the shift, but the continuity is one of form or structure, not of content (Worrall 1989: 117).[7]

Such structural mathematical continuities are also exhibited by the Copernican and ibn al-Shatir models despite both being used to frame different cosmological theories. The Copernican model can be seen as mathematically similar to the al-Shatir theory once we invert the vector connecting the Earth and the Sun – i. e. make the Sun rather than the Earth the center of orbit for the planets. It is such structural continuities, similar to those that occur when one theory develops out of and displaces a predecessor, that support the claim by dialogical historians that the al-Shatir model was a geocentric predecessor of Copernicus' heliocentric theory.

Indeed both theories are motivated by the same desire to eliminate the equant and use the same theorems – the Tusi couple and the Urdi lemma – to accomplish this project. Moreover, the resemblances between the Copernican model and that of al-Shatir do not simply end here. In order to deal with the problem that Ptolemy's moon epicycles would exaggerate the moon's visual diameter, and amplify parallax effects beyond what is observed, Ibn al-Shatir had constructed a new model of lunar motion – one similar to that later deployed by Copernicus.

Ibn al-Shatir also reformed Ptolemy's model of the motion of the planet Mercury. With the exception of Mercury, he used constructions for all planets identical to that deployed by Ptolemy but with variations in the speed and size of the epicycles. However in the case

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[7] J. Worrall, ›Structural Realism: The Best of Both Worlds?‹ *Dialectica*, Vol. 43, pp. 99–124. Reprinted in D. Papineau (ed.), *The Philosophy of Science*, Oxford: Oxford University Press, pp. 139–165.

of Mercury, he applied the Tusi couple once again in the last step – a procedure that was followed by Copernicus for the same planet more than two centuries later (Saliba 2007: 163–164).

Was the Maragha School motivation to eliminate the equant and its discoveries transmitted to Copernicus? An affirmative answer to this question seems reasonable if we adopt the criterion Needham recommended for establishing transmissions of Chinese discoveries to Europe when he writes:

> Of course there may have been some degree of independence in the European advances. Even when we have good reason to believe in a transmission from China to the West we know very little of the means by which it took place. But as in all other fields of science and technology the onus of proof lies upon those who wish to maintain fully independent invention, and the longer the period elapsing between the successive appearances of a discovery or invention in two or more cultures concerned, the heavier the onus generally is (Needham 1970: 70).[8]

Needham clearly offers us a criterion for inferring transmission based on priority of discovery and the temporal interval that separates rediscovery in another culture. Adopting this criterion we would have to say that the parallels with discoveries in the Scientific Revolution earlier made by the Maragha School should be seen as also having influenced similar discoveries in Europe since they preceded rediscovery in Europe by many decades if not centuries.

Needham's approach has been used by other historians for transmissions from West to East. Van der Waerden argued that Aryabhata's trigonometry had methodological similarities with earlier Greek works and therefore must have been borrowed from them. Using a similar argument he claimed that Bhaskara II's work on Diophantine equations can be traced to an *unknown* Greek manuscript he must have had access to. It leads van der Waerden to conclude that the works of Aryabhata and Bhaskara had Greek origins since, as he put it, »in the history of science independent inventions are exceptions: the general rule is dependence« (van der Waerden 1976: 224).[9] Similarly Neugebauer appealed to »priority, accessible communication routes and methodological similarities« to demonstrate that the In-

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[8] J. Needham, *Clerks and Craftsmen in China and the West*, Cambridge: Cambridge University Press, 1970.

[9] B. van der Waerden, ›Pell's equation in Greek and Hindu Mathematics‹, *Russian Mathematical Surveys*, Vol. 31, 1976, pp. 210–225.

dian *Siddhantas* (astronomical treatises) had Greek origins (Neugebauer 1962: 166–167).[10]

However, the historian of science Floris Cohen questions the principle adopted by Needham. He argues that Needham rightly assumes that absence of evidence for transmission cannot be used to establish independent discovery. But he considers Needham to assume wrongly that in the absence of evidence against transmission we can take influence to have occurred simply on the basis that it was possible. Cohen writes:

> Although it is true that, if no transmission took place, there will necessarily be no source material to indicate transmission, the absence of source material need not prove independent rediscovery, even though it would seem to count against transmission rather than for it. Thus the two possibilities are not logically symmetrical but since one can argue endlessly over whether the affirmation that stands in need of proof is transmission or independent rediscovery without ever getting anywhere, it would seem better to seek another criterion if one wishes to say anything at all about these really quite doubtful yet highly important matters (Cohen 1994: 436).[11]

Despite recommending that we find another criterion, Cohen himself does not offer one. Instead he assumes that absence of evidence for transmission counts against transmission. Thus he disputes Needham's demand that the onus of proof lies upon those who wish to claim independent discovery and sees it as unnecessary to offer positive evidence for such discovery. This allows him to slide into a perspective of the history of the Scientific Revolution that ignores wider Eurasian contributions that made it possible. If he had made the symmetric demand that we should provide positive evidence both for transmission and for independent discovery then the absence of evidence either way would have left him in a limbo state, where he could neither articulate a history confined to European contributions nor a dialogical history accommodating wider Eurasian connections. This is further confirmed in a subsequent study by Cohen, *How Modern Science Came into the World*, where he does develop a full-fledged explanation for why the Scientific Revolution took place in Europe. Here he not only explicitly assumes that there were no *significant* Arabic influences, but also claims that we should »regard Copernicus

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[10] O. Neugebauer, *The Exact Sciences in Antiquity*, New York: Harper & Row, 1962.

[11] H. F. Cohen, *The Scientific Revolution: A Historiographical Inquiry*, Chicago: University of Chicago Press, 1994.

as Ptolemy's last and greatest heir, who throughout books II–VI of *De Revolutionibus* carried the hoary art of ›saving the phenomena‹ to new heights by means of his heliocentric, Aristarchos-inspired hypothesis« (Cohen 2011: 209).[12] Cohen then proceeds to maintain controversially that Kepler, who replaced Copernican complex epicycles with simple ellipses, was the real revolutionary and seems to overlook that Kepler himself explicitly acknowledged Copernicus in his study *Epitome Astronomiae Copernicanae.*

Indeed Cohen's asymmetric approach in dealing with establishing transmission and independent discovery is reminiscent of many historians who find it easier to claim transmission from Europe to other cultures without positive evidence, but demand such evidence when the transmission is from other cultures into Europe. This double standard, designed to work against dialogical approaches to history, has been noted by Joseph:

> [A] case for claiming the transmission of knowledge from Europe to places outside does not necessarily rest on direct documentary evidence. In certain circumstances priority, communication routes, and similarities appear to establish transmission from West to East as more plausible, on balance of probabilities, than independent discovery in the East. However, when it comes to East-to-West transmissions, there seems to be a complete change of orientation. The criterion is no longer the comparative notion of ›balance of probabilities‹ but the absolute notion of ›beyond all reasonable doubt‹. This double standard makes it possible to sustain a case for Eurocentric histories against their dialogical competitors, even in those situations where an across-the-board application of the principle of the balance of probabilities would make a stronger case for East-to-West transmissions (Joseph 2011: 437–438).

The dispute between Needham and Cohen may be formulated as follows. Needham demands, given priority and possibility of communication, strong proof for independent discovery and weak proof for transmission, that is, transmission is assumed if independent discovery cannot be established. By contrast Cohen demands strong proof for transmission and weak proof for independent discovery, that is, independent discovery is assumed if transmission cannot be established. Thus adopting Needham's approach would lead us to a Eurasia-centric dialogical history, but adopting Cohen's would guide us

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[12] H. F. Cohen, *How Modern Science Came into the World: Four Civilizations, One 17th-Century Breakthrough*, Amsterdam: Amsterdam University Press, 2011.

into a Euro-centric history that ignores the wider Eurasian connections.

Clearly we need better criteria than those assumed by Needham explicitly, or presumed by Cohen implicitly, if we are to resolve the transmission versus independent discovery debate in a rational fashion. Such criteria cannot always hope to establish either transmission or independent discovery beyond reasonable doubt. To do so we would always be required to find evidence in the form of translations or other kinds of documentary proof, or explicit acknowledgement of the influence. In the absence of such evidence the best that we can have is a balance of probabilities judgement between claims for transmission and claims for independent discovery. Such a judgement cannot, like the Needham and Cohen approaches, demand establishing a claim beyond reasonable doubt in one direction, and assume that the failure to do this allows us to make the claim in the other direction.

3 Transmission versus Independent Discovery

Arguments for transmissions from the Maragha School have been made by many writers such as Kennedy and Roberts (1959), Hartner (1973), Swerdlow and Neugebauer (1984), Ragep (2007) and Saliba (2007).[13] Contesting these transmission are those who propose independent discovery including Rosinska (1974), Bono (1995), Huff (2011), Kokowski (2012) and Blåsjö (2014).[14] Perhaps the most com-

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[13] E. S. Kennedy and V. Roberts, ›The Planetary Theory of Ibn al-Shatir‹, *Isis*, Vol. 50, 1959, pp. 227–235; W. Hartner, ›Copernicus, the Man, the Work, and its History‹, *Proceedings of the American Philosophical Society*, Vol. 117, 1973, pp. 413–422; N. M. Swerdlow, and O. Neugebauer, *Mathematical Astronomy in Copernicus's De Revolutionibus*, New York: Springer Verlag, 1984; F. J. Ragep, ›Copernicus and His Islamic Predecessors: Some Historical Remarks‹, *History of Science*, Vol. 45, 2007, pp. 65–81.

[14] G. Rosinska, ›Nasir al-Din al-Tusi and Ibn al-Shatir in Cracow?‹ *Isis*, Vol. 65, 1974, pp. 239–243; M. Di Bono, ›Copernicus, Amico, Fracastoro and Tusi's Device: Observations on the Use and Transmission of a Model‹, *Journal for the History of Astronomy*, Vol. 26, 1995, pp. 133–154; T. E. Huff, *Intellectual Curiosity and the Scientific Revolution: A Global Perspective*, Cambridge: Cambridge University Press, 2011; M. Kokowski, ›Copernicus, Arabic Science, and The Scientific (R)evolution‹, in A. Bala (ed.), *Asia, Europe and the Emergence of Modern Science: Knowledge Crossing Boundaries*, New York: Palgrave Macmillan, 2012, pp. 55–72; V. Blåsjö, ›A Critique of the Arguments for Maragha Influence on Copernicus‹, *Journal for the History of Astronomy*, Vol. 45, No. 2, 2014, pp. 183–195.

prehensive account of the transmission position is given by Saliba who argues that the Copernican theory can be easily derived from the al-Shatir model by inverting the vector connecting the Earth-Moon system with the Sun. He also points to more specific circumstantial evidence to show that the way Copernicus deployed the Tusi couple and the Urdi lemma, as well as his models for the Moon and Mercury, were directly drawn from Ibn al-Shatir. Saliba's case for transmission has been systematically contested by Viktor Blåsjö who argues for independent discovery in a recent paper entitled *A Critique of the Arguments for Maragha Influence on Copernicus*. We will now compare their arguments to explore more deeply the epistemological issues surrounding transmission versus independent discovery debates between dialogical historians and their critics.

Consider the Tusi couple, which came to be deployed as a substitute for the equant by Maragha School astronomers, that combines two circular motions to generate rectilinear motion. Since Copernicus uses the device as well to eliminate the equant, Saliba has argued that he must have learnt it in some way from the Arabic tradition. However Blåsjö disputes this claim. He writes:

> [T]he Tusi couple is nothing but the special case of a simple epicyclic model where the epicycle has the same radius as the deferent. It would therefore be natural for the Tusi couple to suggest itself to a skilled geometer working in the Ptolemaic tradition. Thus independent discovery by Copernicus is not at all implausible (Blåsjö 2014: 185).

Blåsjö's argument that Copernicus as a skilled geometer could easily have discovered the Tusi couple leaves unanswered the question why it was not discovered much earlier in Europe by astronomers working with the Ptolemaic model, or by other astronomers in the Arabic tradition before al-Tusi. In fact al-Tusi himself was motivated to make this discovery because he accepted Ibn al-Haytham's demand nearly two centuries earlier that astronomers should do away with the equant. But the fact that in the Arabic world it required two centuries to accomplish this discovery makes Blåsjö's claim that any skilled geometer could easily have discovered the theorem extremely implausible.

Blåsjö also addresses the circumstantial evidence, first noted by Hartner, that Copernicus used an order of the letters in Latin that followed the order of Arabic letters used by Tusi in his proof of the Tusi couple. Hartner concludes that this »proves clearly that we have to do with a case of borrowing« (Hartner 1973: 421). However, only five of the six letters in the Tusi figure agree with that of Copernicus. Saliba argues that the sixth letter came from a misreading of an Arabic character which was translated as an F rather than a Z. Blåsjö disputes this point. He argues that anyone familiar with Arabic would not make such a mistake, and that the order of the letters follows that in which the points are invoked when Copernicus gives his proof of the theorem. This argument has some plausibility. If the lettering follows the order in which the points are invoked in the proof of the theorem both Tusi and Copernicus are likely to independently label the points in the same order. However, showing that Copernicus may have given independent justification of the theorem does not show he independently discovered it. It would be reasonable to say that any skilled geometer could prove the theorem once it has been discovered, but we have seen that it is extremely implausible to suggest that any such geometer could have discovered the theorem.

Let us now turn to the Urdi lemma which, like the Tusi couple, was also unknown to the ancient Greeks. Although Copernicus did attempt to prove the Tusi couple he gave no formal proof for the Urdi lemma. It leads Saliba to conclude that »the almost unconscious use of Urdi's Lemma by Copernicus […] must raise doubts about Copernicus's awareness of the roots of all the mathematical techniques that were at his disposal« (Saliba 2007: 205). However, Blåsjö disputes the charge that Copernicus was ignorant of what he was doing. He argues that the Urdi lemma is not used by Copernicus as a theorem in the way he deploys the Tusi couple but merely as a tool to define his planetary models. Consequently he would not have felt any need to give formal proof of the Urdi lemma since it was not proposed as a theorem. Even if Blåsjö is right, Copernicus would have had to have known the theorem in order to construct the model. Blåsjö seems to presume that Copernicus would have independently discovered the Urdi lemma. Again this is extremely implausible since if he had done so he would have mentioned it as a discovery of a new mathematical theorem.

Indeed it is particularly striking that although Copernicus deployed the Tusi couple and the Urdi lemma in his astronomical model, he did not make any mention of the fact that he independently discovered these theorems quite unknown to the Greeks. Neither did anyone of the other astronomers and geometers reading Copernicus' treatise attribute to him the discovery of two new geometrical theorems. This suggests that these theorems must have been quite widely known even before Copernicus invoked them for use in his heliocentric model of the universe.

George Saliba writes that »Ibn al-Shatir's lunar model was indeed identical, in every respect, to that of Copernicus« (*ibid*.: 196). Both their lunar models were intended to rectify perceived defects in Ptolemy's lunar model that were first noted by Ibn al-Haytham. The main problem with the Ptolemaic theory was that the structure of epicycles that defined the moon's motion caused its furthest distance from the earth to be more than twice the nearest. However, the changes in the visual appearance of the moon, and the parallax effects it exhibited, did not conform to such large variations in its distance from the earth. Despite his undoubted genius al-Haytham was unable to solve the problem which was taken up by a string of mathematical astronomers who followed him, including al-Tusi and Urdi, until it was finally solved by Ibn al-Shatir more than two centuries later. The same solution was offered by Copernicus lending credibility to Saliba's claim that Copernicus was influenced by Ibn al-Shatir.

Blåsjö disputes this. He writes:

> Copernicus and ibn al-Shatir solve this same obvious problem in the same obvious way, namely by controlling the effect of the epicycle by varying its size instead of its distance. This has the same visual effect so far as the longitudinal position of the Moon is concerned but without requiring great variations in its distance. The variation in the radius of the epicycle is achieved by a second epicycle, whose period is such that it always displaces the Moon inwards and outwards when Ptolemy moved it further and closer respectively. […] This single idea constitutes the full extent of the similarities, or alleged ›identity‹, of Copernicus' and Ibn al-Shatir's lunar models. There can be little doubt that this very simple idea would have suggested itself to any serious astronomer tackling the problem, so this trivial agreement proves nothing about influence (Blåsjö 2014: 189).

It is quite striking that the major achievement of solving the problem of lunar motion is seen by Blåsjö as a simple idea that would have occurred to any astronomer. Since Ptolemy himself would have been aware of the problem it makes one wonder why a solution that appears so obvious to Blåsjö would not have been adopted by Ptolemy himself. Moreover, even after Ibn al-Haytham pointed to the problem, it took centuries of work by a chain of astronomers for a solution to be found.

The similarity between Copernicus's model for planetary motion with that of Ibn al-Shatir has also been noted. This is especially the case for the planet Mercury where the similarity between them is even more striking because here is where they make the greatest changes to the Ptolemaic model, if we make allowance for the fact that Copernicus was proposing a heliocentric theory but Ibn al-Shatir a geocentric one. It leads Saliba to argue that Copernicus was following the Maragha tradition. But Blåsjö again disputes this claim by writing:

> The similarities between Copernicus's planetary models and those of the late medieval Islamic astronomers are not surprising. Heliocentrism aside, Copernicus picked up where Ptolemy left off and attempted to reform Ptolemaic astronomy while adhering to the framework of classical Greek astronomy very closely and conservatively, as did his Islamic counterparts. Overlapping, independent discoveries are to be expected in such circumstances. […] This leaves us no good reasons for believing that Copernicus was influenced by late medieval Islamic astronomy (*ibid*.: 193–194).

Blåsjö's conclusion is questionable because there are many ways of constructing non-Ptolemaic astronomical models that do away with the equant by deploying the Tusi-couple and the Urdi lemma. The al-Shatir model is merely one possible approach. In the sixteenth century Shams al-Din al-Khafri (died 1550) produced four such different models for Mercury's motion. None of them were similar to the others in mathematical construction but all of them were able to account for the same set of observations (Saliba 2007: 166). This raises the question: Why did Copernicus propose a model so similar to the Ibn al-Shatir model and not any of the other possible models that al-Khafri had devised? This seems too much of a coincidence suggesting that it is reasonable to assume that Copernicus was indeed influenced by Ibn al-Shatir.

Moreover, Blåsjö places too much weight on the singular genius of Copernicus to make the leap directly from Ptolemy to his heliocentric theory bypassing the Maragha revolution. Yet the Maragha revolution was many centuries in the making. It began with Ibn al-Haytham's critique of the Ptolemaic model and its use of the equant, the discovery of the Tusi couple and the Urdi lemma, the construction by various astronomers of planetary models rectifying the problems found in Ptolemy, culminating in the Ibn al-Shatir model which was but one of several possible models as demonstrated by al-Khafri. Yet Copernicus is seen by Blåsjö as not only rediscovering independently the Tusi couple and the Urdi lemma, and constructing a planetary model similar to that of Ibn al-Shatir's, but also deploying all this to produce a revolutionary heliocentric model of the universe. This seems too much to accomplish in a singular lifetime even for the genius of a Copernicus – a point earlier stressed by Ragep when he notes:

> 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. […] 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 (Ragep 2007: 70–71).[15]

Moreover there is evidence that the use of harmonic operators, developed by Maragha astronomers, spread to early sixteenth-century Latin astronomers such as Angelus, Johann Werner and Copernicus (Dobrzycki and Kremer 1996: 189).[16] Such connections would explain why Copernicus might have been motivated to address the equant problem, and even recognise potential solutions for it when he found it in the writings of these authors. Indeed Owen Gingerich has suggested that Copernicus could have come to know of the Urdi lemma without being aware of its Islamic antecedence, through acquaintance

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[15] However, where we do have similar historical contexts it is possible to make a case for independent discoveries across cultures. Thus the parallel discovery of the theory of the rainbow by Kamal al-Din al-Farisi and Theodoric of Freiberg can be seen as independent achievements built upon the optical discoveries of al-Haytham. Such independent discoveries are common where scientists working far apart are nevertheless operating within the same conceptual paradigm. See D. Lamb and S. M. Easton, *Multiple Discovery: The Pattern of Scientific Progress,* Amersham: Avebury Press, 1984.

[16] J. Dobrzycki and R. L. Kremer, ›Peurbach and Maragha Astronomy? The Ephemerides of Johannes Angelus and Their Implications‹, *Journal for the History of Astronomy,* Vol. 27, 1996, pp. 187–237.

with the works of Angelus (Gingerich 2004: 263–265).[17] All this suggests that it is far more credible to believe, as Swerdlow and Neugebauer conclude in their study *Mathematical Astronomy in Copernicus' De Revolutionibus*, that »In a very real sense, Copernicus can be looked upon as if not the last, surely as the most noted follower of the Maragha School« (Swerdlow and Neugebauer 1984: 225).

4 Rethinking the Transmission Problem

However, even if the case for transmission appears to be comparatively more plausible than independent discovery it still leaves unanswered a number of problems raised by critics of transmission. This is highlighted by Blåsjö who writes:

> Copernicus's planetary models exhibit some striking similarities to those of late medieval astronomers. On this basis many have concluded that he must have been influenced by them. We claim that no good evidence for this inference exists. Certainly there is no direct trace of it in the documentary record: the sources Copernicus supposedly copied from are not cited by him or any of his European contemporaries – despite the fact that Copernicus cites numerous earlier Islamic sources – and there is virtually no evidence they were accessible to him (Blåsjö 2014: 183).

Blåsjö raises three key concerns that any transmission theory must address. First why is there no documentary evidence by way of translations or records of this influence? Second why is there no acknowledgement of this influence by either Copernicus or his European contemporaries? Finally, if Eurocentrism is the cause for lack of acknowledgement as Joseph suggests, why does Copernicus acknowledge other Arabic sources but not those of the Maragha School? Answering these questions adequately would greatly strengthen claims for transmission.

I would like to argue that transmissions did not occur directly through translations or readings of Arabic texts, but were mediated by oral exchanges with scholars who travelled between Europe and the Islamic world. Such exchanges would not leave direct documentary evidence or motivate acknowledgement in writing. Support for

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[17] O. Gingerich, *The Book Nobody Read: Chasing the Revolutions of Nicolaus Copernicus*, New York: Walker & Company, 2004. I would also like to thank one of the anonymous reviewer of this article for bringing these connections to my attention.

such transmissions comes from a recent paper by Robert Morrison in which he argues that Jewish scholars expelled from Spain in 1492 constituted a transnational diaspora creating connections between the Christian and Islamic worlds and serving as a conduit for scientific knowledge in the mid-sixteenth century (Morrison 2014: 34).[18]

In particular Morrison points to Moses Galeano (Musa Jalinus) as a possible transmitter of scientific knowledge between the Ottoman Empire and the Veneto, especially between 1497 and 1502. He notes that Galeano had knowledge of the Arabic scientific theories that appear in Copernicus' work and was in contact with Christian scholars in the Veneto where Copernicus studied medicine at the University of Padua between 1501 and 1503 (*ibid.*: 35). He also adds that Galeano had knowledge and understanding of Ibn al-Shatir's theoretical astronomy (*ibid.*: 39). This leads Morrison to conclude:

> [T]here may be a limit to how much can be attributed to independent discovery in the face of a plausible path of transmission. Andre Goddu has written that without a clear path of transmission to explain the parallels between the astronomy of the Islamic world and the work of Renaissance astronomers, including Copernicus, we should entertain other explanations. To account for the overwhelming parallels between Copernicus's models and Ibn al-Shatir's, he noted that Sandivogius of Czechel and Albert of Brudzewo proposed double-epicycle models for the moon. But those lunar models were not the same as Ibn al-Shatir's and Copernicus's and there was no explanation of how those double-epicycle lunar models led to planetary models that, with the exception of heliocentrism, were equivalent to Ibn al-Shatir's. In contrast, the network of transmission that this article has described would seem to offer a more plausible explanation (*ibid.*: 57).

The presence of Jewish scholars who moved between the Islamic and Christian worlds is also significant for another reason. These were scholars capable of reading both Latin and Arabic and transmitting knowledge from one culture to the other without the need to do actual written translations. This further weakens the plausibility of independent discovery claims and strengthens that of dialogical connections.

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[18] R. Morrison, ›A Scholarly Intermediary between the Ottoman Empire and Renaissance Europe‹, *Isis*, Vol. 105, 2014, pp. 32–57. For a more comprehensive study of such exchanges through intermediaries between the Latin and Islamic worlds see A. Ben-Zaken, *Cross-cultural Scientific Exchanges in the Eastern Mediterranean, 1560–1660*, Baltimore: Johns Hopkins University Press, 2010.

Indeed the hypothesis of oral transmission resolves many of the objections that motivate claims for independent discovery by critics of dialogical historians. It explains the absence of documentary evidence for transmission, how Maragha knowledge could be acquired without acquaintance with the Arabic language, why European astronomers came to such knowledge only after corridors of communication were opened through Jewish scholars and others, why Copernicus's use of the Tusi couple and Urdi lemma were not seen as seminal mathematical achievements, and why Copernicus developed the Ibn al-Shatir model rather than al-Khafri's other variants.

By contrast the hypothesis of independent discovery adopted by critics of the dialogical approaches makes the incredible assumption that Copernicus's genius not only recapitulated centuries of Maragha School mathematical achievements single-handedly, but also used these to frame a revolutionary new astronomical vision. Moreover it cannot explain either why Copernicus's contemporaries did not see his use of the Tusi couple and Urdi lemma as seminal mathematical achievements, or why his model coincidentally paralleled Ibn al-Shatir's and not any of al-Khafri's other possible variants. Thus from the point of view of comparative plausibility the dialogical historical case for transmission of Maragha discoveries to Copernicus appears more plausible than that for independent discovery as claimed by its critics, even though no conclusive documentary evidence can settle the matter either way.

This conclusion has wider implications for the dialogical construction of the history of the Scientific Revolution. It is not just the Maragha School, but also the contributions from the Chinese infinite empty space cosmology and Indian Kerala School astronomy that have been repudiated by critics of the dialogical approach on the grounds of lack of documentary evidence. The tacit assumption informing such judgments is that all transmission must be established by documentary evidence. But we have seen that transmissions made through intermediaries or brokers across cultures need not leave documentary evidence, but could nevertheless be traced through circumstantial evidence. Thus we cannot assume independent discovery simply on the basis of absence of documentary evidence. What seems more reasonable is to compare the circumstantial evidence for both transmission and independent discovery against each other to determine which has greater plausibility. By adopting this approach we have found that the case for transmission from the Maragha School to Copernicus has more credibility than the case for independent discovery. If similar approaches show dialogical influences from Indian and Chinese astronomical traditions on the Scientific Revolution we might be required to radically revise our orthodox conceptions of the role that a dialogue of civilizations played in the birth of modern science.

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