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Islamic Reactions to Ptolemy's Imprecisions

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

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

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

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

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

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

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

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

The Measurement of the Earth

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

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

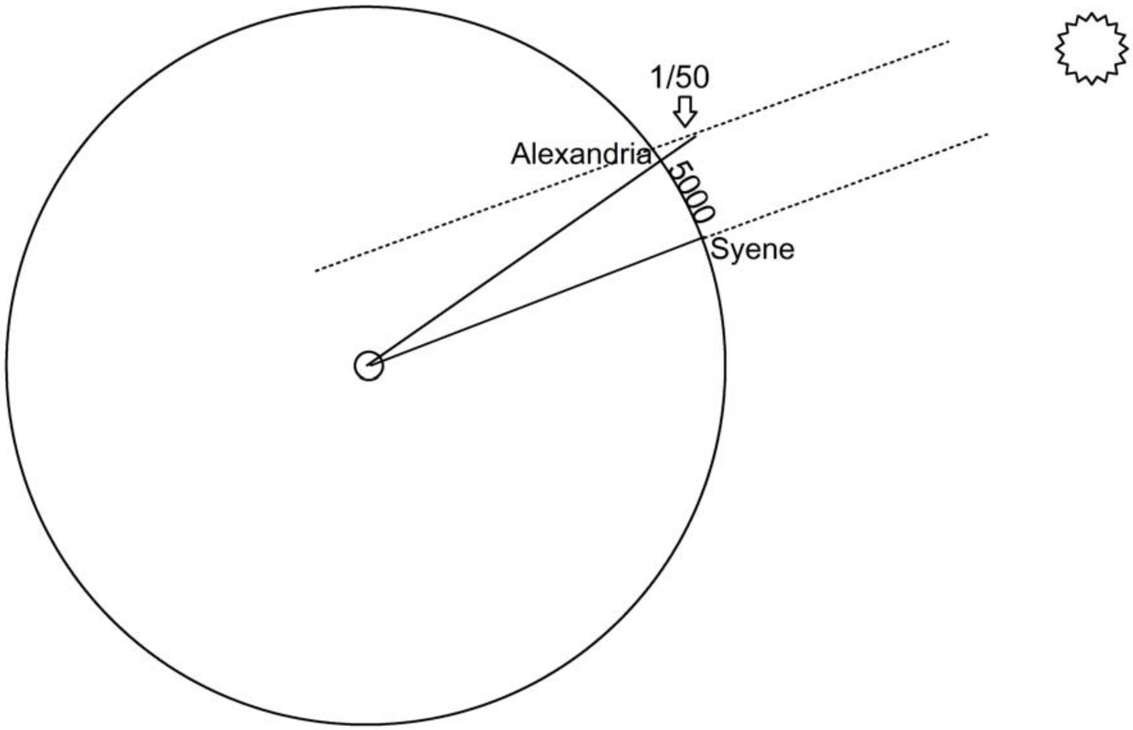

Fig. 1 Eratosthenes' measurement of the Earths' circumference

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 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 35º20′ and 34º, respectively. (The modern values are 35º58′ and 34º35′; in actuality, Raqqa is about 45′ east of Palmyra.) Since the Ptolemaic distance was given as 90 Roman miles, this did more or less confirm the Ptolemaic value of 66⅔ miles/meridian degree or 180,000 stades for the Earth's circumference. (Note this is based upon a Roman mile of 7.5 Ptolemaic stades rather than the 8 Attic stades presumably used by Eratosthenes; see above.) What is interesting about this story is that Maʾmūn seems not to have been happy with this "confirmation," perhaps because he was, correctly, not convinced that his astronomers knew the exact length 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 km² 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, 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

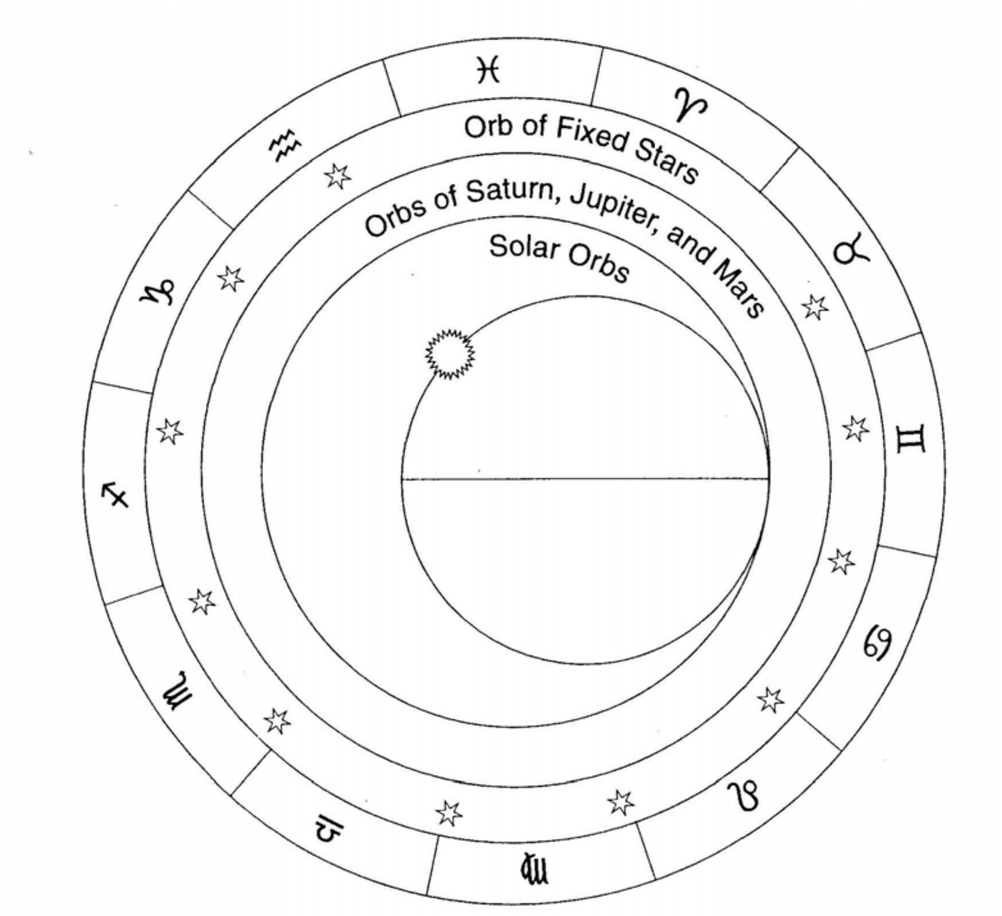

Fig. 2 A continuous speeding up (by trepidation or some other means) of the motion of the Eighth/Fixed Star Orb is here transmitted to the Sun's orbs, causing the Sun to reach the fixed vernal equinox sooner than it would with a simple monotonic precession. Battānī claims this might explain the differences 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)	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	1,628 (+882)	365¼ – (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 with 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. 47º42′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 (30º58′), 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 23º35′, 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 al-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 23º34′ had been made after Maʾmūn's time. But then Ibn Sīnā gives his own observation to the nearest half minute, namely 23º33½′. This is a remarkably good value inasmuch as the estimate using modern tools gives 23º33′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 23º33′40″, which for 1040 would have been correct to within 8 or 9 s (al-Jūzjānī*, Khilāṣ kayfiyyat tarkīb al-aflāk,* Mashhad MS Āstān-i Quds 392 (=Mashhad 5593), p. 96). How they obtained such astonishing accuracy is not entirely clear, since they have not left us with detailed observational notes. We do, though, know that Ibn Sīnā was very interested in observations and invented an innovative observing device of some sophistication (Wiedemann and Juynboll 1927). It is also worth mentioning here that Ibn Sīnā claimed to have observed a Venus transit and also found the longitude distance between Jurjān and Baghdad to be 9°20′ [modern: 10°3′; traditional: 8°] (Ragep and Ragep 2004, p. 10). Although Bīrūnī did not think much of Ibn Sīnā's astronomical abilities, it is interesting that Bīrūnī basically ended up "confirming" the Maʾmūnī observations, whereas Ibn Sīnā and his circle seem to have embarked upon a serious observing program to test, and modify, previous results. Whether the remarkably accurate values they came up with are a matter of accident or due to innovative observational techniques remains a matter of conjecture. (It is worth noting that although the normal human visual acuity is limited to 1 min of arc, it is possible under certain circumstances involving the observation of a moving object to become hyperacute, with the capability to distinguish even 5 s of arc (Buchwald 2006, pp. 620-621)).

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

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

Confirming vs. Testing

Let us look a bit more closely at the distinction I am trying to make between confirming and testing. (For the following, I am much indebted to Sabra 1968.) One often finds derived forms of the verb *iʿtabara* to indicate something like testing in the sense of checking whether a received value or parameter is correct; this is what Bīrūnī uses when saying that he wishes to test his predecessors' values for the obliquity. We also find another word, *imtiḥān*, which is used in the names of some *zīj*es such as the *Mumtaḥan Zīj* of the early ʿAbbāsid astronomer Yaḥya ibn Abī Manṣūr, and also in works that are meant to weed out incompetents, such as al-Qabīṣī's (10th c.) *Risāla fī imtiḥān al-munajimmīn* (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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(Extracted from the General Bibliography, pp. 217–229)

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