Simplicius and the Early History of Greek Planetary Theory

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In earlier work, Bernard R. Goldstein and the present author have introduced a procedural rule for historical inquiry, which requires that one take pains to establish the credibility of any citation of ancient thought by later writers in antiquity through a process of verification. In this paper, I shall apply what I call the Rule of Ancient Citations to Simplicius' interpretation of Aristotle's remarks in Meta Λ. 8*, which is the primary point of departure for the modern understanding of Greek planetary theory. I first sketch several lines of argument that lead me to conclude that Simplicius' interpretation should not be accepted because it assumes a concern with planetary phenomena unknown to the Greeks before the late 2nd and early 1st centuries* BC*. Then, after showing that there is a fairly well defined range of readings of Aristotle's remarks more in keeping with what we actually know of astronomy in the 5th and 4th centuries* BC*, I conclude that neither Aristotle's report about the Eudoxan and Callippan accounts of the celestial motions nor Simplicius' interpretation of this report is a good starting point for our understanding of early Greek planetary theory.*

1. Introduction: A principle for research

Over the past years I have enjoyed collaborating with Bernard Goldstein in the study of ancient Greek and Latin astronomy. So, I hope that it will be found appropriate that I propose to honor him now by endeavoring to extend a key principle of our collaboration and outlining a critical finding in the history of early Greek astronomy.

During the course of our collaboration, we have had several occasions to declare what I shall call the Rule of Ancient Citations. This Rule, which we have presented in various ways, is essentially procedural and concerns verification (Bowen and Goldstein 1991, pp. 235, 241–245; 1994,

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pp. 690–691; Goldstein and Bowen 1999, p. 341). It demands that one take pains to establish the credibility of any citation of ancient thought by later writers in antiquity through a process of verification. (By 'citation', I mean a quotation, paraphrase, and report or *testimonium.*) So put the Rule seems unexceptionable. Indeed, one would rightly regard it as a necessary feature of any historical analysis that is to yield knowledge of the past. Then what is the fuss about?

Well, to begin, it turns out that this Rule plays but a minor role, if any at all, in the current study of ancient thought. And this is especially true of the various histories of astronomy now available. The fact of the matter is that when one consistently adopts this Rule, it quickly becomes evident that there are important instances in which ancient citations of works still extant are wrong or seriously misleading. Now these instances do more than show the mere unreliability of the particular writers making the citations in question. In fact, they raise, I submit, a nasty problem when the object cited is no longer extant, since such instances entail that we may seriously deceive ourselves by extending any benefit of the doubt when what is cited is no longer available to us for critical inspection. Thus, for example, while one may reasonably presume that Hipparchus' quotations of Eudoxus' lost treatises are reliable on the ground that his quotations of Aratus' *Phaenomena* are demonstrably so, we should not, like so many modern historians of early Greek astronomy, assume without question that Hipparchus' reports or accounts of what Eudoxus meant are also reliable (see Bowen and Goldstein 1991, pp. 241–245). Accordingly, to avoid such self-deception we are obliged to seek confirmatory support for citations of this sort case by case.

The upshot is that by virtue of the Rule there falls on us a real burden of vigilance precisely in those instances when we should hope to extend our knowledge of the past beyond the confines of what has come down to us in writing. At the same time, as one might well imagine, following this Rule means that scholars will inevitably have to decline to accept many ancient claims made about the past, on the ground that too little remains of the intellectual context of the object cited to permit a reasoned, critical acceptance of the citation in question. But enough of the generalities.

2. A case at issue: Simplicius' commentary on Aristotle, *Metaphysica* Λ 8

The *testimonium* I have chosen for study combines the two epistemological situations I have just mentioned in so far as it is an interpretation of an extant report concerning views that would otherwise be unknown to us. Specifically at issue is Simplicius' interpretation of Aristotle's remarks in *Meta.* Λ 8 about the views of Eudoxus and Callippus concerning the number of distinct celestial motions. This *testimonium* is without a doubt the primary point of departure for the modern understanding of Greek planetary theory. Indeed, the numerous histories of astronomy published of late typically cite Aristotle's remarks and then offer the reader a sanitized version of Simplicius' account. (See Heglmeier 1996, Mendell 1998 and 2000, Yavetz 1998 for recent attempts to 'rectify' this account). None asks the critical question, "Is Simplicius' explanation of Aristotle's testimony a credible starting point for reconstructing early Greek planetary theory?," or recognizes any need for verification.

In this paper, I will first sketch the various lines of argument that lead me to conclude that Simplicius' interpretation of Aristotle's remarks in Λ 8 fails because it assumes a concern with planetary phenomena unknown to the Greeks before the late 2nd and early 1st centuries BC. Then I shall propose that, though there is a fairly well defined range of readings of Aristotle's report about Eudoxus and Callippus that is more in keeping with what we know of astronomy in the 5th and 4th centuries BC on the basis of texts dating from that period, there remains too little to say what Eudoxus and Callippus actually had in mind.

3.1 Simplicius and the early awareness of planetary stations and retrogradations

One critical assumption of Simplicius' testimony in his commentary on Aristotle's *De caelo* is that Eudoxus, Callippus, and Aristotle were familiar with the phenomena of planetary station and retrogradation. Deny this assumption and the testimony with all its various details fails dramatically. So let us examine this assumption.

To begin, we should observe that the usual technical language in Greek and Latin for the planetary stations and retrogradations is not attested until documents dating from the 1st century BC. This is an important consideration. Granted, the absence of a technical vocabulary does not by itself prove the absence of the related concepts; nevertheless, it does at least suggest that the concepts, if they *were* present earlier, were *at best* marginal in the science of that period.

Next, and more telling, is the fact that, if we set aside *Meta.* Λ 8 for the moment, there is no warrant for reading the concepts of planetary station and retrogradation into any Greco-Latin text written prior to the late 2nd century BC. As I have shown elsewhere (Bowen 2001, pp. 812–817, 821–822), none of the other passages usually culled from Plato, Aristotle, Epicurus, and even from Ptolemy, is good evidence of knowledge that any of the 5 planets have stations and retrogradations. Indeed, I maintain that these passages are better read without importing this knowledge, since the basic conception underlying the talk of falling behind and advancing that one finds in many of them is not meant in relation to the fixed stars (as it would have to be, if retrogradation and direct motion were at issue) but only in relation to the other planets. Let me explain.

The early Greeks distinguished stars that seem fixed in relation to one another and those that were not fixed in this way but appear to wander. They were also aware that these wandering stars or planets have diurnal and sidereal motions. But, so far as I can tell, this was pretty much the full extent of their knowledge of planetary motion. Symptomatic of this was, I submit, their tendency to analyze the eastward motions of the planets in relation to one another and not in relation to the background of the fixed stars. Their image for this was drawn from the race-course. Thus, in Plato's *Timaeus* at 38c7–d6, for example, Venus and Mercury are imagined as runners who, though they keep pace with the Sun overall and complete their eastward motion in the same time, occasionally fall behind, then catch up to, and even overtake the Sun before slowing down and falling back again (see Bowen 2001, pp. 814–816). It is important to see that nothing in this metaphor implies or demands that Mercury and Venus make stations and retrogradations. That is, the imagery no more entails this than, for instance, falling behind in a footrace and then catching up and going ahead entails really stopping, going backwards, stopping, then racing forwards, or even appearing to do any of these things against the background of stationary spectators. All the image requires is a sense of the overall eastward direction of the race, and this itself may have been inferred from the fact that the planets rise later and later in relation to the fixed stars over the course of time. In any case, the image is no warrant for talk of planetary stations and retrogradations.

3.2 Simplicius' interpretation

I remind you that Simplicius' account of *Meta.* Λ 8 is a report. This means that there should be no presumption of its accuracy: though Simplicius is demonstrably reliable in quoting the words of others, his reports of what the words mean are themselves notoriously contaminated by anachronism and distorted by alien (Neoplatonist) commitments. This is not to say, of course, that Simplicius is necessarily wrong about Eudoxus and Callippus, but only that we must not take it for granted that he is right. In other words, we should treat Simplicius' testimony as an interpretation, a requirement that accords with the slight internal evidence that he is conjecturing how Aristotle's third and fourth spheres for the planets are to work together (see Heiberg 1894, p. 495.29: thus far, Simplicius has been relying on Aristotle in order to explain Eudoxus' planetary models; but the phrase, ὧδέ πως ἔχουσιν 'are presumably disposed in this way', registers uncertainty as Simplicius now goes beyond anything found in Aristotle's text [cf. Heiberg 1894, p. 505.30] and infers what Aristotle and, hence, Eudoxus, had in mind).

But how, then, does Simplicius' testimony fare when viewed in this light? Are there any features of his interpretation that commend it above others? For my part, I do not think so. Some might point to the fact that Simplicius is drawing on a tradition that goes back to Eudemus *via* Sosigenes (e.g., Mendell 2000). Given, however, that there is no way to sort out the putative contributions of Eudemus and Sosigenes, or, more importantly, to confirm the historical accuracy of what either wrote, this hypothesis proves to be nugatory. Indeed, one might even say that not only does this hypothesis fail to change our epistemic situation, it makes it worse. When we started we were asking about the historical worth of an interpretation in hand. But now we must contemplate not only the question of Simplicius' fidelity to the views expressed by Sosigenes and Eudemus, we must wonder about the historical accuracy of these views so far as the meaning of Λ 8 is concerned. Thus, we start out with a question about a real document and are asked to exchange it for similar concerns about the historical reliability of texts that are no longer extant. In truth, there is no way to get behind this claim of a scholarly tradition to the historical 'facts' and to assure ourselves that the process of transmission has not introduced all manner of distortion as often happens in such instances. Indeed, there are serious flaws in Simplicius' report which one might—if verifiability were not a desideratum—attribute to the sort of distortion that typically occurs when information is passed through several hands. So what are these flaws?

To begin, there is the inescapable fact that, if Venus and Mars are assigned the values for their sidereal and synodic periods that Simplicius reports, they will not go retrograde at all (see Heath 1913, pp. 209–211). Now, one might choose to embrace this difficulty by assuming that mistakes of this sort are to be expected in the embryonic stages of scientific theory. Others, myself included, will see it as a sign that Simplicius' testimony is really a historical fiction created by writers who were not expert in astronomy. But many will, I expect, prefer to mitigate the problem by supposing that the numbers assigned the various periods, albeit Babylonian in origin, were not part of the original scheme conceived by Eudoxus and Callippus. The upshot, then, would be that Eudoxus and Callippus were concerned only with the qualitative features of planetary motion. (The problem with this, however, is the unacceptable assumption that we are at liberty to correct Simplicius' testimony so that it makes sense on our terms.)

Less easy to resolve are the difficulties arising from the fact that Simplicius embeds his remarks about Eudoxus and Callippus in a narrative aiming to locate all Greek philosophical knowledge in the works of Plato and Aristotle. As Simplicius would have it (see Heiberg 1894, pp. 488.18–24, 492.31–493.11), the history of astronomy from Plato to his own time was but an answer to Plato's challenge to explain planetary motion by means of smooth, ordered, circular motions. So viewed, this history clearly entails a sharp transition. For, while the planetary theory in the Platonic dialogues and *Meta.* Λ 8 is homocentric, the understanding in Simplicius' time of how the planets move is decidedly non-homocentric, based as it is on computational models involving eccentric and epicyclic circles put forth by Ptolemy. The main historiographic problem facing Simplicius, then, was to specify when this transition took place and why.

As it happens, Simplicius was working in an academic tradition of interpreting past intellectual achievements that was already quite well developed. Proclus, the teacher of Ammonius, who was in turn Simplicius' own teacher, had already located the origins of astronomy in the desire to explain (and thus 'save') phenomena that appeared to be at odds with fundamental tenets concerning the workings of the heavens. Moreover, Proclus had also cited the authority of Ptolemy in asserting that Apollonius of Perga had demonstrated the way to determine geometrically the stationary points of planetary motion using a non-homocentric model (but see Bowen 2001, pp. 821–822). And, according to Pappus, Apollonius had studied under the students of Euclid, a claim with which Proclus would seem to concur when he reports that Archimedes and Apollonius apparently used Euclid's *Elements* as a starting point for their own investigations. Further, Proclus also has it that Euclid was active in the late 4th and early 3rd centuries BC (but see Bowen and Goldstein 1991, p. 246 n. 30; Bowen 2003). So, even if these substantive claims about Apollonius or this date for Euclid are not acceptable today, there was obviously a well established chronology by Simplicius' time that I suspect he found quite persuasive, especially in light of Proclus' endorsement.

Accordingly, I infer that, for Simplicius, his goal of schematizing the history of Greek astronomy as a sequence of stages in planetary theory going back to Plato obliged him to account for the quick demise of homocentric theory in the relatively brief interval of, say, roughly 80 or 100 years, from Aristotle and Callippus to Apollonius. Simplicius' solution to the problem of the transition from homocentric to non-homocentric astronomical theories of the planetary motions is ingenious and elegant. Indeed, the narrative structure has a charming verisimilitude. Yet Simplicius 'history' is, I think, little more than a reconstruction of earlier celestial science on the basis of later astronomical theory. This is not the occasion to point out the numerous signs that Simplicius' narrative of how the Peripatetics came to abandon Aristotle in planetary theory is a fiction. Suffice it for now to indicate but one.

The pivotal moment in Simplicius' story about the change in the Peripatetic view of homocentrism is defined in part by his claim that Venus and Mars seem many times greater in size to the naked eye when they are at the middle of their retrograde arcs (see Heiberg 1894, pp. 504.16–505.30: there are a number of problems with the various claims made in this passage, but here I focus on one claim that figures prominently in modern histories of early Greek science). Now, given that all point sources of light in the night sky appear to have size, and that the brighter ones are seen as larger—a confusion not cleared up until the invention of the telescope—there is a marked tendency in the ancient literature to present differences in brightness as differences in apparent size. So, we shall have to bear this in mind when dealing with Simplicius' claim.

The first problem we must confront is that Venus is invisible to the naked eye whenever it is at the middle of its retrograde arc, a position otherwise known as inferior conjunction. Moreover, even if we allow Simplicius some looseness in his account and suppose him to be talking of Venus when it is *near* inferior conjunction, the empirical fact remains that neither the planet's apparent diameter nor its brightness vary significantly during its synodic cycle, that is to say, during the interval from one first morning visibility to the next. In fact, the variation in Venus' apparent diameter ranges from 0;0,10° to 0;1° of arc, which is well below the threshold of the power of the naked eye to resolve differences in angular distance (0;1°). Further, because Venus has phases which are compensated by its varying distance from the Earth, its brightness only ranges in magnitude from –3.9 to –4.7, which is equally difficult to detect by the naked eye (see Goldstein 1996, pp. 1–2). Yet, in contrast, Mars is visible in the middle of its retrograde arc, that is, at opposition, and is noticeably brighter and, hence, seemingly larger, here than at any other point in its synodic cycle. So what are we to make of this?

It is important to realize that, though there is some recognition in the period before Ptolemy that the stars (both fixed and wandering) differ in size or brightness from one another, there is no evidence that anyone noticed a variation in the size or brightness of any one star, or that they thought such a variation important (see, e.g., Aristotle, *Meteor.* 343b32–34; Pliny, *Hist. nat.* 2.39). Even Ptolemy, whose account in his *Hypotheses planetarum* puts the 5 planets at varying distances from the Earth and thus entails a variation in their size and brightness, makes nothing of it. (The same is true of Pliny: cf. *Hist. nat.* 2.64–68.) Moreover, it would also appear from Ptolemy's remarks in his *Hyp. plan.* that Hipparchus did not recognize any variation in the apparent diameter of Venus (see Goldstein 1967, 8b). Consequently, there is no good reason to follow Simplicius in supposing that there was any concern in the 3rd century BC with any variation in the apparent size and brightness of the 5 planets, especially when this concern is to be identified in part on the basis of mistaken claims about Venus.

Perhaps the key to Simplicius' error lies in his treating Mars and Venus in the same way. According to Ptolemy, the ratios of the farthest to the nearest distance from Earth for both Mars and Venus are nearly the same, roughly 7:1 in each case. Thus, on the ground that such a ratio in varying distance to the Earth ought to manifest itself as a similar ratio in varying apparent size or brightness, and given that Mars is observed by the naked eye to vary greatly in apparent size or brightness, Simplicius may have simply expected the same to be true of Venus. If so, his mistake was twofold. In the first instance, he would have construed a consequence of Ptolemaic theory as an observation—this is common enough in antiquity as it turns out. And, in the second, he would have overlooked the observable fact that Venus does not behave as Mars does. There are, of course, many respects in which Venus and Mars behave differently because Venus is an inner planet and Mars, an outer planet. Of particular relevance here is the fact that Venus is unlike Mars in being invisible at the middle of its retrograde arc, and that Venus does not actually change in apparent size and brightness as Mars does.

Still, however one explains Simplicius' error, it nevertheless remains most unlikely that any ancient came to adopt a non-homocentric theory of planetary motion by observing any variation in Venus' apparent size or brightness with the naked eye. That Simplicius claims otherwise is, I think, a sign that the story he is telling about early Greek celestial science is just that, a story, and not a what we should call history.

Thus far, my attack on Simplicius' interpretation of Λ 8 for wrongly supposing knowledge of planetary stations and retrogradations has advanced on two fronts. First, I have maintained that there is no compelling evidence for such knowledge in either the language or the meaning of any text written in Greek or Latin before the late 2nd century BC. Next, I have indicated that this interpretation is an integral part of a hermeneutic project governing a historical narrative that assumes empirical falsehoods. Now, I will continue the attack by arguing that Aristotle's testimony about Eudoxus and Callippus actually supports a variety of interpretations that are more in keeping with what we do know of Greek astronomy in Aristotle's time.

4. Aristotle on Eudoxus and Callippus

If, as I have maintained, Aristotle shows no awareness of the fact that the 5 planets make stations and retrogradations elsewhere in his writings, then one should not take it for granted that such knowledge informs *Meta.* Λ 8. Indeed, one should look for a reading that does not require it. So, the question is now, "Is there a reading of Aristotle's remarks in this passage that accords with what we know of Greek celestial science in the 5th and 4th centuries BC, that is consistent with Aristotle's declarations elsewhere in his writings about the planets, and that does not require or presuppose knowledge of planetary stations and retrogradations?"

The answer to this question is a resounding, "Yes," on all counts. In fact there are presumably many such readings, since Aristotle's report is underdetermined in several critical respects. This is as one might expect, given that Aristotle's aim in Λ 8 is simply to evaluate the number of distinct motions in the heavens and, hence, the number of unmoved movers (1073a14–b17). Still let us consider just one possibility for the sake of argument, a possibility governed by the 'minimalist' assumption that Aristotle alone is responsible for supposing the distinct planetary motions were just so many in number and that they were to be produced by spheres, as well as for putting these motions and spheres in order.

On this hypothesis, Eudoxus and Callippus spoke only of various planetary motions. Aristotle took their remarks to indicate distinct or independent motions which he then analyzed by a system of revolving spheres arranged in a suitable order. His claim that this is what Eudoxus and Callippus posited would simply be a matter of his taking his interpretation of what they said as what they *really* meant, a standard move in ancient philosophical discourse and hardly unknown today. Note that this hypothesis can serve to explain the reservations Aristotle expresses at *Meta.* 1073b10–17 and 1074a14–17.

Now, one defining interest of Greek celestial science during the 5th and 4th centuries BC lay in stellar phenomena at the horizon. As is evident in the Hippocratic *De aeribus aquis locis* and other medical treatises, the ancients watched the horizon for the risings and settings of the Sun, as well as for those of certain fixed stars and constellations. Their aims in this were diverse but included determining the alignments of buildings as well as constructing solar calendars—calendars that link the risings and settings of certain fixed stars and constellations with changes in the weather at given locations, and divide the solar year into intervals suitable for sowing or harvesting crops as well as into astronomical seasons (see Bowen and Goldstein 1988). Presumably related to this is the interest in mapping the primary reference circles of the celestial sphere that is evident during the

4th century BC, a task that includes recording which stars rise and set at the point where the Sun rises and sets on the days of summer solstice, equinox, and winter solstice.

In point of fact, there seem to be relatively few celestial phenomena which the ancients of the 5th and 4th centuries BC observed by looking upwards. Granted, they certainly saw phases of the Moon, lunar and solar eclipses, various occultations, and the like, in this way; and it undoubtedly figured in their grouping of stars into constellations. Nevertheless, the primary mode of scientific celestial observation during this period was, it seems, to the horizon.

Accordingly, so far as the 5 planets are concerned, let us suppose that Eudoxus' purported distinction of 4 motions for each planet was based on horizon observations. What might these planetary observations have been? Well, the first may have been that each planet makes a diurnal rising and setting; and second, that there are regular intervals between, say, its successive first appearances in the morning after invisibility. The third observation may have been that a given planet's northernmost rising point on the eastern horizon does not coincide with that of the Sun. And the fourth may have been that each planet has regularly recurrent co-risings with a given fixed star. As for Callippus, all one need suppose in addition is that he was aware of a variation in the length of the interval between first morning visibilities, for instance, a variation deemed sufficient to warrant positing another motion.

On this account, then, Aristotle is to be responsible for identifying how many distinct motions these observations disclosed and for assigning them to spheres nested in a certain order. The first and the fourth observations pose no problem—they lead directly to his first two spheres for diurnal and sidereal motion. The second and third observations are problematic; but let us conjecture that the third sphere accounts for motion in latitude and the fourth, for the (supposed) fact that this departure in latitude is tied to a synodic phenomenon such as the planet's first morning visibility. (The inclination of the axis of the fourth sphere to that of the third determines the greatest distance of a planet's northernmost rising point from that of the Sun.)

Now, there are problems with this reconstruction. For instance, one might object that it does not make proper allowance for the fact that each planet has a motion in latitude such that its rising and setting points coincide with those of the ecliptic just twice during the planet's sidereal period. But this is not a difficulty with the reconstruction *per se.* The failure to link motion in latitude with sidereal motion properly is really a feature of Aristotle's report, and it is found in the accounts of his 4 spheres that I have seen thus far (cf. Heiberg 1894, pp. 495.17–496.15); so we may perhaps be excused for leaving it as such. Besides, it is hardly clear (or uncontroversial) that the astronomical *realia* are to be necessary criteria of a successful reconstruction in this case.

There is a another problem, however, concerning this particular hypothesis or reading of Aristotle's testimony. Put simply, it is that nowhere is there attested in the extant literature in Greek dating from the 5th and 4th centuries BC any program for observing planetary phenomena at the horizon. There is, nevertheless, a well documented tradition of such observation in Babylonian literature dating from the mid-7th century BC. So, perhaps, rather than suppose that the Greeks themselves made such planetary observations on their own, it might be better to conjecture that the Greeks learned of them from the Babylonians. Aristotle himself certainly testifies to communication between the two cultures concerning matters in celestial science at *De caelo* 292a7–9 and *Meteor.* 343b28–30 (cf. [Plato], *Epin.* 987a1–6).

Granted, it is certain that the Babylonians knew of the planetary phenomena of station and retrogradation from early on; so it might be puzzling as to why this knowledge was not included in the communication that Aristotle mentions. But consider this: what evidence we do have for the leakage of Babylonian astronomy to Greece prior to the late 2nd century BC shows that it is a very fragmentary process—some observations here, a number there, an arithmetical scheme, and so on. In short, there really is no reason to expect that if the Greeks learned of *some* planetary horizon phenomena from the Babylonians, they must also have learned of their stations and retrogradations. Still, even if they did, as I have tried to make clear, what we lack is any compelling evidence that this information had an impact on their celestial science before the late 2nd century. Thus, on the present hypothesis we would have to allow the possibility that this information was transmitted to the early Greeks but not properly received because they were unprepared or focused on other things.

Here, then, we have one hypothesis concerning what underlies Aristotle's testimony about Eudoxus and Callippus. This hypothesis is only one of a number possible. Indeed, the hypotheses will proliferate as we import more and more assumptions about what Eudoxus and Callippus were maintaining, assumptions for example about the directions in which the spheres move, their periods, the precise location of the planetary body itself, and so forth—to say nothing about assuming that it is valid to make recourse to the astronomical *realia.* Still, at a basic level, our 'minimalist' hypothesis does explain this testimony fairly adequately and it does adhere closely to what is known from documents dating from the 5th and 4th centuries BC. (Note that this hypothesis leaves the project of saving the phenomena and the generation of the hippopede or figure 8—which requires certain, seemingly arbitrary, assumptions about the periods of the third and fourth spheres, their directions of motion, and so forth—as incidental, unintended consequences of the construction Aristotle describes. That is, on this hypothesis, ascribing to Eudoxus and Callippus any knowledge of planetary hippopedes and how they may represent the planetary stations and retrogradations would be a fallacy, specifically, the fallacy of imputing to someone the consequences of what he says or thinks.)

As I said, this is but one possible hypothesis about what underlies Aristotle's report about Eudoxus and Callippus in Λ 8: others will proliferate as one introduces more and more assumptions in order to interpret this text. My aim in offering it is to encourage readers to evaluate Simplicius' interpretation critically according to the Rule of Ancient Citations and to see it for what it is, a historically significant account with no demonstrable historical validity and many good reasons for doubting it. But having done this, I would be remiss if I failed to emphasize that, since we lack appropriate texts from Eudoxus and Callippus, and since we have so very little in the way of context, there is no credible way to test the truth of *any* interpretation of Aristotle's report in Λ 8 or to prefer one to another. In other words, if the goal of historical inquiry is knowledge and not the empty expression of what we are prepared to believe (and would dearly like others to follow us in believing), then we must look elsewhere if we wish to speak sensibly about Greek planetary theory in the period before Ptolemy (see, e.g., Goldstein 1997; Bowen 2001, pp. 822–829).

5. Conclusion

This completes the outline of my argument that neither Aristotle's report about the Eudoxan and Callippan accounts of the celestial motions nor Simplicius' interpretation of this report is a good starting point for our understanding of early Greek planetary theory. I will certainly agree with any reader who thinks much more will need to be said to drive home the various points made. Still this much should be clear: those who follow Simplicius in reading *Meta.* Λ 8 should acknowledge the Rule of Ancient Citations by taking up the burden of demonstrating *critically* the historical validity of his interpretation and of any departures they wish to make from it.

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