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ANAXAGORAS, EUDOXUS, AND THE REGRESSION OF THE LUNAR NODES*

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In the century and a half since Ideler[1] first advanced his claims, Eudoxus has come to be accorded a prominent place in the history of astronomy. Although very little is actually known regarding the details of his work, the hippopede is notorious to historians of science, and no one now doubts that Eudoxus took the first serious step in the development of planetary theory. Exactly how much he accomplished, particularly in the task of establishing accurate parameters, will probably never be known. But that he must have done a fairly rigorous job was documented beyond a reasonable doubt nearly a century ago, through the ingenious re-constructions of Schiaparelli.[2] Given only the numbers of spheres involved, and vague explanations of their arrangements and purposes, he showed how the various systems could have worked, in what respects they would have failed, and how these inadequacies could have been met through subsequent modifications known to have been introduced by Callippus. Schiaparelli's work was almost completely extrapolative: he started from the reports of Aristotle and Simplicius,[3] then went beyond them to develop geometrical combinations and numerical constants (which latter, of course, were wholly idealized, and never taken seriously by him or anyone else). In one respect, however, Schiaparelli felt justified in doing more—in actually altering the information at hand.

The situation arose in the lunar theory,[4] which was represented by three spheres. The outermost, as in all of Eudoxus's other theories, provided the diurnal rotation, and posed no problem. But according to both the text of Aristotle and the gloss of Simplicius, the second or middle sphere had its equator in the ecliptic, and moved with the monthly (draconitic, ideally) motion; while the innermost sphere, carrying the Moon on its equator, was slightly inclined (5°, hopefully) to the second, and moved retrograde very slowly. Now, neither Schiaparelli nor Ideler before him had the slightest doubt as to what Eudoxus was trying to do. Indeed, Simplicius had expressly confirmed their instinctive conclusions by stating that the third sphere was necessary to account for the Moon's movement in latitude, and for the fact that the Moon does not always reach its greatest latitudes at the same zodiacal points, but in places which shift steadily westwards. Unfortunately, the latitudinal motion produced by the reported arrangement is most peculiar compared to the actual behaviour of the Moon. For instead of propelling the Moon through its latitudinal cycle each month, what the purported mechanism would do is carry the Moon through its monthly longitudinal circuit at a nearly constant latitude(!) and keep it alternatively north and south of the ecliptic for half the period of the slowly rotating (18·6 years?) inner sphere. Because of the blatant contradiction with the phenomena, Ideler and Schiaparelli

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reversed the actions of the middle and inner spheres, so that the middle one moved slowly retrograde and the inner one provided the monthly motion.

For as long as historians of astronomy have been reading accounts of Eudoxus's system, this reversal has seemed both reasonable and justified. Only recently has the first challenge been issued, by D. R. Dicks, in his *Early Greek astronomy to Aristotle*.[5] As part of a general and laudable attitude of scepticism toward the interpretations that have been placed on various features of early astronomy by enthusiastic commentators, both ancient and modern, he severely criticizes this past willingness to "unhesitatingly take Simplicius' explanation of the third sphere as referring to the regression of the lunar nodes". Characterizing this assumption as "yet another example of a misleading interpretation of early astronomical thought by attributing to it concepts which were only discovered much later", he regards it as "highly improbable that Eudoxus knew anything about the nodes. . . ". This difference of opinion constitutes the subject of this paper. So far, all that exists is a difference of opinion. If the technicians never bothered to present an explicit argument in favour of their conclusions, neither has Dicks offered any counter-argument in favour of his. At the risk of being guilty of piling speculation on speculation, I wish to defend Ideler's emendation by analyzing the situation beyond what has thus far been done.

If there is anything that can truly be called a datum in pre-Socratic astronomy, it must surely be that Anaxagoras provided, if not for the first time, then at least to the first general satisfaction, an explanation of the phases of the Moon.[6] Closely related to this tradition is the report that he also explained the cause of eclipses.[7] Now, while the authority for this part of the story is considerably less impressive, there are at least two reasons for taking it seriously. The first is the fact that the reporter (Hippolytus)[8] included in his account an elaboration on the theme (to wit, that lunar eclipses are sometimes caused by bodies other than the Earth)[9] which renders it considerably less likely that he was just carelessly endowing an ancient figure with recent ideas. The second is the logical relationship between phases and eclipses. With the crux of both being the shining of the Moon by reflected sunlight—something explicitly credited to Anaxagoras by Plato,[10] whose knowledge would have been practically first hand— it is surely not stretching the imagination unduly to believe that Anaxagoras might actually have followed the consequences through to the explanation of eclipses. Unfortunately, of course, the parallel is rather too complete. For while the phases cycle around every month, nobody needs to be told that eclipses occur a great deal less frequently. The fact that some would take place during daylight hours may have occurred to someone: we have strong reasons for believing that the Babylonians had already recognized this difficulty.[11] But there would still be a lot of phantom eclipses involved in this so-called explanation. To avoid the ridicule of his fellow philosophers (and the satire of impertinent dramatists) Anaxagoras would have had to eliminate them. That he actually did so is suggested by Hippolytus's report of "other bodies"; for it is hard to imagine Anaxagoras working to get extra eclipses if he already had them occurring every month![12] It would be nice to be able to assume that he got rid of the superfluous eclipses by inclining the orbit of the Moon to

the ecliptic, but that happy solution seems untenable. For, if it did not still leave eclipses occurring more frequently than the Greeks noticed them (they must have missed at least a few) a complete grasp of the situation would certainly have obviated the necessity of introducing sub-lunary bodies to generate extra lunar eclipses. If we are to take these bodies seriously, we must assume that they were invoked to account for the fact that lunar eclipses are seen so much more often than solar eclipses. All of this would indicate that Anaxagoras probably (and not surprisingly) lacked an appreciation of the immense size of the Earth relative to the Moon's shadow, and had to restrict the frequency of his primary occultations to something commensurate with the frequency of solar eclipses. Under such circumstances, there would have been little else to do but call upon other bodies to provide the required lunar eclipses.

It would appear, then, that Anaxagoras probably failed to work out the entire potential of his theory. The alternative is to assume that the Greeks had not at that time succeeded in distinguishing astronomical from meteorological phenomena,[13] and that the "other bodies" were hypothesized to contend with occasional cloud-coverings. It is one thing, however, to suppose that Anaxagoras might have contented himself with some vague postulate whereby true eclipses happened only at occasional new and full Moons, and quite another to assume that such an incomplete explanation would stand unchallenged and unextended for any great length of time. Sooner or later, someone would have to reach the concept of an inclined orbit. Dicks (p. 179) is willing to believe that it might have arisen empirically, through observation of the Moon's path among the stars. Whether it is as easy as Dicks assumes to "observe" the ecliptic and various deviations therefrom, the fact remains that once the inclined orbit is attained, the concept of the node is there. Such a concept would provide a powerful generalization of eclipse phenomena: *viz.*, eclipses would perpetually occur at two opposite seasons of the year, in two opposite points of the heavens. Experience, of course, would soon falsify this proposition: the premisses would have to be modified to account for the fact that the eclipse positions move slowly backward through the year and around the zodiac. Voila! the regression of the lunar nodes.

Now, how reasonable is it to suppose that the Greeks might actually have followed some such train of thought? What kinds of problems are involved in assuming that, by Eudoxus's time at the latest, they had managed, through eclipse considerations, to formulate the concept not only of the nodes themselves, but of the regression of the nodes as well? One difficulty, obviously, is the lack of any real proof that pre-Socratic philosophy (or astronomy) took eclipses seriously enough to invest a certain minimum amount of time and effort in noting and remembering their occurrence, and worrying about explaining them. To this point, one can at least reply that eclipses appear to be second only to things such as earthquakes in their capacity to impress primitive man, and that we know from Herodotus's report about Thales that the Greeks had already accepted the *ideal* of predicting their appearance well before Anaxagoras's day.[14] Beyond this more or less *a priori* point stand Dicks's two objections. The first is the (as he sees it) abstract nature of either the concept of the node, or the conception of its movement. This attitude is, to say the least, puzzling. Dicks is willing to concede the notion of an inclined orbit. How, then, does

one have an inclined orbit without having nodes? and how much of a geometer does one have to be (Dicks labels Eudoxus a mathematician of genius) to recognize that the one entails the other? As for the regression of the nodes, Dicks again cheerfully admits that Eudoxus's theory was set up "to explain why the maximum deviations north and south did not occur always at the same points of the zodiac". Yet, despite this reference to the movement of the limits, and Simplicius's expressed statement that the limits "shift steadily westwards" (retrograde), Dicks balks at assuming a regression of the nodes. Given the indissoluble association between the nodes and the limits, the only reasonable way to deny the regression of the nodes is to reject Simplicius's comment on the westward shift of the limits. Yet this is precisely what Dicks is unwilling to do—tamper with Simplicius's statement.

In this last issue, of course, we encounter the second and more substantial of Dicks's objection to Ideler's reversal of the second and third spheres. Now, one can scarcely argue with the proposition that we must make historical as well as scientific sense of the data available to us: obviously, re-writing ancient reports to conform with our ideas of propriety is a serious step. But so is *rejecting* ancient reports, and Dicks is quite properly willing to do that when the occasion seems to demand it. Here would seem to be such an occasion. Consider the options. On the one hand is the possibility that Aristotle's account is correct, and that Eudoxus just blundered. It certainly cannot be rejected *a priori*; but neither can it be euphemized as "inaccuracy" in the way Dicks does. A system which entails virtually constant lunar latitudes for years at a time,[15] with the consequence of bunching eclipses during two relatively long nodal periods and then ruling them out completely for years in between is not simply erroneous: it is atrocious. Yet what is involved in the proposition at hand is not only conceiving that Eudoxus could perpetrate something of this nature, but also that none of his successors ever picked it up and corrected it.[16] The alternative, of course, is that the error was Aristotle's —that he either misunderstood Eudoxus's system or committed an elementary slip in his account of it. Perhaps it was a combination of these two. Following his run-down of the solar and lunar spheres, Aristotle says "The planets are moved by four spheres in each case; the first and second of these are the same as for the Sun and Moon...".[17] Note Aristotle's generalization. For the planets, the first sphere conveys the diurnal motion; likewise for the Sun and Moon. For the planets, the second sphere gives the major motion proper to the planet, while the third and fourth spheres together (he goes on to relate) handle the esoteric phenomena: for the Sun and Moon, *it should have been the other way around*—with the second providing special minor effects and the third providing the main peculiar motion. Is it not possible that Aristotle, the grand synthesizer, here on unfamiliar ground of less than vital interest to him, may have leaped to an invalid generalization in his own thinking on the subject?

Admittedly, this supposition is not without its problems. It is hard to imagine such a slip passing from Aristotle down to Simplicius, through hundreds of years of readers and commentators, without being noticed by anyone. Nor can challenging the direct, neutral statement of a person as alert as Simplicius (to say nothing of Aristotle) ever be a comfortable situation. Yet, the fact remains that there was a slip of some kind. Either Aristotle blundered and

generations of people (equally out of their element on the subject in question) read a brief description of an intricate mechanism presented in an alien context without finding any problem in it (as, indeed, Simplicius obviously did);[18] or Eudoxus blundered and generations of succeeding technicians worked from his system into better ones without realizing, or at least taking him to task for, the fact that he had committed a really egregious error. The story is puzzling either way. But surely the lesser puzzle lies in following Ideler and Schiaparelli in their reversal of the periods of the two spheres.

REFERENCES

1. Ludwig Ideler, *Abhandlungen der Berliner Akademie*, Hist.-phil. Kl., 1828, 189–212; 1830, 49–88.
2. Giovanni Schiaparelli, *Publicazione del Reale Osservatorio di Brera in Milano*, ix (1875); German trans. by W. Horn, "Die homocentrischen Sphaeren des Eudoxus, des Kallippus und des Aristoteles", *Abhandlungen zur Geschichte der Mathematik*, i (1877), 101–98.
3. In his *Metaphysics* (Λ, 8), Aristotle says:

   > Eudoxus held that the motion of the Sun and Moon involves in either case three spheres, of which the outermost is that of the fixed stars, the second revolves in the circle which bisects the zodiac, and the third revolves in a circle which is inclined across the breadth of the zodiac; but the circle in which the Moon moves is inclined at a greater angle than that in which the Sun moves. And he held that the motion of the planets involved in each case four spheres; and that of these the first and second are the same as before (for the sphere of the fixed stars is that which carries round all the other spheres, and the sphere next in order, which has its motion in the circle which bisects the zodiac, is common to all the planets); the third sphere of all the planets has its poles in the circle which bisects the zodiac; and the fourth sphere moves in the circle inclined to the equator of the third. (Loeb Library trans., p. 157.)

   Simplicius's commentary on this passage runs (according to Dreyer) from pp. 493–506 of J. L. Heiberg's *Simplicius: De caelo*. T. L. Heath (*Greek astronomy*, 67–70) has translated small portions of it.
4. Actually, the solar theory presents an analogous, but much more obscure situation, which has contributed to the general confusion over the possibility that precession was known before Hipparchus.
5. Cornell University Press, 1970. The discussion on which this critique is based is on pp. 178–81.
6. See Heath, 26–7. Although generally sceptical, Dicks feels (p. 56) that in Anaxagoras's case, the later reports of his ideas can be given more than usual credence.
7. Dicks (p. 59) includes this among his "attributions which are more likely to be correct than not".
8. *Refutation of all heresies*, i, 8. As nearly as I can see, Hippolytus (230 A.D.) included this information simply for the sake of completeness: it does not seem to constitute a target for polemics in his main discussion.
9. "The Moon is eclipsed through the interposition of the Earth, *and sometimes also of the bodies below the Moon*. The Sun is eclipsed at the new Moon, when the Moon is interposed" (Heath, 26; my italics).
10. Cratylus, 409 A.
11. See Pannekoek's discussion of the evidence on pp. 41–7 of *A history of astronomy* (London, 1961).
12. For the sake of simplicity, I shall omit consideration of solar eclipses.
13. As was the case, for example, with the earlier Babylonians.
14. That is, no matter how small the kernel of truth in the tale of Thales's feat, the tale itself demonstrates the existence of a concern for eclipse prediction, at least in Herodotus's day, if not already in Thales's.
15. It is entertaining to note the contradiction between this arrangement of the motion in latitude and Dicks's concession that "there is no reason to doubt that he could have noted approximately the Moon's maximum deviation in latitude. . .and this would give him the inclination of the third sphere". What would it be like to invent a system in which the Moon reached its maximum latitudes every nine(?) years, if one had made any attempt whatever to observe the Moon's latitudes over a short period of time?

16. Neither Callippus, who generally ameliorated the system and specifically added two spheres to the theory of the Moon, nor Eudemus, who is quoted by Simplicius as an independent authority, would appear to have commented on the situation. The later Alexandrians are equally silent. Yet Simplicius (Heath, 68–70) records criticisms of the concentric feature of Eudoxus's system dating virtually from the time of Aristotle, while Ptolemy does not hesitate to criticize even the venerable Hipparchus when he feels it is justified.
17. This translation of Heath (p. 65) expresses the generalization a bit more explicitly than that given in ref. 3.
18. It would be interesting to know whether Simplicius's detailed agreement with Aristotle is due to his merely following Aristotle and elaborating thoughtlessly on the third sphere, without bothering to check his sources, or whether he may actually have been copying or paraphrasing existing independent documents. Perhaps a Greek scholar could estimate the degree to which this portion of Simplicius's commentary differs from extemporaneous comments he makes elsewhere.