ON EARLY GREEK ASTRONOMY

In a somewhat polemical article on 'Solstices, Equinoxes, and the Presocratics'[1] D. R. Dicks has recently challenged the usual view that the Presocratics in general, and the Milesians in particular, made significant contributions to the development of scientific astronomy in Greece. According to Dicks, mathematical astronomy begins with the work of Meton and Euctemon about 430 B.C. What passes for astronomy in the earlier period 'was still in the pre-scientific stage' of 'rough-and-ready observations, unsystematically recorded and imperfectly understood,' of practical men' whose chief concern was to fix the seasons for ploughing, seed-time, sailing voyages and religious festivals. Ionian speculation, says Dicks, took very little note of such observation: 'some of its wilder flights of fancy might have been avoided, if it had taken more'.[2] In this account of the rise of Greek astronomy, the natural philosophers have no part to play. Their theories represent a speculative enterprise without a scientific future, a philosophic sideline with no impact on the development of observational science from Hesiod to Meton or the development of mathematical astronomy from Meton to Ptolemy.

I believe that such a dichotomy between early philosophy and early science in Greece is misguided in principle, and that it seriously distorts our picture of the initial phases of each discipline. It also imposes a considerable strain upon our credulity. Take the case of Anaxagoras who, according to Plato and Theophrastus, had given a causal explanation of eclipses of the moon a generation before Meton.[3] Is Anaxagoras not a typical representative of Ionian philosophy? Since Dicks will allow no scientific astronomy among the Presocratics, what are we to make of this? Is Anaxagoras' essentially correct explanation of eclipse a wild flight of fancy? Or was it derived from unsystematic observations, imperfectly understood? And what shall we say of Parmenides who, a generation earlier, was well on his way to understanding lunar eclipse, since he realised that the illuminated region of the moon is always turned in the direction of the sun (DK 28B15; *cf.* ἀλλότριον φῶς in B14)? This suggests a rather more advanced stage of astronomy than the practical knowledge of seasons and star-risings in Hesiod. Why should Dicks ignore such achievements of the early and middle fifth century in order to begin the history of Greek astronomy in 432 B.C.?

At first, the issue appears to be one of terminology. Dicks speaks of *mathematical* astronomy, by which he means a study based upon careful measurements (for example, of the length of the lunar month and solar year) permitting precise predictions as to the recurrence of certain phenomena, such as the coincidence of 19 solar years and 235 lunar months. Now it is true that 432 B.C. is *the earliest date for which we know* that such precise observations and predictions were carried out. But it certainly does not follow from our ignorance of the earlier period *that no systematic observations were made before that date!* Dicks' case against the existence of scientific astronomy in the Presocratic period depends in large part upon a fallacious inference from the absence of relevant evidence. It is an inference from *We do not know that observations were systematically made and recorded before 432* to *We know that they were not.* The impression of cogency in his argumentation derives almost entirely from his considerable knowledge of Hellenistic and late Greek astronomy. It turns out,

[1] *JHS* lxxxvi (1966) 26–40, quoted hereafter as 'Dicks'. In reflecting on Dicks' discussion of the astronomical problems, I have received valuable advice from Professor Howard Stein of the Department of Philosophy, Case Western Reserve University, and from Dr W. D. Heintz of the Astronomy Department, Swarthmore College. Needless to say, neither is responsible for the views here presented, except where their names are cited.

[2] Dicks 39.

[3] The evidence is conveniently assembled by Heath, *Aristarchus* 78 f. Also DK 59A42. 8–10, A76.

however, that his use of that knowledge establishes nothing about the period with which we are concerned, the sixth and fifth centuries B.C.

Nevertheless, Dicks' article raises a number of interesting questions, both of fact and interpretation, which call for further discussion. One of the most important points concerns his general contrast between scientific (or mathematical) astronomy and the cosmological speculation of the Presocratics. This is expressed as an antithesis between careful observation and wild flights of fancy, with the implication that rigorous science derives exclusively from the former (when 'the accumulation of material' had reached a sufficient level) and owes nothing to the bold speculation of the natural philosophers.[4] What Dicks offers is essentially a Baconian or neo-Baconian view of science which admits mathematical computation together with empirical observation as the necessary characteristics of science, but which denies any role to speculative hypotheses of a strongly theoretical nature. I think this view is false, and indeed generally discredited by recent work in the history and philosophy of science (by the writings of Alexandre Koyré, T. S. Kuhn, Sir Karl Popper, among others). Furthermore, Dicks does not always stick to this view himself, insofar as he recognises the importance for Greek astronomy of the development of a cosmic model, including a spherical heaven, a spherical earth, and a geometrical account of celestial motion. For the attempt to construct such a model is precisely one of the principal objects of Presocratic speculation. If we accept Neugebauer's definition of astronomy as 'those parts of human interest in celestial phenomena which are amenable to mathematical treatment',[5] the Presocratic model is astronomical from the start, since Anaximander proposed a clearly defined geometrical system of cylindrical earth and rotating astral rings with relative dimensions specified by numerical ratios. Against Dicks, and in part against Neugebauer who seems to favour a similar dichotomy, I shall here defend the traditional view that the history of Greek astronomy before 400 B.C. cannot be treated in isolation from the history of Greek philosophy, and that the cosmologists made a contribution of decisive importance for the development of science by proposing their kinematic models for the universe. If anyone wishes to call the theory of eclipses 'pre-scientific' before it reaches the stage of precise predictions, we need not quarrel about a name. What is important to note is that the possession of a generalised model, like that of Anaxagoras, is a first and necessary step not only to a more refined theory of eclipses but also to a number of other major advances, such as the precise measurement of the size and distance of the moon. For this and other reasons, I think we would be ill-advised to abandon the traditional view which sees the history of Greek astronomy as a continuous development from the Milesians to Ptolemy and which regards the speculative constructions of the Presocratics as the natural ancestors and progenitors of the more solidly based systems of Hellenistic times.

One merit of Dicks' paper is to raise certain general questions of historical method. He describes two errors of anachronism against which every historian of science should be on his guard: (1) 'a failure to understand what knowledge was possible at a particular epoch ... with regard to the historical development of scientific ideas', and (2) 'a failure to recognise the tacit assumptions, based on the scientific theory of late antiquity or even (sometimes) of our own times, that underlie so much of the writing about early Greek science'.[6] As stated, the first principle of anachronism seems circular, since it presupposes that we already *know* what course the development of ideas actually followed. But the second principle is valid, and can be reformulated so as to include what Dicks probably intended by the first principle as well. We might agree on the following maxim of method: the historian of science should indicate as clearly as possible just what level of knowledge or theoretic insight he is ascribing

[4] Dicks 39.

[5] O. Neugebauer, 'The History of Ancient Astronomy, Problems and Methods' in *Journal of Near Eastern Studies* iv (1945) 2.

[6] Dicks 27 and 29.

to the scientist or period which he studies. He should distinguish sharply between this explicit ascription of knowledge and the convenient but potentially misleading usage by which he describes an early result in the conceptual terms of a later stage of science. And it follows from this maxim that the historian will ascribe to any given time or scientist only that degree of explicit knowledge and insight which he is willing to incorporate in a coherent account of the development of scientific ideas for the period in question. It is easier to formulate these principles than to follow them consistently, but it is surely important to *try*. Applying them to my own account of Anaximander, I shall respond to Dicks' criticism by clarifying an ambiguity which was present in my earlier treatment of the obliquity of the ecliptic. On the other hand, I shall argue that, in the light of these same principles, Dicks' chief thesis about the radically different scientific status of equinoxes and solstices is entirely mistaken. And by avoiding the more strident tones of personal polemic, I hope to steer the controversy in a constructive direction.[7]

Let us begin with the question of the ecliptic, since it raises most of the general issues. Aëtius, our most unreliable source, tells us that according to Anaximander the wheel-like circle of the moon, like that of the sun, 'lies aslant'.[8] We might ignore this as simply a confusion of Aëtius if we did not have Pliny's statement that Anaximander had recognised the obliquity of the zodiac (*nat. hist.* ii 31 = DK 12A5: *obliquitatem eius* [*sc. signiferi*] *intellexisse, hoc est rerum foris aperuisse*), with a precision of date (548–545 B.C.) that suggests a good source, since it corresponds to the only other date preserved for Anaximander (from Apollodorus: DK 12A1.2). Behind Pliny's note, then, stands some Hellenistic history of astronomy, or biography of philosophers, and we have no reason to suppose *a priori* that information from this source must be unreliable. Aëtius may be drawing on the same source as Pliny, or on a parallel tradition. In either case, there is the possibility that the information in question goes back to Eudemus or to Theophrastus.[9] Hence I see no reason to take it as 'obvious that the words *κείμενον λοξόν* are a late addition in the doxographical tradition, inserted by someone who was so familiar with the slanting ecliptic of late Greek astronomy that he could not conceive of its not being a well-known concept in this early period'.[10] On the contrary, the point of Pliny's remark is that the concept was not known at all until Anaximander; and the related notice in Aëtius ii 12.2 also emphasises the importance of this as an *innovation* (although the discovery is there attributed to 'Pythagoras'

[7] Thus I shall not follow the precedent set by Dicks' article, which accuses me not only of systematic error and 'lack of historical sense', but also of selecting and rejecting doxographical material on the basis of personal preference in order to produce a 'monstrous edifice of exaggeration', 'a travesty of the historical truth', whose key features are 'entirely unsubstantiated by the available evidence' (Dicks 31, 35–38). One hopes that this vehemence of tone reflects Dicks' sense of the importance of the questions at issue between us.

[8] DK 12A22 = Aëtius ii 25.1 (*κύκλον τῆς σελήνης*) *πυρὸς πλήρη καθάπερ τὸν τοῦ ἡλίου, κείμενον λοξόν, ὡς κἀκεῖνον*.

[9] For Aëtius' dependence on Theophrastus in this context see Aëtius ii 20.3 = Theophr. *Phys. Op. fr.* 16. Eudemus' report on the early investigation of the ecliptic, as given by Theon of Smyrna, is also reflected (and distorted) in Aëtius ii 12.2: *Πυθαγόρας πρῶτος ἐπινενοηκέναι λέγεται τὴν λόξωσιν τοῦ ζῳδιακοῦ κύκλου, ἥντινα Οἰνοπίδης ὁ Χῖος ὡς ἰδίαν ἐπίνοιαν σφετερίζεται.* (DK41A7; see Wehrli, *Eudemos fr.* 145 with commentary, p. 120.) Theon's version is *Εὔδημος ἱστορεῖ ἐν ταῖς ᾿Αστρολογίαις ὅτι Οἰνοπίδης εὗρε πρῶτος τὴν τοῦ ζωδιακοῦ διάζωσιν*, where Diels read *ζωδιακοῦ λόξωσιν* on the basis of the parallel in Aëtius. Now if Eudemus regarded Oenopides as the first to observe *the fact that the sun's annual path is set at an angle* to the line of diurnal motion, or to the equator, he cannot have ascribed the same discovery to Anaximander. But there are other possible interpretations of Theon's report: (1) What Eudemus attributed to Oenopides was not the mere discovery that the zodiac is inclined but a precise determination of the angle (so von Fritz in PW xvii 2260 f., followed by W. Burkert, *Weisheit und Wissenschaft* 285, n. 42); (2) keeping *διάζωσις* in Theon's text, we might suppose that what Eudemus (or Theon) meant by the 'belting' of the zodiac was a definite description or measurement of the zodiacal signs. However, the whole context in Theon makes a very unreliable impression as a 'fragment' of Eudemus. Finally, even if Eudemus did *not* ascribe knowledge of the ecliptic to Anaximander, the common source of Pliny and Aëtius may have drawn this conclusion—rightly or wrongly—from information in Theophrastus.

[10] Dicks 35 f.

or Oenopides). Our sources are unanimous in asserting that knowledge of the zodiac and its obliquity was *developing* between 550 and 430 B.C., though they do not agree on—and indeed, do not attempt to specify—just which discoveries were made when.

Hence we must apply our maxim and make quite explicit what astronomical knowledge is implied by the doctrine of the obliquity of the zodiac. I inferred earlier that the inclination in question must be that of 'the ecliptic relative to the diurnal path of the stars' (*Anaximander* 88), and spoke of the sun's circle as lying aslant the celestial equator (*ibid.* 86 and 103 n. 2). Dicks observes correctly that I had not grasped the implications of ascribing such a doctrine to Anaximander; I was simply following Heath.[11] What neither Heath nor I made clear was that this way of describing Anaximander's view is equivalent to making him the discoverer of the celestial sphere as a geometric model for representing the diurnal motion. For, strictly speaking, the obliquity in question is defined only by reference to the celestial equator.[12]

Let us call this interpretation of the ecliptic as a circle on the celestial sphere the *maximum* interpretation of Anaximander's alleged discovery. What must now be emphasised is that there is a weaker interpretation of the expression 'lies aslant' which does not ascribe so much theoretical insight to our Milesian astronomer. And furthermore, there is another sense in which the zodiac or ecliptic could be discovered without any reference to its obliquity at all. First the weaker interpretation of *κείμενον λοξόν*.

Suppose that the circle or wheel of sun (or moon) is designed to explain not its annual (monthly) path among the stars but simply its apparent daily motion. Then 'aslant' will mean that the circles of sun and moon lie *aslant the earth*, i.e. inclined to the plane of the horizon, just as the daily motion of the stars is itself 'tilted' with respect to the visible surface of the earth.[13] Now the tilting of the heavens is frequently mentioned in the doxography for the Presocratics, as a phenomenon which must be accounted for in cosmogony.[14] However, the term for this general tilting is always *ἔγκλισις*, whereas *λοξὸς* (*κύκλος*) is the technical expression for the obliquity of the ecliptic relative to this general inclination, and never, as far as I can see, a designation for the tilting itself. Hence the text of Aëtius clearly favours the stronger interpretation. It is again this strong interpretation, in terms of intersecting circles on the celestial sphere (or in terms of rings set obliquely in some more complex model) which Pliny or his source must have in mind when he describes Anaximander's insight as 'opening the door to ⟨our knowledge of⟩ the nature of things'. Thus, even if we prefer for historical reasons to assign the weaker interpretation to Anaximander, we cannot extract it from our sources. The weaker view of 'lies aslant' is, by comparison with the

[11] Heath, *Aristarchus* 36: 'the hoops remain at fixed inclinations to the plane of the equator'.

[12] Hence I do not see the point of Dicks' complaint (36) that I 'cannot envisage the ecliptic without mentioning the equator, although there is not a word about this in the original quotation'. How does Dicks interpret the reference to obliquity in Pliny and Aëtius? Geminus says the zodiac is called *λοξὸς διὰ τὸ τέμνειν τοὺς παραλλήλους κύκλους* (*Elementa astronomiae* ed. Manitius, v 53). One of the parallels it cuts is of course the equator.

It should also be noted that to attribute to Anaximander the concept of the ecliptic is actually to credit him with *more* than the celestial sphere and the inclined zodiac. See below, esp. n. 24.

[13] I owe this suggestion to Howard Stein, who writes: 'It seems to me that this fits better with Anaximander's attribution of the solstices to meteorological causes: the circle of the sun, remaining always parallel to the circles of the stars, moves as a whole among (or rather above) the stars, towards the north in spring, then, turning, towards the south under the influence of the exhalations; and all the while, the circle turns on itself, one revolution per day.'

[14] See, for Anaxagoras, DK 59A1.9 (*τὰ δ' ἄστρα κατ' ἀρχὰς μὲν θολοειδῶς ἐνεχθῆναι . . . ὕστερον δὲ τὴν ἔγκλισιν λαβεῖν*); similarly *ἐγκλιθῆναί πως τὸν κόσμον ἐκ τοῦ αὐτομάτου* in 59A67. The former passage is from Diogenes Laertius, the latter from Aëtius. The same two authors ascribe a similar doctrine to the atomists (Aëtius iii 12 = DK 67A27 and 68A96), and once to Empedocles (Aëtius ii 8.2 = DK 31A58); in Aëtius it is generally the earth rather than the heavens which is said to be tilted. The ancient sources do not clearly distinguish such *ἔγκλισις* from the obliquity of the zodiac, but an outright confusion of the two nowhere occurs except in the proposed emendation of Diogenes ix 1 printed in DK 67A1.33 *⟨τὴν δὲ λόξωσιν τοῦ ζωδιακοῦ γενέσθαι⟩ τῷ κεκλίσθαι τὴν γὴν πρὸς μεσημβρίαν*.

other version, astronomically trivial. If Anaximander introduced celestial wheels to explain the diurnal motion of sun and moon, then he was *obliged* to set them inclined to the horizon. For better or worse, however, what the Hellenistic sources ascribe to him is the discovery of obliquity in the stronger sense, which presupposes that the tilting of the diurnal motion had *already* been recognised.

At this point we should make clear that there is another sense in which the ecliptic or rather the zodiac could have been, and almost certainly was, discovered without any reference to its obliquity. Even to speak of the 'ecliptic' in connection with early Greek astronomy may be misleading since the term ἐκλειπτικός, which reflects the knowledge that this is the only circle in which eclipses can occur, is not used until much later. The common expression for the sun's path is ὁ διὰ μέσων τῶν ζῳδίων (κύκλος), 'the circle through the middle of the zodiacal signs', which clearly shows that the zodiac is the primitive concept.[15] What we are concerned with, then, is the first recognition of the fact that the movements of the sun and moon (and, later, of the planets also) lie within a given path or zone marked by conspicuous stars. Let us call this 'the empirical discovery of the zodiac'. Now this discovery is clearly independent of any spherical model for the heavens, and even of any particular identification or division of the zodiacal constellations. It requires only *some* identification of stars lying along the annual path of the sun. It is in this sense that the zodiac was discovered by the Babylonians. Note that this is not yet the discovery of the ecliptic or *circle of the sun*; the latter requires a recognition that the sun always moves not merely *within* the zodiacal band but *always on a single line*, namely, on the great circle in the middle of this band. In fact, it seems that the Babylonians first recognised the zodiac as the 'path of the moon', marking the constellations through which the moon passes once each month. These so-called stations of the moon are much easier to observe than the sun's path since the circuit is completed more frequently and the relevant stars are often visible at the same time as the moon. One of the Babylonian tablets known as 'Mul apin' apparently specifies that the sun and the five planets ('all six gods') travel in the Way of the Moon, which is here identified by 17 constellations, 14 of which lie in the zodiac.[16] This text makes clear that the empirical zodiac, as the path of sun, moon and planets, was known to the Babylonians by the seventh century B.C.; in fact the information contained in the text probably goes back to the second millennium.[17] At some point the zodiac was limited to

[15] See Arist. *Met.* 1073b20, Euclid, *Phaenomena* p. 6, 21 ff. (ed. Menge), Geminus, *Elementa* v 51. Even technical authors do not always make a clear terminological distinction between the zodiac and the sun's circle, but use an expression for the former (ὁ τῶν ζῳδίων κύκλος, ὁ ζῳδιακός) in asserting something true only of the latter (e.g. that it is a great circle, Euclid *op. cit.* 8, 15; that it touches the tropic at a solstitial point, Achilles *Isagoge* xxv 4, ed. Maass p. 57.21). It seems to have been understood that in referring to celestial κύκλοι that are really bands, such as the zodiac and the Milky Way, the astronomers normally take the middle circle of the band as its geometrical representation.

[16] See E. F. Weidner, *Amer. Journal of Semitic Languages and Literatures* xl (1924) 192–5; J. Schaumberger, in *Sternkunde und Sterndienst in Babel iii* (1935) 319; and now B. L. van der Waerden, *Erwachende Wissenschaft Band ii*: *Die Anfänge der Astronomie* (1966) 77. This last work will be quoted below as 'van der Waerden (1966)'.

[17] See Weidner, *op. cit.*, and B. L. van der Waerden, *Journal of Near Eastern Studies* viii (1949) 6–26. The same tablet divides the year into four periods of three months each during which the sun is described as located in the path of Anu (twice, in spring and fall), in the path of Enlil (summer), and in the path of Ea (winter). Since the path of Anu must thus represent an equatorial belt some 30 degrees wide, the statement that the sun moves in and out of this path can be interpreted as showing that the Babylonians knew (by Assyrian times, and perhaps much earlier) 'that the sun moves in an oblique circle' (van der Waerden, *op. cit.* 24). This is, however, a very *Greek* way of describing their knowledge. As far as I can judge, the Babylonian texts quoted do not refer to the sun's path as a circle; there is no mention of the equator, and the concept of obliquity is at best implicit in the recognition that the sun passes back and forth across the (presumably parallel) borders of the way of Anu.

In his 1966 book van der Waerden has developed the consequences of his own description and explicitly assigned not only the obliquity of the ecliptic but also the celestial sphere to the Babylonian astronomers of the 'Mul apin' period (78 f. with fig. 10; and 134).

the 12 canonical constellations and eventually to 12 equal *signs*, divided into 30 degrees each. The conventional use of a zodiac of $12 \times 30 = 360$ degrees does not by itself constitute a new level of astronomical *knowledge*, but it is of considerable historical importance both as an index of Babylonian influence in Greece (and elsewhere) and as the expression of a new desire for systematic quantitative precision in observing the position of sun, moon and planets. The date of this mathematical zodiac in Babylon is still contested,[18] and it does not appear in Greece before the early third (or, according to Dicks, 28 n. 15, the second) century B.C. In the present context, it should be clear that all references to the zodiac are to the earlier, less precise system of familiar constellations marking the path of sun and moon.

When was this empirical knowledge of the zodiac available to the Greeks in some form? There is no trace of the corresponding constellations in Hesiod, but the Scorpion appears in a quotation from the astronomical poem of Cleostratus of Tenedos, from the late sixth century apparently, and two other zodiacal constellations (Aries and Sagittarius) are ascribed to him by Pliny (DK 6B1 and B2). This same Cleostratus is mentioned by Theophrastus in a list of observational astronomers preceding Meton.[19] Now the designations of Scorpio and Sagittarius derive from Babylon, and it is not likely that Cleostratus has hit upon them by himself.[20] Whether or not Cleostratus knew that these constellations marked the sun's path cannot be ascertained from the insignificant remains of his poem, but it is difficult to suppose that a practising astronomer would *not* have received this information together with the names of the constellations. There are two other bits of evidence for Greek astronomy around 500 B.C. which also point to Babylonian influence (leaving aside the question of the gnomon and *polos*, to which we shall return): (1) the use of an 8-year intercalation cycle for the soli-lunar calendar, again associated with the name of Cleostratus, and (2) the knowledge that Morning and Evening Star are one and the

If this view were correct, the whole question of the originality of Greek astronomy would have to be regarded in a new light. But it seems unlikely that van der Waerden's conclusion will prove acceptable to other historians of Babylonian science. I have the impression (from a conversation with Dr Heintz) that the paths or zones of the 'Mul apin' text can be understood in purely observational terms (e.g. as designating which stars rise and set together) without reference to any geometric model.

[18] See van der Waerden (1966) 125; and compare O. Neugebauer, *The Exact Sciences in Antiquity* (2nd ed. 1957) 140. I regret that I helped confuse matters by quoting from the 1st edition (1952) of Neugebauer's book (in *Anaximander* 92); and indeed my entire quotation there is regrettable since, in either edition, Neugebauer is referring to a level of astronomical refinement at the end of what he calls the 'prehistory' of Babylonian astronomy (dated to 'about 400 B.C.' in the second edition, 103) which was certainly *not* reached in sixth-century Miletus, and perhaps nowhere in Greece before the time of Meton.

Nonetheless the point which I intended to illustrate by the quotation from Neugebauer is one which I still maintain: that the creation, by the Milesians and their successors, of a theory of geometric world-models different *in kind* from mythic speculation is to be understood in part as the Greek reaction to new and more extensive contact with astronomical lore from the East. For a more modest estimate of the knowledge available to the Greeks in the sixth century, see below.

[19] DK 6A1. Dicks 26 f. is very contemptuous of Cleostratus, and of those modern historians who take him seriously, on the grounds that our authority, Pliny, is unreliable. But Dicks ignores this reference to Cleostratus in Theophrastus and the surviving verses from his poem which mention the Scorpion. The fact that Atlas is included by Pliny in the same context as discoverer of the celestial globe (by obvious rationalising of a well-known story derived from Hesiod, and often represented in vase-painting and sculpture) shows that Pliny is uncritical of his sources, but it cannot be used to impugn his authority wholesale. Like Diogenes Laertius, Pliny repeats whatever he has found written somewhere, and what he has found is often silly. But each case must be judged on its merits, and in this case, what Pliny tells us about Cleostratus is just what we would expect on the basis of the other evidence.

[20] See A. Rehm, *Abh. bayer. Akad.* (Munich 1941) Heft xix 12–14. Other discussions of Cleostratus are cited in W. Burkert, *Weisheit und Wissenschaft* 312 nn. 56 and 58. For a comparison of Greek and Babylonian constellations in the zodiac, see van der Waerden, *Journal of Near Eastern Studies* viii (1949) 13 f. and now van der Waerden (1966) 256 ff.

same, first attested for Parmenides in Greece but known since the second millennium in Babylon.[21]

Now it would be possible to suppose that any *one* of these items had been discovered independently by the Greeks of the sixth century: the constellations of Scorpio and Sagittarius, the eight-year cycle, and the identity of Evening and Morning Star. Taken together, however, they point unmistakably to a new if rudimentary acquaintance with Babylonian astronomy, an acquaintance reflected in Herodotus' statement that it was from the Babylonians that the Greeks derived their knowledge of the gnomon, the *polos*, and the division of the day into 12 parts (ii 109). It is almost certain that they learned of the empirical zodiac at the same time, in the sixth century. I take it that this is one of the things Parmenides is alluding to when he makes his goddess promise to reveal 'all the signs in the *aither* and the obscure deeds of the pure torch of the brilliant Sun . . . , and the circling deeds of the round-faced Moon (B10 *εἴσῃ . . . τά τ' ἐν αἰθέρι πάντα/ σήματα καὶ καθαρᾶς εὐαγέος ἠελίοιο/ λαμπάδος ἔργ' ἀίδηλα καὶ ὁππόθεν ἐξεγένοντο,/ ἔργα τε κύκλωπος πεύσῃ περίφοιτα σελήνης*). The deeds of the sun are *ἀίδηλα*, both 'invisible' and 'making invisible', because the zodiacal constellations through which it moves are blotted out by the brilliance of its torch, whereas these constellations *are* observable in the course of the moon's peregrinations as marked by her phases: *κύκλωψ* here plays upon the fact that the moon's cyclical progress (*περίφοιτα ἔργα*) through the zodiac is reflected in the periodic changes of her face.[22]

If such items of old Babylonian lore had reached Parmenides in distant Elea by the beginning of the fifth century (as the identity of Venus in her two appearances certainly *had* reached him), then it is surely more plausible to assume that this knowledge was transmitted through Miletus (which provides so much of the background for Parmenides' work) than to imagine some other, entirely unattested channel for scientific ideas passing from the Persian empire to southern Italy. (Pythagoras comes to mind, but this would not be a genuinely alternative route. Pythagoras' island of Samos is practically within sight of Miletus.) I suppose that the Milesians were familiar with the zodiac in the descriptive sense if they were doing any astronomy at all.[23] Now to proceed from the empirical zodiac to a discovery of its obliquity only one step is required: either to conceive somehow of a division of the sky oriented parallel to the equator but without a spherical model, as the Babylonians are said to have done with their 'path of Anu', or to conceive the fixed stars as lying on a sphere. The nature of the Babylonian solution is not entirely clear, and there seems to be no trace of it in Greece. For Greece, then, the discovery of the obliquity, given some knowledge of the empirical zodiac, is just the discovery of the fact that the diurnal movement of the stars can be accounted for by the rotation of fixed points in a celestial sphere. For as soon as the stellar sphere is so conceived, the empirical zodiac will appear on it as a great circle inclined to the equator.[24]

[21] See Cleostratus DK 6B4, Parmenides DK 28A1.23 and A40a. For the Venus observations in early Babylon, see A. Pannekoek, *A History of Astronomy* (1961) 33; van der Waerden (1966) 49. For the Babylonian 8-year cycle, Pannekoek 51 f. and van der Waerden (1966) 112. Dicks is sceptical of the 8-year cycle (33, n. 39), but the convergence of Geminus, Censorinus, and the evidence from Babylon should suffice to establish its chronological priority over the Metonic cycle.

[22] In the case of *κύκλωψ* as in the case of *ἀίδηλα* I see an intentional use of ambiguity or plurisignificance, which is an essential feature of Parmenides' style too often overlooked by commentators. For *κύκλωψ* there are at least two appropriate meanings: (1) 'round-faced' or 'round-eyed' (so Diel-Kranz), and (2) 'cycle-faced', i.e. changing her appearance according to the monthly cycle. The first is the surface reading, on the level of Parmenides' Homeric diction; the second is the deeper reading, on the level of his astronomical concerns. For further remarks on intentional ambiguity in Parmenides see my review of J. Bollack, *Empédocle*, in *Gnomon* xli (1969) 441 f.

[23] The suggestion that the Milesians were *not* interested in observational astronomy seems to be incompatible with almost every one of the stories concerning Thales, and with the traditional ascription to him of an old 'Nautical Astronomy'. On this and other early didactic poems with scientific content, see Nilsson, *Rh. M.* lix (1904) 180–6.

[24] This is an oversimplification, since only the ecliptic is a great circle. As suggested above, we

In this sense, the discovery of the celestial sphere implies no trigonometry, no zigzag functions, and no geometry beyond what can be learned from observing a solid ball turning on an axis, where parallel circles are traced by a stationary point or stylus as the ball is rotated. What is required is not advanced mathematics but a stroke of genius, which suddenly transforms a complex mass of star observations into a scheme which is exceedingly simple and easy to grasp.[25]

Since I had not realised this connection between the obliquity of the zodiac and the spherical model for stellar motion, I claimed the former for Anaximander without claiming the latter. When properly posed, however, the question whether we can believe Pliny in ascribing the obliquity of the zodiac to Anaximander presupposes an answer to the question: did he or did he not interpret the diurnal motion of the stars by reference to a spherical model? I do not think this question can be answered definitely either way, but at least it is worth discussing.

When is the spherical model for the fixed stars first attested? Once again, the evidence points to Parmenides (*fr.* 10):

εἰδήσεις δὲ καὶ οὐρανὸν ἀμφὶς ἔχοντα
ἔνθεν [μὲν γὰρ] ἔφυ τε καὶ ὥς μιν ἄγουσ(α) ἐπέδησεν Ἀνάγκη
πείρατ' ἔχειν ἄστρων.

'You shall learn about the surrounding Heaven,
from what source it was formed, and how Necessity leads and bound it fast
to hold the limits (or fastenings) of the stars'

The goddess will explain how 'outmost Olympus' (ὄλυμπος ἔσχατος, *fr.* 11), the extreme heaven enclosing the *aither* and all the stars within the *aither*, came into being out of Night, probably as a solid sphere, and now forms the dark backdrop of the night sky to which the fiery σήματα are attached, and whose uniform rotation proceeds under the leadership of Ananke. Neither these verses nor the confused doxography in Aëtius (DK 28A37) say explicitly that the outmost heaven is spherical in form, but that is certainly the most plausible interpretation.[26] In the first place, once the stars are fastened to the οὐρανός, as Parmenides

must distinguish between (1) the discovery that the zodiac is inclined to the equator, and (2) the discovery that the sun always moves in the middle of this band *on a single great circle*. Pliny assigns only (1) to Anaximander; Aëtius combines this with (2) by referring to the distinct circles of sun and moon as 'lying aslant'. (Howard Stein reminds me that there is in fact no single circle for the moon's path, but this error could be due to Anaximander rather than to the doxography.) I am therefore inclined to suppose that Aëtius has conflated two reports, both of which were given in his sources: (i) the inclination of the zodiac, as reported by Pliny, (ii) the explanation of sun and moon as huge circles or rings, as given by other doxographers such as Hippolytus. If Aëtius is responsible for this confusion, then Anaximander may in fact have discovered the obliquity of the zodiac (or learned of it from Babylon) without discovering the ecliptic in any precise sense. He may have thought of the circles of sun and moon as always lying parallel to those of the stars, and rotating daily, but as pushed north and south in the course of the year (for the moon, in the course of the month). See below, p. 107.

[25] Compare the passage in the pseudo-Platonic *Erastai* 132a–b (DK 41.2) where two boys are said to be disputing 'about Anaxagoras or Oenopides; for they appeared to be drawing circles and representing certain inclinations (ἐγκλίσεις), by inclining their hands relative to one another (ἐπικλίνοντε), all in great earnestness'. Our question is: who was the first *scientist* to do for the zodiac, or for the ecliptic, what our author imagines *schoolboys* doing in the latter part of the fifth century?

[26] In favour of the spherical shape for Parmenides' οὐρανός see Heath, *Aristarchus* 69; and now L. Tarán, *Parmenides* (1965) 241: 'a solid sphere of Night'. Burnet's case against the spherical heaven (*EGP* 188 with n. 2) relies entirely upon the untrustworthy wording of Aëtius, who compares the outer circle to a τεῖχος or city-wall. I have argued (*Anaximander* 116 f.) that spherical shape is first assigned to the earth (by Parmenides or by Pythagoreans) as a generalisation of the principle of cosmic symmetry.

After Parmenides, the spherical shape is reasonably well attested for Empedocles. See *fr.* 38.4 αἰθὴρ σφίγγων περὶ κύκλον ἅπαντα. A spherical οὐρανός for Empedocles is presupposed by the reference to hemispheres in DK 31A51 (Aëtius); that the fixed stars are 'bound' to it is stated in 31A54 (also Aëtius).

says they are, no other simple, symmetrical shape will readily account for the phenomena. And in view of the unique importance of the sphere as a symbol of rational order and symmetry for Parmenides (as later for Empedocles), it is hard to believe that he did *not* have the celestial sphere in mind here as the visible work of Ananke, the paradigm of immutable recurrence.[27]

In all probability, then, the stellar sphere is attested for Parmenides. What are the chances that it was known earlier, in Miletus? The evidence is partial and inconsistent. I mention the positive indications for what they are worth.

1. Anaximander's world-model is spherical in some sense, since the earth remains at rest in the centre because of its symmetrical distance 'from everything', in all directions.[28] Distance from what exactly? Perhaps from 'the rings of the heavenly bodies, of which the sun's is the largest', as Kirk and Raven suggest.[29] Simpler and more natural, however, is the idea of a central position within a celestial sphere (as in Plato's restatement of the argument from ὁμοιότης and ἰσορροπία at *Phaedo* 109a).

2. The fixed stars, we are told, were explained by κύκλοι or wheels, derived (like sun and moon) from the separation or sectioning of a primordial 'sphere of flame' (φλογὸς σφαῖραν DK 12A10). Now the wheels of the fixed stars are presumably oriented so as to account for the diurnal motion. That means that they are, in effect, set perpendicular to the axis of rotation. If these circles are all assumed to have the same diameter, the result will be a cylinder. But a cylindrical model is unattested, and it would in any case be incompatible with Anaximander's conception of celestial symmetry, since the cylinder would have to be set obliquely to the plane of the earth. The only natural solution is to suppose that the circles were in fact *smaller* near the pole, larger near the zenith. And this assumption is roughly equivalent to placing them in a celestial sphere. If Anaximander himself did not see this, his own construction of the stars as rings could easily have led the next man to introduce a spherical model as a simplification of the whole scheme. That the spherical model is Anaximander's own, however, is suggested by his use of the 'sphere of flame' in explaining the origin of the wheels.

3. Anaximander himself is said to have constructed a σφαῖρα, a celestial globe.[30]

4. When we turn to Anaximenes, we are told that the stars are 'fastened like nails in the crystalline (or ice-like)' circumference of heaven.[31] This has often been interpreted as 'the

The conflicting view reported by Aëtius in another context (31A50), that Empedocles' heaven is egg-shaped, must be mistaken, and is in fact contradicted by other evidence in Aëtius (*cf.* the σφαῖρα enclosing the sun in A58 with the statement in A50 that the circuit of the sun marks the limit of the κόσμος).

[27] If, as I suggest, Parmenides was working with the model of a celestial sphere on which the ecliptic or zodiac is drawn obliquely, there may be a grain of truth to Strabo's report (DK 28A44a), on the authority of Poseidonius, that Parmenides was the inventor of the division into five zones. The only anachronism may lie in Poseidonius' application of this to the earth (although, if Parmenides' earth was spherical, as it is reported to have been, even the projection of the zones on to the earth is not *impossible* for his time: it may at first have been done *a priori*, without latitude observations). Three of the five celestial zones are given as soon as the ecliptic is drawn. The other two are easily defined: the arctic region where the stars never set and the antarctic where they never appear (to an observer, say, in Elea). The fact that Parmenides is said to have placed the tropics too far from the equator—i.e. that he had too high a value for the obliquity of the ecliptic—perhaps tells against this being a late fabrication. Note that Parmenides' error could easily be explained by the assumption that the ecliptic, as a single circle, had not yet been distinguished from the wider zodiacal band. In that case, the true discovery of the *ecliptic* (and its obliquity) would belong after all to Oenopides in the middle of the fifth century. This discovery would naturally be connected with an Anaxagorean investigation of the precise circumstances of lunar eclipse. See n. 25 above.

[28] Evidence and discussion in *Anaximander* 53–5. On this point even Dicks is not sceptical, though he doubts whether the argument from symmetry should be regarded as a *mathematical* insight (36 n. 53).

[29] *The Presocratic Philosophers* 134 f.

[30] DK 12A1.2 (Diogenes Laertius). Also Pliny *nat. hist.* vii 56.203 cited in *Anaximander* 60: *sphaeram invenit.*

[31] DK 13A14 (Aëtius): *ἥλων δίκην καταπεπηγέναι τὰ ἄστρα τῷ κρυσταλλοειδεῖ.*

conception of the stars being fixed on a crystal sphere as in a rigid frame'.[32] This interpretation has been doubted, by Burnet and others, on the grounds that the theory of a celestial sphere is incompatible with Anaximenes' view that the stars go *around* the earth rather than under it.[33] These grounds are inconclusive, as will now be seen.

5. Anaximenes held that 'the stars do not move under the earth, as others had supposed, but around it, as a cap (πιλίον) turns around our head. The sun is hidden not because it passes under the earth, but by being covered by the higher parts of the earth and by its greater distance from us' (DK 13A7.6, Hippolytus). Is this description of the stars' motion really incompatible with a spherical model? Not if the sphere is assumed to be very large in relation to the earth, for then very few stars will actually pass directly *under* the earth. In contrast to the naïve view, which thinks of the sun and stars as simply 'rising', i.e. moving up vertically in the east, setting (vertically) in the west, and passing underneath the earth, Anaximenes may have been trying to convey the correct picture of the diurnal motion in parallel circles, inclined to the earth's surface so as to rise towards the south and descend towards the north. If the comparison to a cap or beret is due to Anaximenes, he may have been explaining the crucial idea of the 'tilting' of celestial motions relative to the horizon which is so frequently mentioned in Presocratic cosmology.[34]

6. Finally, if the Milesians thought of the heavens as spherical, we would have a natural explanation for Xenophanes' choice of that shape for his cosmic god, assuming (as I do) that Theophrastus has correctly understood him.[35] Xenophanes' 'greatest god' cannot himself be identical with the astral sphere, since he 'moves not at all' (*fr.* 26). But he causes this sphere—and everything else—to move (*fr.* 25), and is naturally thought of as lying invisibly around it, like the surrounding Boundless of Anaximander.

No one of these points is decisive in itself. Together they constitute a body of circumstantial evidence in favour of the view that the spherical model for the heavens was invented in Miletus, whether by Anaximander or Anaximenes. The case is not air-tight, for evidence can also be found which tells *against* this view. In the first place, there is the doxographic statement that Anaximander placed the fixed stars below the sun and moon. This is not strictly incompatible with a spherical model, but it makes it considerably less plausible. Secondly, there is the problem, for both Milesians, of the explanation of the sun's τροπαί by meteorological causes. Here again we have no formal contradiction but rather an entirely non-geometrical way of envisaging celestial phenomena. But in the case of Anaximander we have the apparently hopeless dilemma: either the circles of sun and moon were set parallel to the stellar circles, in order to explain their diurnal motion (and in that case κείμενον λοξόν is just a mistake) or else these circles are set obliquely (as λοξόν implies), and in that case they do *not* explain the diurnal motion of sun and moon without further, complex combinations of which the doxography offers us no hint. In view of these discrepancies, and in the absence of more detailed information, we simply cannot tell how the various

[32] Heath, *Aristarchus* 45, following Tannery.

[33] Burnet, *EGP* 77 with n. 4. Similarly Kirk and Raven, 155. For a different view, see W. K. C. Guthrie, *A History of Greek Philosophy* i (1962) 135–8.

[34] For the tilting, see above, n. 14. My comments on Anaximenes are based upon a suggestion of Howard Stein, who writes: 'If I were trying to convey to a student the basic notion, and the chief difference from the naïve view, the first point I should make is that the stars do not, in the naïve sense, "rise" and "traverse the sky" and "go down" and "return underneath", but in a more accurate view they *go around*; the ones near the pole go around remaining always in view, while the ones farther from it are hidden part of the time by the northerly parts of the earth, since in their turning they *sink behind it.*' Anaximenes' metaphor of the cap revolving on our head 'approaches the metaphor of the celestial sphere. A cap, after all, sits back on the head: it goes low in back and high in front. If a cap is twisted around on the head in the most natural way, it stays "on" the head (does not go "under" it), but the part that goes around in back does also sink down low "behind" the head. The very fact that later commentators seem mystified by this phrase prevents any suspicion here of a backwards attribution of later ideas.'

[35] See σφαιροειδῆ in DK 21A33.2 (Hippolytus), with the parallels cited by Diels in note on *Doxographi graeci* 481.9.

aspects of Milesian cosmology fitted together. What we can say is that it is certainly *possible* that either Anaximander or Anaximenes explained the diurnal movement by a spherical model, and perhaps more *probable* that this innovation was made in sixth-century Miletus than at any later moment in the history of Greek astronomy—unless we wish to appeal to the legendary exploits of Pythagoras or the unrecorded theories of his school before Philolaus.[36]

If the Milesians invented the model of a stellar sphere, I think it is sufficiently obvious in what sense they contributed to the development of scientific astronomy in Greece. For the conception of a rigid sphere for the fixed stars 'remained the fundamental postulate of all astronomy up to Copernicus',[37] as it remains still the convenient assumption in descriptive astronomy. Even if they did not achieve this insight themselves, however, they clearly prepared the way for it by the attempt to present *some* geometric model as an explanation of the observed motions—and indeed, a model based on circular motion. As Neugebauer has observed, the assumption of circular motion for the heavenly bodies 'remained the cornerstone of celestial "dynamics" of ancient astronomy comparable to a law of inertia'.[38] And it is just here that the principal difference lies between Greek and Babylonian astronomy. At its highest stage of perfection, as Dicks points out, Babylonian astronomy operated as far as we know 'without any underlying cinematical model at all: . . . without any knowledge of the fundamental concepts of the spherical earth set in the middle of the celestial sphere, of the obliquity of the ecliptic, and of geographical latitude and longitude; these are all *Greek* discoveries and in comparison with their fertility, the arithmetical methods of Babylonian astronomy proved sterile'.[39] On this point, apparently, all can agree. But I ask: when were these decisive discoveries made? The spherical earth is presented in the *Phaedo* as a familiar concept in the fifth century (*Phaedo* 97d8); a good tradition ascribes it to Parmenides.[40] The celestial sphere is also, as we have seen, as old as Parmenides and probably older. The obliquity of the ecliptic *may* have been recognised as soon as the sphere was understood; it was known at latest to Oenopides in the middle of the fifth century. Thus, except for terrestrial latitude and longitude (which required the refined mathematical techniques of Hellenistic times), *all* of the fundamental concepts which distinguish Greek

[36] I have not touched upon the possibility of significant work in astronomy by Pythagoras or Pythagoreans in the period between Anaximenes and Parmenides, since the evidence for such is practically non-existent. This does not mean that there *was* no Pythagorean astronomy around 500 B.C.; only that we can scarcely hope to know anything about it. B. L. van der Waerden, in *Die Astronomie der Pythagoreer* (1951) 28 f. and PW xxiv 290–4, reconstructs an earlier and a later Pythagorean world-system, but he does not attempt to specify how old the early system is. If we could trust Aëtius' statement (DK 24A4) that Alcmaeon recognised that the movement of the planets was in the reverse direction to that of the fixed stars, and *if* we were sure of the early date of Alcmaeon, we would have a faint glimmer of pre-Parmenidean astronomy in Magna Graecia.

[37] Heath, *Aristarchus* 45.

[38] *The Exact Sciences in Antiquity* 155.

[39] Dicks 39 n. 64. *Cf.* the remarks of Neugebauer cited over n. 45. But it now seems that van der Waerden would dissent; see his new thesis on the geometric elements in Babylonian astronomy (1966), 134, and my comment above, n. 17.

[40] See *Anaximander* 115. Neugebauer's statement that the discovery of the sphericity of the earth was 'recent' in the time of Eudoxus is one for which I can find no evidence (*The Exact Sciences in Antiquity* 153). A reader points out that *Phaedo* 97d8 *πότερον ἡ γῆ πλατεῖά ἐστιν ἢ στρογγύλη* would not be decisive if the words could refer to a choice 'between the two kinds of flat earth, the traditional one, round like a penny, and an oblong rectangular one, such as Herodotus believed in'. At the dictionary level, perhaps, the words might mean this (just as 'the earth is round' in English might mean 'the earth is disk-shaped'), but in the context of the *Phaedo* such an interpretation is implausible, to say the least. The words quoted express *the first burning question in cosmology* which Socrates wished to have Anaxagoras decide on the basis of a rational order for the whole universe; and the immediately following questions are concerned with the position of the earth and the position and movements of the astral bodies. In this connection, the hypothesis of an oblong earth is of no interest, but the question of spherical shape is of very great interest indeed, and is, with the position of the earth, the first question to be decided when the subject is taken up again at *Phaedo* 108e5.

from Babylonian astronomy were developed before Meton's work in 432 B.C., i.e. in what Dicks calls the 'pre-scientific stage' of Greek astronomy.

Neugebauer has recently contrasted the Greek reactions to Babylonian science in mathematics and in astronomy. In the first case, 'the discoveries of the Old Babylonian period had long since become common mathematical knowledge all over the ancient Near East'.[41] What the Greeks added to the mass of geometrical, algebraic, and arithmetical information available was 'a fundamentally new aspect . . . , namely the idea of general mathematical proof. It is only then that mathematics in the modern sense came into existence'.[42] In regard to astronomy, however, the situation seems very different. Here Neugebauer sees 'the really significant contribution of Babylonian astronomy to Greek astronomy . . . in the establishment of very accurate values for the characteristic parameters of lunar and planetary theory'.[43] Now this is a relatively late development in Mesopotamia that did not have any great impact in Greece until Hipparchus and his successors developed the geometrical theory required to exploit such refined empirical data. Hence Neugebauer can speak of Greek astronomy as beginning its own development *in the early second century B.C.*[44] Yet he recognises that the Greek enterprise of 'mathematical description of the motion of the celestial bodies' was completed by Ptolemy 'on the basis of cinematic models and spherical trigonometry, both unknown to his Babylonian predecessors'.[45]

I submit that, if spherical geometry and trigonometry are new creations of the Hellenistic period, the later cinematic models are direct descendants of those worked out in Miletus, Elea and Athens, from Anaximander to Eudoxus. And I would claim that, in Greek astronomy, *the idea of a geometric model for earth and heavens* played the same revolutionary role as the idea of proof in mathematics. It is in this sense that Anaximander may be regarded as the founder of Greek astronomy, just as the first practitioner of geometric proof, whoever he was, might be described as the founder of Greek mathematics. It is not surprising that the first models were relatively crude, just as the early examples of proof can scarcely have satisfied the canons of Euclidian rigour. What matters is that a beginning had been made, a beginning without which there could have been no Eudoxus, no Ptolemy, and no Copernicus.

In insisting here upon the continuity in the development of Greek astronomy from the sixth century on, I do not mean to deny the importance of the differences that emerge in the course of time and to assimilate the Presocratics to Hellenistic mathematicians. Just as the cosmic scheme of Anaximander was not geometric in the same sense as that of Eudoxus or Hipparchus, so the observational techniques of Miletus must have seemed quite unsystematic by the time of Meton. It would surely be a mistake to suggest that an attempt at very *accurate* observations played a great role in early Greek astronomy, since the notion of accuracy is itself one which evolves with the general development of science. Nevertheless, it is essential to see that, from the beginning, *some* work in empirical observation went hand in hand with the predominant Presocratic interest in a theoretical model for the heavens (and in the cosmogony which 'explains' this model), and that early improvements in the model were accompanied by important scientific discoveries, such as the illumination of the moon by the sun and the explanation of eclipse. Thus Oenopides is credited not only with the discovery or study of the obliquity and with a reasonably precise solar-lunar cycle of 59 years, but also with a highly speculative account of the origin of the Milky Way as the

41 Neugebauer, *Proc. Am. Philos. Soc.* cvii (1963) 530.

42 *ibid.* Neugebauer dates this discovery in the time of Theaetetus and Eudoxus. But the most powerful form of geometric proof, by *reductio*, is brilliantly exemplified in Zeno's paradoxes and clearly embedded in the argument of Parmenides' poem. The systematic generalisation of the notion of proof in mathematics must have been a slow development, perhaps not completed before Euclid. But the *origins* can be traced back to the early fifth century, in fact, to nearly the same time (*c.* 500 B.C.) when the celestial sphere is first attested.

43 *ibid.* 534.

44 *ibid.* 530.

45 *ibid.* 535.

former path of the sun (DK 41.7–10). And Democritus, whose cosmological theory is as bold as any, wrote a book on the planets and apparently compiled a detailed *parapegma* recording astral and meteorological observations throughout the year (DK 68A33 VIII3, III4, B11r–14. The suggestion that this is not Democritus of Abdera seems highly implausible). We find Democritus standing here next to Meton and Euctemon among the first Greek astronomers for whom systematic observations are explicitly attested.

It is hard to say how far Greek astronomy in this period moves in its own course, how far its progress depends upon new data from the East. I suggest that the Greek development should be regarded as profoundly original from its beginning in the sixth century, but as periodically fructified by successive waves of influence from Babylon. At least three of these waves make possible a decisive advance in Greek astronomy: (1) the elementary acquaintance with the zodiac, with at least one of the planets (Venus), with the 8-year cycle, and probably also with the technique of gnomon observation—all in the sixth century, (2) more precise knowledge of the five planets (and probably of their periods), together with the Metonic cycle, about 450–430 B.C., and (3) the later Hellenistic access to eclipse records and to precise data concerning lunar and planetary movements, referred to in the earlier quotation from Neugebauer.[46]

The more developed and complex a science becomes, the less a philosopher is likely to contribute to its progress. Plato and Aristotle made no contributions to astronomy, though they kept abreast of the latest developments. But the evidence suggests that, in the previous century, Parmenides, Anaxagoras, and Democritus were practising astronomers. An historian of astronomy may well complain that this early phase of Greek astronomy has been too exclusively studied as a subordinate part of the history of philosophy, and that unphilosophic astronomers like Cleostratus, Oenopides, and Meton have received far too little notice, both in ancient and in modern writing on the subject. But he can scarcely claim that the invention of the celestial sphere, the doctrine of a spherical earth, and the explanation of lunar eclipse have no place in the history of astronomy, simply because their authors or their earliest known Greek advocates happened to be philosophers.

Dicks dates the rise of mathematical astronomy to the time of Meton and Euctemon, in part because of their precise measurements of the length of month and year, giving a figure for the mean lunar month 'accurate to within two minutes'.[47] Accuracy gradually increased over the next two centuries, as Dicks points out. Are we not to suppose that the level of accuracy attained in 432 B.C. was itself the result of continued efforts and gradual improvement in the course of the preceding century? Our sources are nearly silent on such measurements, but one passage of Theophrastus does mention Meton's predecessor Phaeinos, together with Cleostratus of Tenedos and Matriketas of Methymna, each one observing

[46] See n. 43. For some suggestions on fifth-century contacts, see W. Burkert, *Weisheit u. Wissenschaft* 295. My metaphor of the three waves is of course a simplification, and is not intended to exclude more or less continuous contacts with Eastern science on the part of individual Greeks. Thus I leave open the question whether a distinct 'wave' should be recognised in Plato's old age and associated with the alleged voyage of Eudoxus to Egypt. (See, e.g. F. Solmsen, *Plato's Theology* 96, n. 23; van der Waerden [1966] 129–31.) The *Epinomis* (986e–988a) recognises that the names and knowledge of the planets originated in Eastern lands, and Aristotle in *de Caelo* 292a6–9 speaks of the elaborate observational data accumulated in Egypt and Babylon. What is not clear, however, is how much information was made available to Eudoxus and his contemporaries that was essentially *different* from the knowledge of astral cycles that had reached Meton and others in the fifth century. In any case, the really important astronomical innovation of Plato's old age, the theory of uniform circular paths for the 'planets' (mentioned at *Laws* 822a), is surely a Greek and not a Babylonian achievement. What is suggested by the tradition concerning Eudoxus' travels is that the specifically Greek interest in a simple cosmic model led him to seek more detailed information on the relative lengths of the cycles. But that these planetary phenomena were characterised by cyclical recurrence had been known—in the case of Venus—since early Babylonian times, and in Greece since Parmenides. Some of the details were presumably recorded in Democritus' book on the planets.

[47] Dicks 33 f., following Heath, *Aristarchus* 294.

from a mountain or dominant peak, and at least one of them (Phaeinos) associated with solstice observations (τὰ περὶ τὰς τροπὰς συνεῖδε).[48] When did such attempts to improve the calendar begin in Greece? The doxographical tradition gives us a definite answer: solstices and equinoxes were observed in Miletus as part of a general attempt to measure the seasons and hours (and thus to establish a reliable calendar), and the gnomon was used for this purpose.[49] The evidence is late and meagre, but there it is. It represents Anaximander and his associates as practical astronomers, the precursors of Meton and Euctemon. Dicks would have us reject this evidence altogether. Why? In part, no doubt, because he does not see that the alliance between careful observation and bold speculation is not only natural but essential in early Greek thought, the very condition for the creation of science and philosophy in the Greek sense. But he argues, more specifically, that the tradition in this case must be worthless because it mentions the study of solstices and equinoxes together, whereas the two phenomena are on an entirely different scientific level. While the early knowledge of solstices is practical and superficial (as in Hesiod), so that the attribution of such knowledge to Anaximander is trivial, the early knowledge of equinoxes was non-existent, so that its attribution to him must be false. The chief purpose of Dicks' article is 'to show that, whereas knowledge of the solstices does not presuppose anything other than (relatively) simple observations, the concept of equinoxes is a much more sophisticated one, involving necessarily the complete picture of the spherical earth and the celestial sphere with equator and tropics and the ecliptic as a great circle traversed by the sun' (p. 30).

I submit that Dicks has not shown this, and cannot do so, for his claim is clearly false. Or rather, part of it is true just in case one understands 'the concept of equinoxes' in the precise, theoretical sense as the concept of the points of intersection between the ecliptic and the celestial equator—and in that case, one must understand 'knowledge of the solstices' in an equally theoretical way, as knowledge of the points where the ecliptic touches the tropics. Dicks' thesis about equinoxes is defensible only in so far as it is question-begging, i.e. only if he means by 'equinox' the concept defined in the fully developed theory of the celestial sphere. But then he should mean the same kind of thing by 'solstice'. And in either case it is incorrect to say that the concept of equinoxes (at a given place) involves the concept of a spherical earth. The fact that the Babylonians observed or computed the equinoxes, but never discovered the sphericity of the earth, should have prevented Dicks from making such an extravagant assertion.

Dicks begins with a true premiss, namely that solstices are observable in a crude way in which equinoxes are not. If we carefully note where the sun rises and/or sets each day, we may spot, in the course of a year, the northernmost and southernmost rising and setting

[48] DK 6A1.

[49] DK 12A1, A2, A4 (Diogenes Laertius, Suda, Eusebius, all referring to Anaximander). Exactly the same list of achievements is assigned by Pliny to Anaximenes, apparently as a result of a copying error (DK 13A14a). A systematic interest in solstices (together with eclipses) is ascribed to Thales by Diogenes Laertius, on the authority of Eudemus (Eudemus *fr.* 145 Wehrli = DK 11A1 = D.L. i 23). In this context, Diogenes mentions as witnesses to Thales' competence in astronomy not only Herodotus but also Xenophanes, Heraclitus and Democritus. This suggests (1) that Eudemus is the source for the consistent core of the late doxography concerning practical astronomy in Miletus (whereas Favorinus is quoted only for an implausible variant, the anecdote about the sundial at Sparta), and (2) that Eudemus in turn appealed to the four authors cited as his own source of information.

Dicks' attempt (33, n. 35) to discredit this doxographical tradition by deriving it all from Diogenes' text, as if Eusebius had Diogenes in front of him as he copied out the entry on Anaximander, will not recommend itself to anyone familiar with the usual standards of doxographical scrutiny. In fact Diogenes, although the earliest, is the least restrained of our three Greek sources here: he alone cites the improbable anecdote from Favorinus. All we can infer is that Diogenes, Suda and Eusebius have a common source, who is earlier than Favorinus and Pliny (DK 13A14a).

points, and call these four points the τροπαί, the 'turnings'.[50] We cannot, in the same direct way, note the points on the horizon where the sun rises and sets when day and night are of equal length (unless we have a good clock, of course; and the early astronomers did not). These points must be located indirectly, by more subtle observation or by some sort of calculation. But it does not follow that the only calculations we can make are those of Meton or Hipparchus. In claiming that 'without the fundamental concepts of equator, tropics, and ecliptic on the celestial sphere, the equinoxes are meaningless' (p. 33 n.), Dicks is involved in a strange anachronism, a violation of the very rules of historical method he proclaims. For his conclusion, which draws upon the procedure of Ptolemy in the latest period of ancient astronomy, depends for its cogency upon the fantastic assumption that no simpler method existed in the earlier period for forming the concept of equinoxes and setting out to determine their dates.

It is easy to see that this assumption is false. In the first place, τροπαί means not only the points on the horizon where the sun turns but also the two seasons of the year when this occurs. This is the sense of τροπαί in Hesiod (*Op.* 479, 564, 663). Similarly, the term for equinoxes, ἰσημερία, refers primarily to the season at which day and night are of equal length; and it is in just this seasonal sense that the word is first attested, in the fifth-century Hippocratic treatise *On Airs, Waters, Places*.[51] The concept is certainly earlier than the word, for anyone who reflects on the fact that days get longer after the winter solstice and shorter after the summer solstice will realise that there is in each case a moment when day and night are about equal in length. There is probably a reference to this fact in a Hesiodic phrase ἰσοῦσθαι νύκτας τε καὶ ἤματα, in verses which are generally bracketed by the editors.[52] Whatever the date of these verses, they illustrate the concept of equality of day and night in a thoroughly untechnical context.

Suppose now that the Milesians wanted to specify the date of the equinoctial season more closely. The simplest method would be to count the number of days from solstice to solstice and divide by two. This involves, I trust, no theory which Dicks is unwilling to grant to our primitive astronomers. Furthermore, the assumption that the four solar seasons are equal, although false, is a natural one which seems to have been used by the Babylonians at one time and still accepted by Eudoxus, despite the more precise (but only partially correct) estimates of Meton and Euctemon.[53] Thus in order to fix the date of the equinoxes no theory is required except for the assumption that the sun's annual motion—understood simply as the variation in time of sunrise and sunset—is uniform from season to season.

If the Milesians were carrying out rudimentary observations, as the tradition asserts, they would naturally have attempted to check this dating of the equinoxes in various ways.

[50] This seems to be the sense of τροπαὶ ἠελίοιο in Homer, *Od.* xv 404, though we are not told which solstitial rising (or setting) point is intended. Compare ἀντολαὶ Ἠελίοιο in *Od.* xii 4 ('mean rising point' = due east); in both cases a place or direction is named for the phenomena which occur there.

[51] Περὶ ἀέρων ὑδάτων τόπων ch. 11 (the most dangerous changes of the seasons are): ἡλίου τροπαὶ ἀμφότεραι καὶ μᾶλλον αἱ θεριναὶ καὶ αἱ ἰσημερίαι νομιζόμεναι εἶναι ἀμφότεραι, μᾶλλον δὲ αἱ μετοπωριναί, 'both solstices, and especially the summer one, and the equinoxes, both of which are generally believed to be dangerous (νομιζόμεναι εἶναι sc. ἐπικινδυνόταται) but the fall equinox is especially so'. Dicks' comment on these words, 'the equinoxes, which as a less familiar concept require an explanatory description' (33, n. 38), is unintelligible to me. As the term νομιζόμεναι shows, the author can assume that most people, or most doctors, are perfectly familiar with the concept of equinoxes.

[52] *Op.* 562. Wilamowitz comments (*Hesiodos Erga* 106): 'Das vollendete Jahr wird durch die Tag—und Nachtgleiche bezeichnet. . . . Der Verfasser hat an ein Jahr gedacht, das mit Frühling anfing, wie z.B. in Keos . . . und an die Isemerie'. Friedrich Solmsen suggests to me (in a letter) that the 'interpolation' is not likely to be later than the sixth century; and he agrees that the reference must be to the equinoxes.

[53] *Cf.* Dicks 34 f. For the Babylonian practice in the pre-Hellenistic period, see Neugebauer, *The Exact Sciences in Antiquity* 102: 'it is the summer solstices which are systematically computed, whereas the equinoxes and the winter solstices are simply placed at equal intervals'.

For example, they might have plotted the points midway between solstitial risings (or settings) by bisecting the angle, and then simply looked to see if the sun did in fact rise (or set) at that point on the day expected. On the other hand, with the use of gnomon and shadow, more precise measurements could easily be attempted. They need only have set up a vertical gnomon at a place with a clear view of eastern and western horizons, and noted the line of the gnomon's shadow each day at sunrise and at sunset. This gnomon could be used for solstitial observations, of course, by watching for the extreme northern or southern excursion of the shadow. But to use it for equinoctial observations, one does not even have to compare the line of the shadow from one day to the next: the equinoctial phenomenon can be recognised *on the day when it occurs*, and consists simply in the fact that *the morning shadow and the evening shadow form a straight angle*, i.e. are diametrically opposed.[54]

This is indeed a method for determining equinoxes by simple observation alone, with no theory and no computation. A theory is required only to explain *why* it is that day and night are equal when the sun rises exactly in the east. And the theory which does the trick is of course that of the celestial sphere.

It would be idle to speculate further as to just which observations the Milesians actually carried out and what theories they used to explain the results. What has been shown is that no theory is required for observations which determine equinoxes with the same rough accuracy as naked-eye observations of solstices. And hence there is no scientific reason to doubt the tradition that some attempts to fix the solar seasons were made in the sixth century. Furthermore, the great precision of the results attained by Meton and Euctemon in 432 B.C. suggest that they are not likely to reflect the first Greek attempt to measure the astronomical year, and may rest on earlier Greek observations made over a long period.[55] Meton's equipment was known as a *πόλος*; the attention it attracted is well attested by several quotations from Aristophanes.[56] Now the *πόλος* is mentioned by Herodotus among

[54] This procedure with the gnomon was suggested to me by Howard Stein. Note that by either procedure one has an astronomical determination of due east and due west. It is worth recalling that Anaximander was a cartographer, and that Greek maps were normally oriented by reference to six cardinal points: 'equinoctial sunrise' (=east), 'equinoctial sunset' (=west), plus summer and winter sunset and sunrise at the four solstitial points. See Aristotle, *Meteorology* ii 6, and W. A. Heidel, *The Frame of the Ancient Greek Maps*.

Dr W. D. Heintz points out that the gnomon procedure described in the text is exposed to systematic error if the horizon is not perfectly level in east and west, and that more precise measurement is possible on the basis of the sun's altitude at noon (as computed from the shadow length): one dates the equinox by the noon altitude half-way between two solstitial measurements. Here again, no spherical model is required for the measurements, but only for their theoretical justification.

After writing this, I notice that essentially the same two procedures are conjecturally assigned to the Babylonians by Kugler, *Sternkunde und Sterndienst in Babel* i 175 f. Tables correlating the length of shadow with the hour of the day at different dates in the year are partially preserved in the second 'Mul apin' tablet; see Weidner, *Am. Journal Sem. Lang.* xl (1924) 198–201. Both Weidner and van der Waerden regard these tables as confirming Herodotus' report of the Babylonian origin of the gnomon. See van der Waerden (1966) 63, 80.

In his 1966 book, p. 134, van der Waerden suggests a variant of the straight line test which does not depend upon sunrise and sunset observations but makes use of the fact that only at the equinox does the tip of the gnomon shadow describe a straight line on a flat sundial, as the shadow moves throughout the day.

[55] Professor Stein and Dr Heintz, working on different assumptions as to the procedure used, agree that the accuracy of measurement reflected in the Metonic cycle requires a comparison of observations recorded over at least a century. If Meton and Euctemon discovered the cycle themselves, they must have had access to solstice and/or equinox records going back to the time of Anaximenes, if not earlier. Of course these records, or more likely the cycle itself, may have been introduced directly from Babylon in the fifth century. Hence Meton's achievement cannot guarantee the antiquity of systematic observations in Greek astronomy.

[56] *'Αριστοφάνης ἐν τοῖς Δαιταλεῦσιν* (cited by Achilles, *Isagoge* xxviii, ed. Maass p. 62; not included in the numbered fragments):

πόλος τοῦτ' ἐστίν, ᾗ 'ν Κολωνῷ σκοποῦσι τὰ μετέωρα ταυτὶ καὶ τὰ πλάγια ταυτί.

fr. 163 (*Gerytades*):

πόλος τόδ' ἐστίν; εἶτα πόστην ἥλιος τέτραπται;

the Greek borrowings from Babylon, together with the gnomon and the division of the day into twelve parts. The duodecimal scheme is authentically Mesopotamian, and the use of a gnomon is indirectly attested by the Babylonian tables for shadow-lengths. But what about the πόλος? If this means, as Rehm argues and as the name suggests, a concave bowl or hemisphere on which the sun's shadow was traced (with reference to circles marking the lines of equinox and solstice), i.e. the instrument later called the σκάφη,[57] then it is perhaps more likely to be a Greek improvement on an earlier flat sundial than a slavish copy of its Babylonian prototype. (A continued use of the gnomon with a flat base would in any case be required for shadow measurements.) The name πόλος seems to refer to the starry dome of heaven turning round the pole, and the shape of the instrument itself is difficult to explain except on the basis of a spherical model for celestial motion. In fact the geometric pattern of the πόλος, with its system of concentric circles, is strikingly reminiscent of the world models of Anaximander and Parmenides. Perhaps the true antecedent of Meton's πόλος is the σφαῖρα which Anaximander is said to have constructed. At all events the instrument cannot have been a new invention in 432 B.C., since Herodotus ascribes it to Babylon. If the hemispherical plan was already traditional by that time, this would provide one more piece of indirect evidence for the relatively early date proposed here for the spherical model in Greek astronomy.

One final point about historical method. Neugebauer has shown that no scientific techniques existed in Babylon which would have permitted Thales to predict the solar eclipse of 586 B.C. Does it follow that Herodotus and the later tradition are mistaken, and that no such prediction was made? Dicks describes Herodotus' story as a 'hoary fable', and implies that it is sufficient proof of a lack of historical sense for anyone to refer to this alleged prediction in any less derogatory terms.[58] But what follows from Neugebauer's research is only that, *if* Thales predicted the eclipse, he did not do so on a scientific basis. *If* he predicted a solar eclipse, he made an extremely lucky guess, perhaps on the basis of partial information. It would be absurd to assert the occurrence of Thales' prediction as an historical fact. But I do not see that it is the least bit more historical to deny it flatly. Herodotus is the closest thing we have to a contemporary testimony; hence it is the historian's task to report Herodotus' story with the appropriate qualifications. Where we have some evidence in favour of an unlikely (but not impossible) event, and no direct evidence against it, the reasonable man will suspend judgment. It is not sound method but sheer prejudice to suppose that the cause of historical truth is served by a dogmatic rejection of the ancient tradition as we find it preserved in Herodotus, Eudemus, and the later doxographers. This tradition is not always reliable, and can never be accepted without critical scrutiny. But, except for an occasional fragment from a Presocratic poem or treatise, this tradition is our only source for the development of Greek science from 600 to 432 B.C. To regard the tradition as unreliable *in principle* is to close the door to any understanding of the early history of Greek astronomy.

The story of Thales' eclipse deserves to keep its place in the traditional narrative, not as a well-established report of an historical event, but as part of the popular memory of the true *pre-history* of Greek astronomy, the first period of Milesian ἱστορίη when great things were stirring of which we perceive only a distorted echo. Whatever the facts about Thales may have been, we can see that by the end of the Milesian period the characteristic Greek form of astronomy, operating with kinematic models and (initially crude) geometric concepts, had come into existence. There is every reason to suppose that the tradition is essentially correct, and that these first Greek cosmologists were also engaged in crude observational

[57] See A. Rehm, s.v. 'Horologium', PW viii 2417: 'eine hohle Halbkugel, das Gegenbild des Himmelsgewölbes'.

[58] Dicks 37.

astronomy where practical and theoretical interests were fused, as in the contemporary Milesian enterprise of cartography.

To isolate the history of science in Greece from the history of philosophy, as Dicks would have us do, is not simply an act of historical injustice to Anaximander or Anaxagoras. It is false to the very nature of Greek science and philosophy as these stand in fundamental contrast with, for example, the science that developed in Mesopotamia or the philosophy that developed in India. What distinguishes Greek geometry from its Babylonian antecedents is the notion of mathematical proof. Similarly, what distinguishes Greek astronomy is the systematic use of a geometric model to represent celestial motions. In each case, it is because Greek science was initially developed by men of a philosophic or strongly theoretical turn of mind that such a momentous innovation was possible. Conversely, what characterises Greek philosophy in its classical development, from the Milesians to Aristotle, is the close connection with actual scientific research in astronomy, geometry, and biology. There appears to be nothing comparable in the major philosophical developments of India, where linguistics seems to play the role which mathematics and natural science play for the Greeks.[59] But this conjunction of philosophic and scientific work recurs regularly in the Western tradition, for example in medieval Islam (where Avicenna and Al-Rāzi were doctors and authors of works on medicine) and, most conspicuously, in the new and revolutionary interaction of natural science and philosophy in seventeenth-century Europe. What is at issue here in the discussion of early Greek astronomy is not merely a critique of the doxography for two or three Presocratic thinkers but an understanding of the fundamental character of the relationship between science and philosophy in the tradition which we have inherited from the Greeks, a tradition which had its second birth with Galileo and Descartes, and which happens to have begun in Miletus.

CHARLES H. KAHN.

University of Pennsylvania.

[59] It may be argued that in India Pāṇini's grammar occupies the position of geometry in Greece, as the paradigm of scientific knowledge. See I. F. Staal, 'Euclid and Pāṇini', *Philosophy East and West* xv (1965) 99–116.