time that the terms of the firman were being exceeded and that the Voivode was being pressed beyond his better judgment, but he must have kept his scruples to himself. There is no suggestion in Gell's account that any of the four Englishmen present felt the slightest doubt as to the propriety of what was happening; they were saving precious sculptures from destruction and making them available for study in London, to the lasting benefit of British scholarship and taste. The passage in Dodwell's *Tour through Greece* (1819), quoted by St Clair at p. 102, combines inaccurate recollection of facts (he did not see 'several metopae taken down in 1801', or 'the south-east angle of the pediment thrown down', though he did see, in 1805, that these things had happened subsequent to his first visit) and hindsight sentiment. There is no sign that in 1801 he felt 'inexpressible mortification' at the damage, though he may well have felt this four years later.

During their stay, Gell and his friends paid a visit to Eleusis, and saw the colossal bust of Ceres, of which Gell wrote:

'The inhabitants have yet retained some notion of the gifts of this Goddess, for they affirm that the fertility of the land will cease if the statue be taken away. As it seems to be in no danger of further mutilation, I hope the poor people will never be undeceived.'[5]

Less than three months later, Dr E. D. Clarke came with levers and oxen and shipped it to the Fitzwilliam Museum at Cambridge.

Gell and Dodwell left Athens on September 15th and cruised for three months in the Aegean; they explored the Plain of Troy and reached Constantinople about December 10th, when Lord Elgin invited them to stay at the Embassy. In the following four weeks Gell wrote up, from his daily journal, a connected account of his travels since he had left England; his story must have been completed before January 12, 1802, when Dr Clarke arrived at the embassy, for Gell could not have written what he did of the Eleusinian Ceres had he known that it was no longer in place. On his death in Naples, in 1836, he left all his papers to his friend Keppel Craven, who gave thirteen sketchbooks, including the one in use in 1801, to the British Museum, but the great bulk of Gell's papers passed on Craven's death in 1851 to his Italian secretary. One collection, of eleven notebooks, was found in Naples by Dr Ashby of the British School at Rome, and is now divided between the Schools at Athens and at Rome; a second collection reached the London market in 1926. This included Gell's 'Diaries' for the winter of 1800–01 and for 1801; they are now in the Library of Bristol University and extracts are here printed by the permission of the Librarian. Other pieces from the same collection are in the Bodleian and the Gennadios Library at Athens.

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[5] University Library, Bristol, D.M.7, p. 84.

G

More Astronomical Misconceptions

It seems clear from an article by C. H. Kahn in *JHS* xc (1970) 99–116 that there are still prevalent numerous misconceptions concerning early Greek astronomy. To attempt to correct all these in detail would require a lengthy exposition of elementary points which would be extremely tedious for the discerning reader. There are, however, two matters a lack of understanding of which leads to such erroneous conclusions that one may perhaps be forgiven for a final attempt to clarify the issues.

The first concerns the passage in the Hippocratic treatise *On Airs, Waters, Places* (ch. 11) which warns against giving drastic medical treatment at certain times of the year; the passage (in which ἰσημερίαι is Heringa's certain emendation of the MSS ἰσημεριναὶ) lists these times as follows: μέγισται δέ εἰσιν αἵδε καὶ ἐπικινδυνόταται· ἡλίου τροπαὶ ἀμφότεραι καὶ μᾶλλον αἱ θεριναί, καὶ αἱ ἰσημερίαι νομιζόμεναι εἶναι ἀμφότεραι, μᾶλλον δὲ αἱ μετοπωριναί. Kahn (113 n. 51) finds 'unintelligible' my comment (*JHS* lxxxvi [1966] 33 n. 38) that the equinoxes, as a less familiar concept than the solstices, require the additional qualification νομιζόμεναι εἶναι (literally, 'as they are considered to be'—I now think, *pace* my original note, that the German 'sogennanten' or the Loeb 'so reckoned' are both adequate translations), and himself translates 'both of which are generally believed to be dangerous (νομιζόμεναι εἶναι sc. ἐπικινδυνόταται)'. This is an extraordinarily strained interpretation, which not only entails the mistranslation of a superlative but also runs counter to the style of the whole work. There is a certain fullness of expression in the Greek of the Περὶ ἀέρων which makes ellipsis comparatively infrequent and the insertion of the copula normal, when it might commonly be omitted in other Greek; see, for example, ch. 4.24–29 (threefold repetition of γίνεσθαι and ἰσχυρός—reference by chapter and line of the Loeb edition), ch. 8.23 ff., and the invariable expression of ἐστί or ἔχει with words like εἰκός and ἀνάγκη. In keeping with this tendency, it will be found that a predicate adjective with νομίζειν or νομίζεσθαι is in this treatise always expressed and not merely understood (cf. 3.28; 4.15; 7.48; 7.97; 23.13; 23.24); so εἶναι must here be taken on its own in the sense of 'to be', 'to exist', 'to occur', 'to take place', without a predicate. Thus Kahn's designation of my comment as 'unintelligible' merely reveals his own lack of familiarity with the Greek. The language of the Περὶ ἀέρων also affords another indication that the concept of equinoxes was not as yet well known, in that the situations of cities and the directions of winds are always described with reference to the sun's *solstitial* risings and settings and never the equinoctial ones. In fact, we find periphrases used (such as τὰ μεταξὺ τῶν δυσμέων τῶν θερινῶν καὶ τῶν χειμερινῶν, 7.75, and τοῦ ἡλίου ἐν μέσῳ τῶν ἀνατολέων, 12.15; cf. 5.4) instead of ἰσημερινὴ δυσμή or ἀνατολή, 'equinoctial setting' or 'rising', to indicate due west and due east (first attested in Aristotle, *Meteor.* 363a–b).

The second matter relates to my contention that the determination of equinoxes belongs to a more sophisticated stage of astronomy than the simple observation of solstices, since it necessarily involves knowledge of the whole system of the celestial sphere. Kahn says that my 'claim is clearly false', that it is 'an extravagant assertion' (112), and that it depends upon 'the fantastic assumption' (113) that there is no simple method for determining equinoxes; but his attempts to controvert such a self-evident proposition are plainly indicative of scientific incomprehension. The sophistries about definitions in the second paragraph of 112 can hardly be taken seriously; the statement that 'the Babylonians observed or computed the equinoxes' (*ibid.*) is simply wrong. The equinoxes played no part in Babylonian astronomy (how should they, since the Babylonians never developed the concept of the celestial sphere and equator?), and were neither computed nor observed, but *placed* arbitrarily on the 15th day of months IV and X in the schematic year of 12 months of 30 days each, the four quarters of this year being assumed to be exactly equal (*cf.* my *Early Greek Astronomy to Aristotle* [1970] 166 and nn. 285 and 286). I do not regard such a procedure as constituting 'determination' of the equinoxes in the sense I am using. Kahn is apparently aware of these facts (see 103 n. 17 and 113 n. 53), but he does not pause to consider their implications. Similarly, he *seems* to realise that the sun (as seen from the earth in a geocentric universe) appears to move faster in some parts of its annual motion round the ecliptic than in others (thus producing the unequal lengths of the seasons), and yet he suggests that counting the days from one solstice to the next and dividing by two would give the date of the equinox (113). In round numbers, the sun takes 92 days to go from the summer solstice to the autumnal equinox, and 89 days from there to the winter solstice; the total divided by two is therefore 90½ days. From the winter solstice to the vernal equinox the sun takes 90 days, and from there to the summer solstice 94 days; the total divided by two is 92 days. Thus by this method, even if we suppose that the solstices are determined to the nearest day (but see below), the autumnal equinox would be put 1½ days early and the vernal equinox two days late—and the errors would, of course, be cumulative. In any event, without knowledge of the great circles of the ecliptic, celestial equator, and solstitial colure, and their relationships to the horizon, what is to show that the time between the solstices is to be divided by two? Once again, Kahn allows preconceptions derived from later knowledge to colour his judgement (*cf.* my criticisms in *JHS* lxxxvi [1966] 35–7).

His next suggestion is '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' (114). I take it he means by this very imprecise formulation that the arc of the horizon comprised between the sun's rising (or setting) at the winter solstice and its rising (or setting) at the summer solstice might be bisected to give the point of the equinoctial rising (or setting). Apart from the fact that this is open to the same objection as above (namely, how does one know, without the picture of the celestial sphere, that the arc should be bisected?), has Kahn given any thought to the practical difficulties attached to such observations (weather conditions, atmospheric refraction, brightness of the sun, etc.) and above all to the uncertainties inherent in solstitial observations? What he apparently fails to realise is that the sun's annual motion on the ecliptic (in the opposite direction to its daily motion) is *continuous*, that the angle between the ecliptic and the horizon is *continually* changing, and that the sun's declination is *continually* altering. At the solstices, this annual motion appears to be at its slowest; over an arc of 10° on the ecliptic each side of the actual solstitial point the declination changes by less than ½°. This means that for a span of more than 20 days (since 360° on the ecliptic = 365¼ days approximately) the point on the horizon where the sun rises or sets changes by less than 1° (actually about 46'), with proportionately tiny changes in shadow lengths and hours of daylight (a glance at a diary will show that the length of daylight varies only by about 6 minutes from December 11th to January 2nd or from June 12th to July 3rd). Does Kahn really think that differences of this order would be detectable by his putative observers in the sixth century B.C.? If we supposed that double these differences might be noticed (itself highly improbable), it would mean that during a period of over a month (16 days each side of the solstitial point) *any* day might be chosen as the day of the solstice—and this would be the case at each solstice. Consider what would be the result for anyone trying to count the days between the supposed solstices in order to find the equinox!

At the equinoxes the sun's declination changes more rapidly, at a rate of about 24' per 1° on the ecliptic over an arc of 8° spanning the equinox (i.e. in a period of just over 8 days); and it is still changing *continuously*. In 12 hours, then (say, from 6 a.m. to 6 p.m.), the sun will have moved nearly ½° on the ecliptic and its declination will have changed by 12'. So Kahn's next suggestion (on which, to judge from the profusion of italics, he places especial reliance), that the moment of the equinox can be judged by observing when the morning and evening shadows 'form a straight angle' (114, an appropriately unscientific term for an unscientific procedure— presumably he means 'a straight line'), is proved to be a theoretical impossibility for a start. But, it may be objected, such a small difference as 12' would never have been noticed. Very well—let us assume (optimistically) the same order of accuracy as for the solstices, namely, that a change of about 1½° might be detected, say 8 times 12'; then the equinox might be put at *any* time within 4 days each side of the actual equinoctial point. And this is without taking

into account the insuperable practical difficulties of making any meaningful measurements of the enormously elongated and blurred shadows that would be obtained near sunrise and sunset—difficulties that Kahn and his advisers completely ignore.

No, my thesis remains unshaken; an equinox, unlike a solstice, cannot be determined by simple observation 'on the day when it occurs' (Kahn 114—his italics), until the theory of the celestial sphere and the relationships between its great circles and the horizon are known (cf. *JHS* lxxxvi [1966] 32 and especially n. 34). This is what all the *factual* evidence suggests (*loc. cit.* 33–5), and what anyone acquainted with the real problems would expect. For the rest, in those parts of his article in which he is not concerned with misrepresenting my views (for a true picture of which, see *Early Greek Astronomy to Aristotle*), Kahn devotes his efforts to rehashing the familiar 'evidence' for an advanced state of astronomical knowledge in sixth century B.C. Greece. He finds this 'evidence' convincing—I do not. I am content to have drawn attention to the very cogent reasons for rejecting it.

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The Neatherd's Progress in 'Theocritus'

'Theocritus'' *Idyll* xxvii describes how a neatherd progressively undresses a girl and then makes love to her. The progress of the neatherd's hand is accurately and humorously depicted. First the maiden says (19):

μὴ 'πιβάλῃς τὴν χεῖρα· καὶ εἰσέτι; χεῖλος ἀμύξω.

The neatherd performs then a strategic withdrawal, but returns to the victorious attack in lines 49 ff. The girl exclaims (49)

τί ῥέζεις, σατυρίσκε; τί δ' ἔνδοθεν ἅψαο μαζῶν;

at which the neatherd is undeterred, so that she has to repeat her request (51)

. . . τεὴν πάλιν ἔξελε χεῖρα.

The neatherd's hand proceeds to the garment of paramount importance, i.e. the μίτρα (55), and finally tears away the ἀμπέχονον (59):

τἀμπέχονον ποιήσας ἐμὸν ῥάγος·[1] εἰμὶ δὲ γυμνά.

I shall now put right line 55. The ms. reads

φεῦ φεῦ, καὶ τὰν μικρὰν ἀπέστιχες· ἐς τί δ' ἔλυσας;

Everybody agrees that μίτραν must be restored: this

is shown by the verb ἔλυσας (cf. Ap. Rh. i 288, Mosch. ii 164, Call. *Hymn.* i 21, Nonn. D. i 347, etc.), by the statement that the μίτρα, being the crucial article, will be dedicated πράτιστον by the grateful neatherd to Aphrodite (56), and by the fact that the context is concerned with the girl's clothes[2] (εἵματα 53,[3] πέπλους 54, ἀμπέχονον[4] 59). On the other hand, Scaliger's ἀπέσχισας is not convincing: it is palaeographically violent and, moreover, semantically inappropriate, because ἀποσχίζω (literally 'cut away from') is never used of loosening a μίτρη.[5] The best discussion of the problem is in Meineke, *Theocr.*[3] 392 (Gow, Cholmeley and Fritzsche-Hiller are unfortunately silent on the question): Reiske desperately tried to support ἀπέστιχες, but his defence failed in that the verb is intransitive and cannot in any case be made to mean 'remove'; Wordsworth, correctly seeing that Scaliger's ἀπέσχισας would be semantically incongruous as well as otiosely repetitive in company with ἔλυσας, proposed καὶ τὰν μίτραν ἀπὸ στήθεσφιν ἔλυσας, which is of course too violent.

Considering that the neatherd's hand, in line 55, has *proceeded to*, has *reached* the all-important piece of clothing,[6] as emphasised by the girl's desperate φεῦ φεῦ, and remembering that confusion between

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[2] On μίτρα cf. Bühler, *Eur.* p. 117 ff. (add Schrader-Schaefer, *Mus.* p. 244 ff.) and p. 200.

[3] Cf. *h. Ven.* 164 λῦσε δέ οἱ ζώνην ἰδὲ εἵματα, Nonn. *D.* xii 387 f. μίτρης . . . εἵματα.

[4] The opposition between ἀμπέχονον in line 59 and ἀμπεχόνη in line 60 is the obverse of 'pointless,' as Gow (*ad* xxvii 59) strangely says. Since the ἀμπέχονον appears to have been worn by 'vilioribus personis' (Thes., s.v. ἀμπέχονον, 160 B), it follows that it must have been a cheaper version of the ἀμπεχόνη. If this is so, the neatherd's words are very much pointed: he shrewdly (cf. xxvii 61) promises the girl ἀμπεχόνην μείζονα (60), i.e. a garment bigger (μείζονα) and better (as opposed to the cheaper ἀμπέχονον which he has just torn). My explanation shows that the notion 'better,' which Cobet, Naber and Platt wanted to introduce into the text (cf. Gow *ad loc.*), is in fact already contained in the text. The 'solecism' τὸ ἀμπέχονον ἐμόν is not only paralleled by xxvii 38 and 72 (as Gow notes *ad* xxvii 59), but also by other examples in Theocritus (listed by Ameis, *De art. usu apud poet. graec. bucol.* [Prgr. Mühlhausen 1846] 41).

[5] The notion 'cut away from' is inappropriate to the context, because a μίτρα is untied, loosened, not cut; the verb ἀποσχίζω, when not expressing the notion 'cut away from', entails the idea of '*tearing off*' (material in Thes., s.v.), which is equally inappropriate to the context: the maiden herself specifies that the neatherd has loosened (ἔλυσας), not 'torn off', her μίτρα.

[6] On this motif cf. Call. *fr.* 75.45 ἥψαο μίτρης. In the Idyll, the hand's progress starts with ἅψαο μαζῶν (49). Cf. Nonn. *D.* xii 387 ἥψατο μίτρης and xvi 268 ff. δεσμὸν λύσατο μίτρης . . . παλάμῃ.

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[1] I think it would be wrong to alter the ms. reading: 'Theocritus' evidently wanted to reproduce the spelling ῥάγος which is in fact attested in papyri (cf. e.g. *LSJ*, s.v. ῥάγος).