

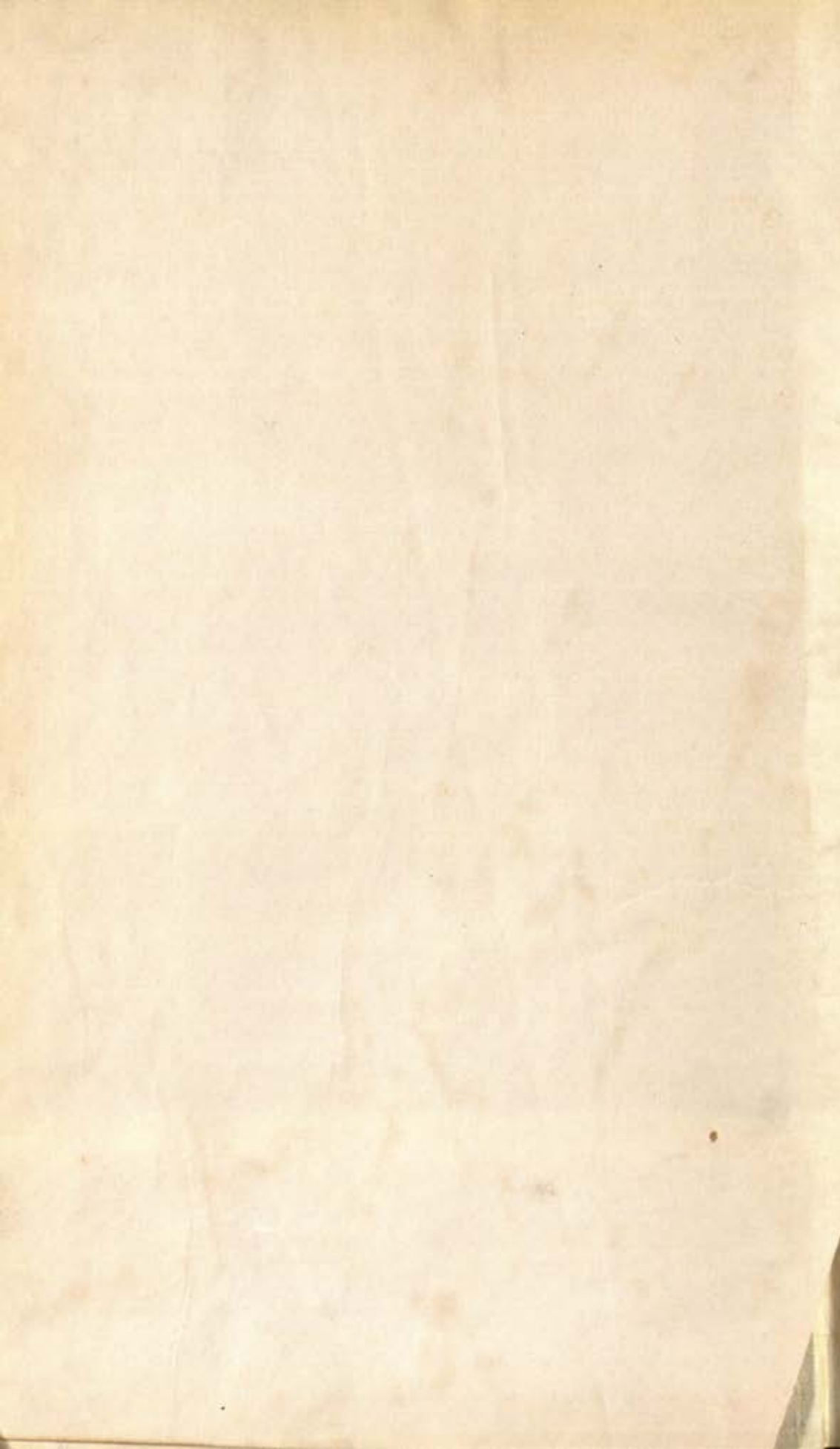
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XENOPHON AND THE WALL OF MEDIA

(PLATES I-III)

How many miles to Babylon?
Three-score miles and ten.
Can I get there by candle-light?
Yes, and back again.

Nursery Rhyme.

IN February of 401 B.C., Xenophon, the Athenian, set out in a contingent of ten thousand Greek mercenaries in the army of Cyrus, the Pretender to the throne of Persia. Cyrus, in fact, was leading the army against his brother Artaxerxes, the Great King of Persia, though the real object of the expedition was not revealed officially until the army reached Thapsacus on the Euphrates in July and crossed the river from Syria into Mesopotamia. Xenophon joined the expedition more or less as a diplomatic attaché, not a soldier. But he became an observer and critic, and, since he was a man of talents, he was driven by self-preservation and by disasters which befell them in the end, virtually to take command.¹ His account of these events was not published or written till many years had elapsed: but it is clear that it must have been based on a regularly kept log or diary.

The geographical problems which Xenophon's report of their itinerary raises have occupied scholars and travellers for nearly two hundred years, the earliest attempt to identify the sites mentioned by him being that of d'Anville in 1779.² For this long history of an unsettled problem, the reasons are that, firstly, we still know little from cuneiform sources about the ancient geography of Central Mesopotamia, and as yet no monograph exists which studies Babylonia in this period; secondly, that the record of Xenophon, though invaluable, is bedevilled occasionally by false reports or inadequate or misunderstood data, or possibly by errors in transmission of the text; and in our own time, progress has been held up by insufficient study of the ground, in particular of the ancient courses of the Euphrates and Tigris and the canals that fed them—and finally, by the inadequate use of aerial photography.

Xenophon's record is, however, reasonably reliable when he is speaking from personal observation or experience, and not from a hearsay report. A more general form of inexactitude in his account derives from our uncertainty as to the length of the parasang in which most of his distances are given. The *parasang*, the Persian measure of distance, was, properly speaking, like many Oriental measurements, somewhat elastic. The parasang was obviously modelled on the Babylonian measure of distance called *bēru*, or 'double-hour', which, it is said, was a distance of 3.738 miles in the Neo-Babylonian period.³ As Layard wrote in 1853:⁴

'The Parasang, like its representative the modern Farsang or Farsakh of Persia, was not a measure of distance very accurately determined, but rather indicated a certain amount of time employed in traversing a given space. Travellers are well aware that the Persian Farsakh varies considerably according to the nature of the country, and

[This paper was originally given to the Society of Antiquaries of London on December 8, 1959. It owes much to the advice and encouragement of Professor Albrecht Goetze, of Yale University.]

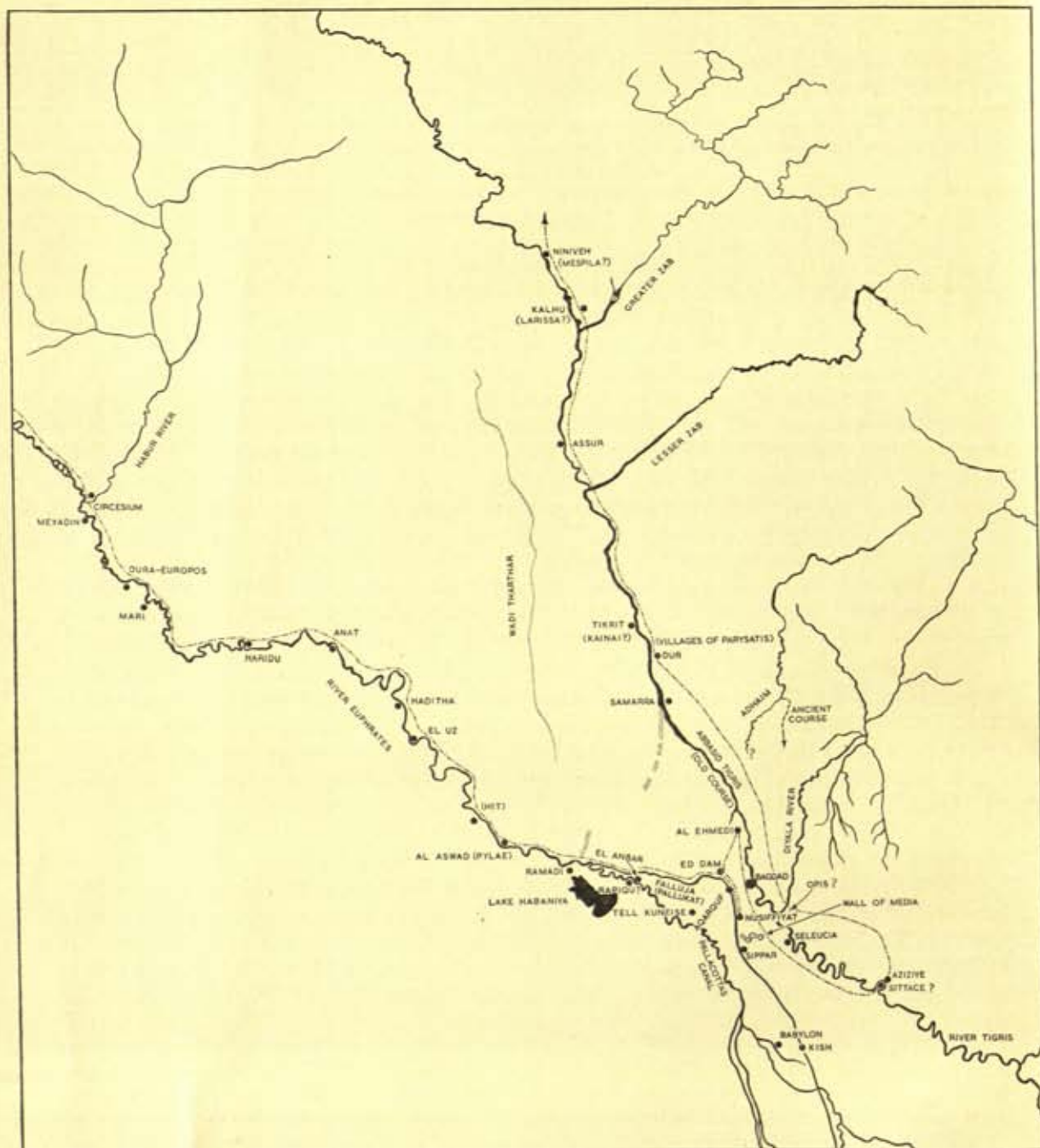
Anabasis, see F. Dürnbach, 'L'Apologie de Xenophon dans l'Anabase', *REG* 1893 343-86.

² *L'Euphrate et le Tigre* (Paris, 1779).

³ E. F. Weidner, *Archiv für Orientforschung* xvi (1952) 19-20; Thureau-Dangin, 'Numération et métrologie sumérienne', *Revue Assyriologique* xviii.

⁴ *Nineveh and Babylon* (1853) 59-60.

¹ For an interesting discussion of the motives for, and circumstances attending, the publication of the



MAP

Showing route of the Ten Thousand and defences between Ramadi (Umm Raus) and Samarra.

the usual modes of conveyance adopted by its inhabitants. In the plains of the Khorassan and Central Persia, where mules and horses are chiefly used by caravans, it is equal to about four miles, whilst in the mountainous regions of Western Persia, where the roads are difficult and precipitous, and Mesopotamia and Arabia, where camels are the common beasts of burden, it scarcely amounts to three. The *farsakh* and the hour are almost invariably used as expressing the same distance. That Xenophon reckoned by the common mode of computation of the country is evident by his employing, almost always, the Persian "Parasang" instead of the Greek Stadium; and that the "Parasang" was the same as the modern hour in the reckoning of the natives.'

So, too, Colonel Leake⁵ pointed out:

'As the ancients had no portable instrument for measuring portions of a day, and could not ascertain the rate per hour, a day's journey was the most exact measure of distance, both by sea and land; though few distances thus reported have reached us, because ancient Geographers and Historians aiming at greater precision have converted the days into stades, and by reporting these without mentioning the number of days, have generally given us instead of a fact, the result of an uncertain calculation.'

It is therefore difficult to know, except approximately, what distance is meant by the elastic term of a parasang, but it was in these regions probably about three miles and three-quarters for a small force or group.

Further, it can be estimated that the army's average day's march was 5.7 parasangs a day between Ephesus and Cunaxa, after that, slightly more. The route down the valley of the Euphrates is fairly well known from itineraries of various dates, and its principal stages can be established, and in several cases, followed from Assyrian⁶ to imperial Roman times, during the whole of which period Aramaic was the local language (see next page).

The Approaches. We take up Xenophon's story at the point where the invading army of Cyrus is crossing at Thapsacus (to be located at Meskene) the Euphrates into Mesopotamia.⁸ From Thapsacus there was, Xenophon says, a march of nine days, representing a distance of 50 parasangs, until they reached the River Araxes, identifiable as the Habur, flowing into the Euphrates 20 miles below the modern Deir-ez-Zor, near the ancient Sirku (Circesium). Instead of Haboras, the proper and ancient name of the Habur, he uses Araxes, which seems to be that of a canal, now called Dawrin, running from the Habur to the Euphrates, where the name is still preserved at the exit of this canal by the site named al Erzi or Arasi.⁹

From the Habur River, the army marched down the left bank of the Euphrates through the desert territory lying between Assyria and Babylonia, for five days' march, a distance of 35 parasangs. The desert abounded in wild life—wild asses, ostriches, bustards and gazelles (some of which they hunted and ate, except the ostriches which they could not catch), until they reached a deserted city of great size called Korsote, at the junction of the Euphrates with the River Maskas,¹⁰ which surrounds the city. Korsote has been

⁵ *Journal of the Royal Geographical Society* ix 9.

⁶ One of the most detailed and interesting of these is the Assyrian itinerary of King Tukulti-Ninurta II (824 B.C.) who struck down the Wadi Tharthar into Babylonia, then making west to the region of the Tigris below Samarra, then via Dürkurigalzu marched south to Sippar, then home via the Euphrates. (Luckenbill, *Annals of Assyria* i § 407-8; A. Musil, *The Middle Euphrates* (New York, 1927) 199-204.)

⁸ i 4.11. The location of Thapsacus (meaning 'a ford', from the Semitic root *ps̄h* 'to pass') is disputed, but seems to have been at Samūma (Meskenē) at the

great bend of the Euphrates. See Pauly-Wiss s.v. Θάψακος. Others place it at Raqqa (Nikephorion). The arguments of W. J. Farrell (*JHS* lxxxi (1961) 153-5) for placing it at Carchemish are unconvincing. The distance of Thapsacus from Babylon is given by Eratosthenes (Strabo ii 1.22, 29) as 4800 stadia or 600 miles.

⁹ Musil, *op. cit.*, 221, followed by Du Mesnil du Buisson, *Baghouz, l'ancienne Corsote* (Leiden, 1948). The Dawrin canal is the Saocoras river of Ptolemy: Musil, 340.

¹⁰ i 5.4.

<i>Assyrian sources</i>	<i>Parasangs (from Thapsacus)</i>	<i>Xenophon</i>	<i>Schoeni</i>	<i>Isidore of Charax (first century B.C.)*</i>	<i>Ptolemy (second century A.D.)</i>	<i>Ammianus Marcellinus (fourth century A.D.)</i>	<i>Modern</i>
				Νικηφόριον	Νικηφόριον		Rakka
			4	Γαλαβάθα, κόμη ἔρημος			Kuleibat Hama
			1	Χουμβανή, κόμη			
Marrata			4	Θιλλάδα Μιφράδα	Θέλλα		Marrat
			—	Βασίλεια, Ἀρτέμιδος ἱερὸν. Σεμυράμιδος διῶρνξ			
			4	Ἀλλάν, κωμόπολις			
			4	Βηοναν, Ἀρτέμιδος ἱερὸν	Βεθαῖνα		
			6	Φάλιγα κόμη	Φάργα		
Sirku			—	Ναβαγάθ, κωμόπολις		Circesium	Deir-ez-Zor
Ḫabur	50	Ἀράξης Πόταμος	—	Ἀβούρας πόταμος	Χαβῶρα	Chaboras	Ḫabur River
Suḫi			4	Ἀσχα κόμη	—	—	—
			6	Δοῦρα Νικάνορος	Δοῦρα	Dura	Tell Salihye
Mari, Ma'eri			5	Μέρραν ὀχύρωμα	—	—	Tell Hariri
Ḫindanu		—	5	Γιδδάν πόλις	—	—	
Ḫarada, Harzē Maškitē	35	Κορσώτη Μασκᾶς πόταμος	7	Βηλεσὶ Βιβλάδα	—	—	Kala't Bulak?
			6	(Island)	—	—	
Anat		—	4	Ἀναθὼ νῆσος		Anatha	Ana
Talbish		—	2	Θιλαβοῦς νῆσος	—	Thilutha	Haditha?
		—	12	Ἰζαν νησόπολις	—	Achajachala	Al-Uz
		Χαρμάνδη		—	—	Paraxmalcha	
Hit, Tuttul			16	Ἀείπολις, ἀσφαλτίτιδες πηγαί	Idicara	Diacira Ozogardana	Hit
	90	Πέλας		—	—	Macepracta	Umm Raus
			12		—	Pirisaboras	Al-anbar
(Totals)	175		102				

* *Parthian Stations* ed. Schoff (Philadelphia, 1914).

identified with a site now called Baghouz, just below Abu Kemal, on the frontier between modern Syria and Iraq, where a French excavator, Du Mesnil du Buisson,¹¹ has dug up material ranging from the prehistoric period to the Iron Age. But it seems to be more likely at ed-Diniyye, the site at a loop of the Euphrates of ancient Harada or Haridu (or Harzē) near Maškitē, where Tukulti Ninurta II spent a night. They are the correct distance from the Habur, and their names are represented in Korsote (for Κορρώτη) and Maskas. The river of that name must be a former canal, now disappeared. After a rest of three days, the army then marched again for thirteen days through the desert, along the Euphrates for 90 parasangs (this figure is hardly excessive; for a large army, the parasangs necessarily became very short) till they reached a place called Pylae, 'the Gates'. The name does not occur elsewhere, but we are allowed the information that the Gates were 23 parasangs (about 86 miles) distant from Kunaxa, followed by the incorrect estimate of the distance thence to Babylon as 360 stadia or 12 parasangs.¹² Felix Jones assumed¹³ that Pylae represents the point where the river narrows at a place called Bekaa, near Ramadi, on the view that the name implies a narrowing of the river like a gate. But it is certain that it marked the 'Gates' or 'entrance' to the frontiers of Babylonia, since Xenophon states the subsequent marches to have taken place 'through Babylonia'. Herzfeld¹⁴ places the Gates at Madain-al-Hit, 10 kms. below Hit, where there is only a narrow path along the Euphrates bank. For Musil they were at Al-aswad,¹⁵ about 22 kms. below Hit. Musil's account of this stretch deserves quotation:

'Xenophon's picture of this part of the Euphrates valley is true to nature. The banks thereabouts are formed by porous rocky bluffs containing much crystallised gypsum and dissected by innumerable short, deep gullies. In some places for a distance of many kilometres the Euphrates washes the foot of steep rocks on the left bank, leaving no room for the road, which has to follow a course far from the river over rocky ground and through gullies. Where some of the gullies run down to the Euphrates, marshy and often impassable bays are formed. In a territory of this character the daily marches could not have been of equal length because the army must have taken care to reach, if not every day, then at least every other day, a fairly large bay where it could obtain water and pasture.'

The stretch between Korsote and Pylae included the towns of Anah and Hit, the latter famous for its bituminous springs; in its neighbourhood, we have to place on the right bank the large and prosperous city called Charmande, to which Xenophon describes the soldiers crossing the river on floats to do their shopping. Charmande has been (not wholly plausibly) interpreted as derived from 'kir', an Aramaic word meaning 'bitumen', 'pitch' and 'mand', a Persian adjectival ending.¹⁶ Musil explained it as *karma-adda* 'vineyard of Adda' and located it at Adde, opposite Al-aswad.

Fifteen parasangs beyond Hit is said to have been on the right bank a fertile region, where the army of Julian similarly provisioned itself in A.D. 363.¹⁷ But the left bank is described by Xenophon as completely bare, the only industry of the inhabitants being to quarry stones (probably, one assumes, of basalt), to be sold in Babylon for grinding corn.¹⁸ But after Pylae they had entered the rich alluvial plain, irrigated by canals. They marched

¹¹ Du Mesnil du Buisson, *op. cit.*

¹² See below, p. 17.

¹³ 'Researches in the Vicinity of the Median Wall', *Selections from the Records of the Bombay Government* xliii (1850) 263.

¹⁴ Herzfeld, *Ausgrabungen von Samarra* vi (1948). This posthumous work, an excellent survey, unfortunately lacks maps, the plates having been destroyed in the War.

¹⁵ Alois Musil in *The Middle Euphrates*, Appendix II 'Xenophon on the Middle Euphrates', 213-14 esp. 223 (New York, 1927).

¹⁶ Obermayer, *Die Landschaft Babylonien* (1929).

¹⁷ Ammianus Marcellinus, *Rerum Gestarum libri qui supersunt* xxiv. For a detailed study, see L. Dilleman, 'Ammien Marcellin et les pays de l'Euphrate et du Tigre', *Syria* xxviii (1961).

¹⁸ i 5.5.

for three days, a distance of 12 parasangs, finding traces of a large cavalry force which had retreated out of reach:¹⁹ and on the fourth day's march Cyrus reviewed his army in the middle of the night, as a battle was felt to be imminent.²⁰ Indeed, as it was now approaching September, it is possible that many of these marches were made at night or early morning, to take advantage of the cool. But another day went past, and the army marched on for 3 parasangs in battle formation.²¹ Halfway through this march they encountered a deep ditch dug in the ground, 5 fathoms across and 3 fathoms deep. The ditch, Xenophon reports, extended inland 12 parasangs 'upwards' (*ἀνω*) over the plain as far as the 'Wall of Media' (or so he was told), but there was a narrow passage about 20 feet wide between the river and the ditch. The Persian king had had the ditch dug (he says) as an obstacle, when he heard that Cyrus was advancing against him. It was by this passage that the army of Cyrus got through, for the trench was undefended. Xenophon remarks that, since the king did not obstruct Cyrus at the trench, he seemed to Cyrus and the others to have given up the fight. But they were wrong, and two days later the armies met at a field of battle²² which Plutarch, writing five hundred years later, calls Kunaxa;²³ and it is extremely tempting to connect this northern frontier with the belt of evidently very ancient fortifications which may be seen on the map between Umm Raus on the Euphrates, and Istabalat on the Tigris.²⁴

The centre is occupied by a long dyke called Jalu or Sadd Nimrud ('Nimrod's Dyke'), with traces of turrets and moat on the west side, which follows a curiously meandering course. Alois Musil describes it as 'a rampart (*čaltu*) four to six metres high, thirty metres wide at the bottom. In some places bulges projected, resembling remnants of towers. On the west side there extended a shallow depression.'²⁵ The northern continuation, however, is formed by a well preserved and highly developed fortification which was first discovered 136 years ago by Dr Ross²⁶ and confirmed by Lieut. Lynch.²⁷ The latter described it as an embankment or wall of lime and pebbles, 'having towers or buttresses on the northern or north-western face and a deep and wide fosse. This we called the Median Wall; and putting our horses to their full speed, we galloped along it for more than an hour, but finding no appearance of a termination, we returned for our morning observations, taking the word of the natives that it reached to the Euphrates.' This is the feature over 20 miles long which, since Lynch's day, has appeared on most classical maps of the area as the Median Wall. But Capt. Jones already in 1867 pointed out that in spite of appearances neither in extent nor construction did it fit the description.²⁸ This wall is called in Arabic by the name of Al-mutabbaq, meaning 'that which is in layers', i.e. built of recognisable straight lines of bricks. Beyond it is only the now empty steppe, once partly irrigated by the Jalu canal, probably in early Arab times. Al-mutabbaq is described in detail by Herzfeld as a wall 10 courses high of burnt brick, each 136 cm. high, forming a skin 1.4 m. thick filled with pebbles, with rounded towers or buttresses 46.3 m. apart, 53.9 from axis to axis (PLATE I).²⁹ The measurements of the bricks and other distances are said to fall roughly into Arabic measurements of an ell of 51.8 cm. of the period of the Caliph Ma'mun, and according to Herzfeld, the construction of Al-mutabbaq was due to the threat of the Bedouin

¹⁹ i 6.1.

²⁰ i 7.1.

²¹ i 7.14 ff.

²² i 8.1 ff.

²³ *Artaxerxes*.

²⁴ GSGS. 3919. 3rd ed. 1949. Quarter inch. 1.38 N.

²⁵ *Op. cit.*, 51, and 142, 148 and 154. See Goetze, *loc. cit.*, 64 n. 94.

²⁶ J. Ross, 'Notes on two Journeys from Baghdad to the Ruins of al Hadhr . . . in 1836 and 1837',

JRGS ix 445, 1; also 'Journey from Bagdad to the Ruins of Opis and the Median Wall in 1834', *JRGS* xi (1841).

²⁷ Lynch, 'Note on a part of the river Tigris between Baghdad and Samarra', *JRGS* xi (1841) 472-3.

²⁸ *Op. cit.*, 263 (see note 13).

²⁹ *Op. cit.*, 8: Lane, *Babylonian Problems*, described in 1921 the wall near the railway as being of unbaked brick forming two casemates 5 feet broad, with regular bastions at 60 yards' interval.

invading the fertile area along the Tigris bank by the river Dujail in the late Abassid period. At its termination may be seen a small rectangular fort with rounded corner bastions.³⁰ It will be noticed, however, that this fort in the first place is not aligned with the wall, and secondly appears to be of a plan similar to others of the Roman period, e.g. Han al Qattar on the Roman frontier Limes, between Bosra and Palmyra.³¹ It seems that investigation is desirable whether this fort is in fact of the same period as the Wall, or whether it is the remnant of an earlier defence along the same line.

The fortification in the central area seems, when it peters out, to be making for the region of Falluja, though there is a considerable gap, as far as can at present be seen. This may well be because a gap like this did not need defending, since in those days an army could not advance far into the desert away from water.

The Wall at Umm Raus. When we get to the Euphrates side, however, we come to a new discovery, to which my attention was very kindly drawn in 1947 by Mr John Saffery, of Messrs Hunting Aero-Surveys. This feature is a wall running inland towards the rising ground of the desert, to meet the central group of defences (FIG. 1). It runs from Umm Raus on the left bank of the river opposite Habbaniyah. Umm Raus itself is a deserted fort which was described as follows by Alois Musil in 1912 as consisting of 'a few low heaps of old brickwork together with the main part of the stronghold itself, which is rectangular in shape, with walls strengthened by semi-circular towers. On its west side the gate was still visible. From afar the fort resembled a Roman camp.'³² From Umm Raus we see the wall running inland for a distance of about 7 miles, with rounded bastions at intervals for the distance of $2\frac{1}{2}$ miles. It is clear from air photographs taken at about 4,000 ft. that it is a very ancient construction, for two reasons: because its patination is identical with that of the surrounding desert (for, when things are newly cut in the desert they show up differently in colour from their surroundings when seen from above) and, further on, we see where the bastions end, and the wall is continued by a mere trench, that the trench is deeply cut across by *wadis*, or ancient watercourses, of long standing. The trench at this stage follows a line which was set out by markers in the form of small double bunkers placed at intervals through which it had to go. In 1953 the wall was inspected at our request on the ground by Mr D. J. Wiseman, and followed in greater detail by Capt. R. W. Huntington, then at R.A.F. Station, Habbaniyah. The latter kindly reported that the wall appeared to be about 35-45 ft. broad, with bastions projecting about 20 ft. to 25 ft., set at a distance of about 190 ft. from axis to axis. At its highest point the mound made by the wall stood about 7 or 8 ft. high. From the air it can be seen that there are about forty buttresses in all. A drawing made from these photographs is shown in FIG. 1.

Now it is very difficult, without further details and scientific examination on the ground, to pass an opinion which can be relied upon as to the date of such a fortification. Normally, one would expect walls with semi-circular bastions or towers to be Roman, of about the third century A.D., such as are found at Nicaea in Asia Minor. And indeed at first sight this wall at Umm Raus seems to resemble Al-mutabbaq at the Tigris side. But the measurements of tower intervals and sizes differ. Indeed the possibility that a wall with these round bastions may be much older than Roman period need not be excluded. The city wall of the ancient Sumerian city of Uruk had over 800 projecting rounded bastions in its circuit.³³ The west wall of the city of Asshur, one of the capitals of Assyria, built of mud brick and stone by Sennacherib at the end of the eighth century B.C., was marked by small

³⁰ Lane, *Babylonian Problems* 42, points out this feature and illustrates it in a photograph (his Plate 5).

³¹ Poidebard, *La Trace de Rome dans le désert de Syrie*, pls. 38-9.

³² *The Middle Euphrates* 154. It seems that this

wall or fortification was observed but never recorded, during or just after World War I; see Major Mason, *loc. cit.* (note 72).

³³ von Haller, *Uruk-Warka* 7. *Bericht* 41-45, pl. 35.

projecting rounded bastions at short intervals (PLATE II).³⁴ The north Syrian city of Senjirli, built about the same time, had its inner citadel wall with similar rounded bastions on a basis of stone and wood.³⁵ We can only say that such a construction at that period—the eighth century B.C.—was unusual but not unknown, and may have been evoked specially to meet the improvements in siege warfare and the use of battering rams, introduced by the Assyrians themselves, to which these round surfaces sought to avoid a ‘purchase’.³⁶ The interesting thing is that we have here in this Wall at Umm Raus a feature which in its location and general dimensions would seem to qualify well to be identified with the trench hastily thrown up by Artaxerxes against the invading army, described by Xenophon as 5 fathoms wide (about 30 ft.) and 3 fathoms deep (about 18 ft.), and as running up, i.e. into rising ground, a distance of 12 parasangs, to the Median Wall. This last detail he was not in a position to investigate, obviously taking the words of his guide for it, but a distance of 12 parasangs would certainly take us towards the Tigris bank, where a similar defensive feature is placed at Al-mutabbaq. It may well be that both these walls with bastions were a

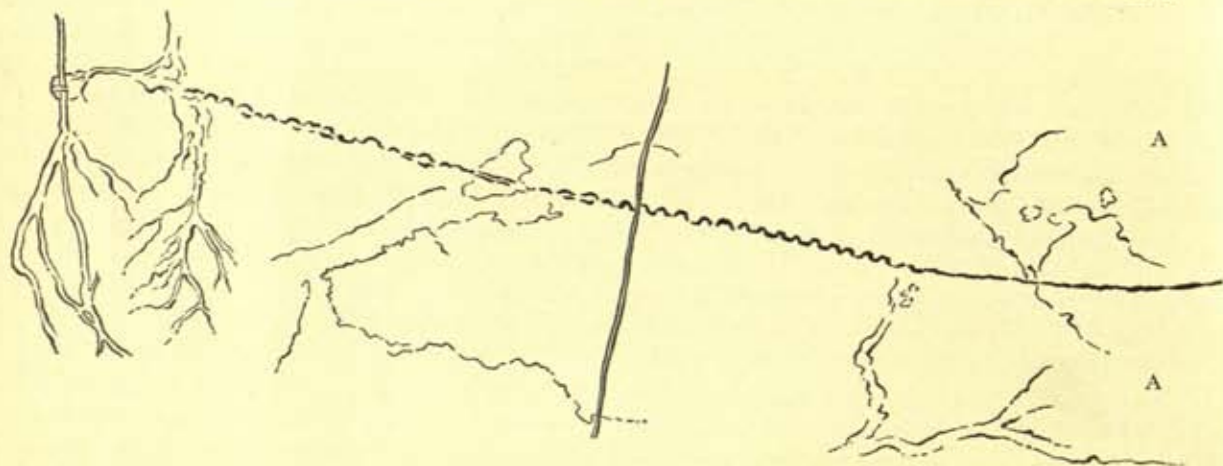


FIG. 1. Traces of Wall at Umm Raus (joining at A-A).

later, e.g. Roman, addition, built on an earlier foundation, but for the present this possibility must be left open. What seems likely is that they mark the course of the early defensive line, the western edge of which, in the form of Xenophon's trench, can still be seen on these photographs.

It is unlikely that these walls shown in this photograph were of so late a date as the Roman period, for this reason: in A.D. 363, Julian, as stated above, marching down the east bank of the Euphrates, reached Ozogardana, which is identifiable with Hit. It was found abandoned and burnt; then *ad vicum Macepracta pervenit in quo semiruta murorum vestigia videbantur, qui priscis temporibus in spatia longa protenti tueri ab externis incursionibus Assyriam dicebantur*.³⁷

From this it would appear that Macepracta must then have been the name of the already ancient walls at Umm Raus. It is difficult to see to what other walls this description could apply.

The Canals. At this point, we have to enter a digression on the canal system of northern Babylonia. This is a subject which is partly rendered necessary by a passage in the

³⁴ Andrae, *Das Wiedererstandene Assur* pl. 72.

³⁵ Von Luschan, *Ausgrabungen von Sendschirli* ii pls. 15, 16, 30.

³⁶ Dr Gadd points out to me that it is now claimed that battering rams were known in the Old

Babylonian period, being called *walibum* in the Mari letters. Kupper, *Revue Assyriologique* xlii 139-45, 125. But the great period of their use was by the Assyrians in the early Iron Age.

³⁷ Ammianus Marcellinus xxiv 2.

text of Xenophon immediately following his mention of the trench which runs up to the Wall of Media and which his army passed before the battle. He says: 'There are there [said to be, *ἐνθα δὲ*] the canals which flow from the Tigris. They are four in number, a hundred feet (*πλεθριαῖαι*) broad, and very deep, and are navigable by ships carrying corn. They empty into the Euphrates, each one at the interval of a parasang, and they are crossed by bridges.' Since the only canals which most scholars are acquainted with and consider (mistakenly) that Xenophon must have crossed, derive from the Euphrates and flow towards the Tigris, Xenophon's statement has been coolly dismissed as a later gloss, referring to four well-known Euphrates canals [the Saklawiye (or Nahr Isa), Sarsar, Nahr Malkha and Cuthiya] of later times.³⁸ But if we for the moment ignore the question of their source, the fact remains that several such canals as Xenophon describes, existed in the area south of the Umm Raus wall and it was on them, and on the irrigation system cunningly developed by means of them, that the rich agriculture of Babylon depended,³⁹ but our information concerning their pattern is as yet too incomplete for us to grasp exactly to which canals

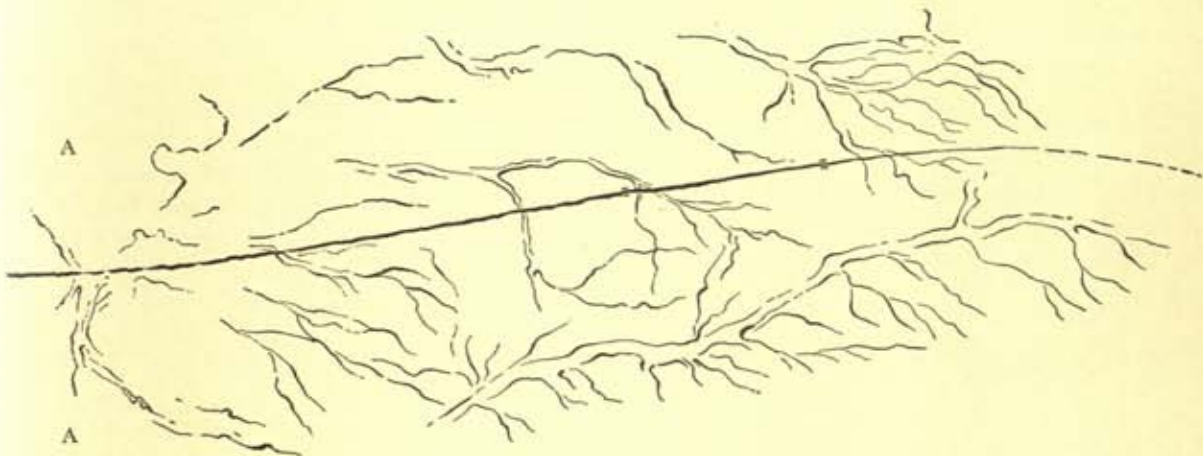


FIG. 1.—contd.

he refers. Unfortunately, canals form a subject which has been too long neglected by the archaeologist. A century ago, Commander Selby, Captain Jones and Lieutenants Collingwood and Bewsher made an excellent beginning of the work of trying to plan and plot these canals,⁴⁰ but the knowledge of the time was quite insufficient to make great progress with the task, and, even though today the remains of the high banks of many of these canals are clearly visible above ground, it is extremely difficult without very expert study to decide which are ancient, which belong to the Sassanian, which to the early Islamic period, and which to more modern times. Often a canal may have served for very many periods, at least for part of its course. There may be historical references which can be used, but are not always easy to understand or to reconcile with each other. Some canals were conducted between banks on the level of the plain, not cut into it, and thus easily became silted up, or became useless by the shifting of the river bed. It is obvious from a glance at the positions of the ancient sites of Babylonia that have been identified, and of the tells representing those which have not, that these lie along lines representing extinct water-courses of this kind. It was first in 1953-4 that the American School of Oriental Research

³⁸ See Musil, *op. cit.*, Appendix VI, 'The Canals of the Middle Euphrates'. On these canals, see Streck, *Die Alte Landschaft Babylonien* (1900-1); Alois Musil, *op. cit.*; Lestrang, *The Lands of the Eastern Caliphate* (1905); and Obermayer, *Die Landschaft Babylonien* (1929).

³⁹ See on this, Laessle, 'Reflexions on Modern

and Ancient Oriental Waterworks', *Journal of Cuneiform Studies* vii (1953).

⁴⁰ 'Surveys of Ancient Babylon' by Commander W. Beaumont Selby, Lieutenant W. Collingwood and Lieutenant J. B. Bewsher (London, 1885). See note 77.

in Baghdad seriously faced this basic question of Babylonian geography,⁴¹ having successfully already undertaken a smaller survey of the central Diyala region, to the east of the Tigris. The area of Central Babylonia was then surveyed by Robert Adams and Vaughn Crawford, who published a brief preliminary report in 1958.⁴² Though the area with which they were concerned was mostly further south than that which interests us, it yet has some relevance (FIG. 2). It shows that, at least until the Kassite period, i.e. the end of the second millennium (they did not yet make any report on the first millennium B.C.) the Euphrates flowed through Sippar as far as Warka (Uruk) and beyond. This had always been assumed by modern scholars, on the strength of texts and other evi-

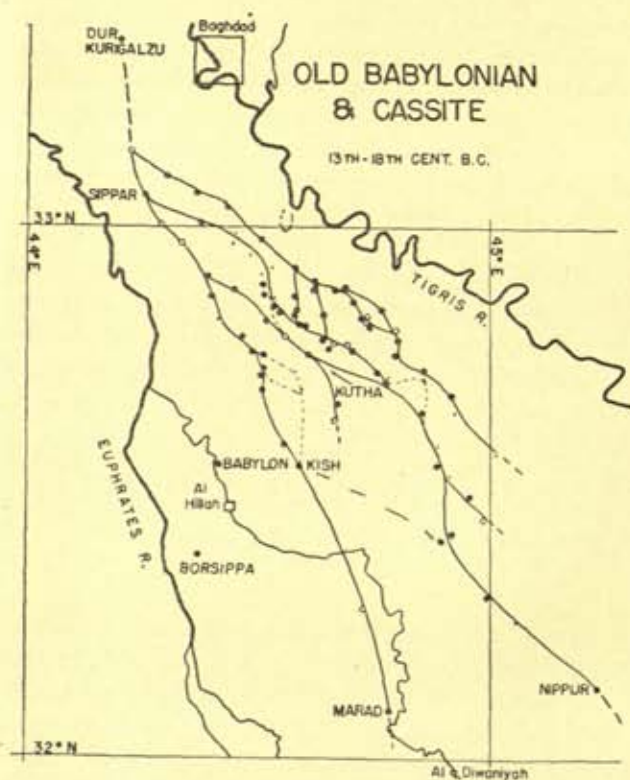


FIG. 2. Diagram showing old course of Euphrates and canals on left bank. From *Sumer* xiv (1958) 102, Fig. 6.

dence (for example, the name of the river Euphrates being written in cuneiform as ID.UD.KIB.NUN.^{K1} 'the river of Sippar') but the original course of the river had nowhere been properly plotted before. Unfortunately, this expedition has as yet only partly surveyed the left bank of this ancient Euphrates course, so that much more still remains to be done, e.g. about its canals on the other, or west, bank, towards Babylon. Again, the American expedition has not yet published a map of the canals of the neo-Babylonian period at all. Nor does any definitive study exist even to show from cuneiform sources what cities were still flourishing in that period and in that of the Achaemenians.⁴³ But

⁴¹ See 'Mesopotamian Mound Survey', *Archaeology* vii (1954); Goetze, 'Archaeological Survey of Ancient Canals', *Sumer* xi (1955) 127-8.

⁴² Robert M. Adams, 'Settlements in Ancient Akkad', *Archaeology* x (1957) 270-3; 'Survey of Ancient Watercourses and Settlements in Central Iraq', *Sumer* xiv (1958) 101-4.

⁴³ Tablets dated under Achaemenid kings have been found, dated and inscribed from the following cities: Borsippa, Babylon, Sahrinu, Sippar, Nippur, Dilbat, Hubadisu, Kutha, Uruk, Ur (information from Mr D. J. Wiseman).

the American expedition's already published report and diagrams emphasise one important fact—that the Euphrates ran down to Sippar, due south, from the Kassite capital of Dûr-kurigalzu (Aqar Qûf), which today lies in a marshy space called the Aqar Qûf depression, a little west of modern Baghdad.⁴⁴ This prompts the question which does not seem to have been asked: how did the Euphrates get to Dûr-kurigalzu? The answer can only be: that in ancient times it must have left its present course where it meets the soft alluvium west of Aqar Qûf—namely, in the neighbourhood of Al-anbar above Falluja, where a watercourse, known to be of great age, called by various names—Nahr Isa,⁴⁵ Dukeil or Karma, branches off and runs eastwards towards the depression of Aqar Qûf. At Aqar Qûf, banks of a great river course, running from the west then turning sharply south towards Sippar, can here be clearly recognised from the air. From Sippar in neo-Babylonian times, the river seems to have been deflected largely into the Arahtu canal⁴⁶ to flow through Babylon, and in fact, the waters of Euphrates and Arahtu were deemed to be as one. In Babylon, the left bank was called the Arahtu bank, the right, that of the Euphrates.⁴⁷ It is this pattern of rivers which, I believe, enabled Strabo and other Greek writers to describe the shape of Mesopotamia, with its sharp bends in the Euphrates, as resembling a rower's cushion or seat, in a trireme (ὕπηρέσιον).⁴⁸ But we may now be justified in tracing the next step in the history of the Euphrates. In an article published in 1899, Meissner⁴⁹ connected a very important Babylonian canal the AP.KAL or Apkallatu (which, in Hellenistic times, was called the Pallacottas canal), with the site of Falluja, known in Syriac as Pallughtha, a little below Al-anbar. We have a valuable description of the Pallacottas and its importance in the first century by Arrian (quoting Aristobulus) in his work on Alexander.⁵⁰ Alexander, with his usual restless energy, constructed a harbour at Babylon large enough for a thousand ships.

'While the new warships were under construction and the work of dredging the harbour proceeded, Alexander sailed from Babylon down the Euphrates to the river known as Pallacottas, about 800 *stadia* downstream from the city. The Pallacottas is not actually a river rising from springs, but a canal leading off from the Euphrates. Now the Euphrates, which rises in the mountains of Armenia, is in winter a shallowish river and runs well within its banks, but in spring, and especially round about the summer solstice, its volume is greatly increased by the melting of the snow in the Armenian mountains, so that the water, rising above the level of its banks, floods the neighbouring Assyrian plains. At least, this flooding would inevitably occur were it not for the cutting by which its waters are diverted along the Pallacottas into the marshes and lakes which continue from that point almost into Arabia, and passing thence over a vast area of swampy land, finally reach the sea by a number of ill-defined channels.

'In autumn, at the setting of the Pleiades, after the snows have melted, the level of the Euphrates drops, yet even so, most of its water continues to find its way along the Pallacottas canal into the lakes; thus, unless the canal were closed by a sluice, to block the entrance of the river-water and allow it to flow along its proper channel, it would,

⁴⁴ Dûr-kurigalzu, founded by the Kassite king, Kurigalzu I (c. 1400 B.C.) appears to occupy the site of an older Sumerian city named Esâ. Poebel, 'The City of Esâ', *Miscellaneous Studies* (Chicago, 1947). (I owe this reference to Dr E. Sollberger.)

⁴⁵ Poebel, *op. cit.*, plausibly suggests that this name, used in Islamic times, and popularly said to refer to the 'Isa, the uncle of Mansur, is, in fact, an adapted recollection of Esâ.

⁴⁶ See T. Jacobsen, 'The Waters of Ur', *Iraq* xx (1960) 175.

⁴⁷ E. Unger, *Babylon, die heilige Stadt* (1931).

⁴⁸ Strabo ii 1.23. In xvi 1.22 he says that Mesopotamia contracts in shape, projecting to a considerable length; the shape of it somewhat resembles a boat, and the greatest part of its periphery is formed by the Euphrates.

⁴⁹ B. Meissner, 'Pallacottas', *Mitteilungen der Vorderasiatisch-ägyptischen Gesellschaft* i (1896) 177-89.

⁵⁰ vii 22.

at this season of the year, empty the Euphrates completely, and so prevent the irrigation of the Assyrian plains. The construction of such a sluice was undertaken by the governor of Babylonia; it proved a tremendous task and the result was unsuccessful, as the soil at that point is mostly soft, wet clay which is easily penetrated by the water of the river. Consequently, it was no easy matter to keep it from percolating into the canal, though for three months over 10,000 Assyrian workmen were kept on the job.

'When these facts came to Alexander's knowledge, he was anxious to do something to improve Assyria's prospects. Accordingly, he proposed to construct a really efficient sluice at the junction of the canal and the river; however, at a spot some four miles lower down he observed that the soil was of a harder and stonier nature, and it occurred to him that if a new cutting were carried from that point into the Pallacottas canal, the problem might be better solved, for the water would be unable to penetrate the hard, impermeable ground, and could easily be shut off by the sluice at the proper time.'

The same story in abbreviated form is quoted by Strabo,⁵¹ writing about a century before Arrian. Some confusion has been caused to scholars who understand by this reference that the Pallacottas ran south of Babylon and was 800 stadia long (a hundred miles), which, in any case, is a wild exaggeration. But, in fact, Arrian need not mean at all that Babylon was its point of commencement. On the contrary, his statement can be explained by Meissner's theory that Pallacottas is represented by the name of the modern village of Falluja, the earlier Syriac name of which was Pallughtha. Pallughtha is a word derived from Semitic root 'plg' meaning 'division'; for example, in *Genesis* x 25, we meet the name of one of the descendants of Noah whose name was Peleg, who was so called, we are told, 'for in his days the earth was divided'. In Akkadian, *palgu* and in Hebrew, *peleg*, means a canal, and the Syriac name *Pallughtha* means 'regulating' of a river.⁵² Pallughtha, in fact, in Babylonian times, was the place called in Babylonian Pallukat, founded by Nebuchadnezzar, and the very significant information is preserved that Pallukat paid the tribute of a tithe to the city of Sippar.⁵³ We may easily conjecture that it was because at or near this point, where the waters of the Euphrates were heavily deflected, Sippar lost a large part of its water, and the wealth it brought. What then has become today of the upper course of the Pallukat or Pallacottas canal? For it seems to be lost. I think the explanation is, that at some date after Aristobulus, and perhaps well after the time of Ptolemy, the Euphrates changed its course completely into the channel of the Pallacottas canal, doing what Aristobulus had described as the ever-present danger. According to his remarks, the junction of the Pallacottas with the Euphrates (at the Macedonian settlement to which Pliny gives the name Bura⁵⁴) is to be sought a little below Falluja. The Euphrates' main channel must then, at the time of Alexander, have struck south-eastward towards Sippar at this point. Traces of this Euphrates' second phase are probably marked by the ancient Abu Ghureib watercourse, now dried up. The Pallacottas, meanwhile, ran more or less to the west of Babylon, being linked with Babylon by another famous canal, the Arah̄tum, which ran down through Babylon, perhaps from Sippar.

Cyrus the Great's Capture of Babylon. We may perhaps digress to touch here in parenthesis the question of the fall of Babylon in 539 B.C., when Cyrus the Great, the namesake of Xenophon's hero, marched successfully into Babylonia and overthrew its defences, so carefully constructed by Nebuchadnezzar. Herodotus⁵⁵ has a circumstantial account of how Cyrus turned the waters of the Euphrates aside from following their course into Babylon

⁵¹ xvi 1.9-11.

⁵² 'Pallughtha—nicht eigentlich ein Kanalname, sondern ein Ausdruck für die Regulierung des Euphrat selbst', Herzfeld, *op. cit.*, 13.

⁵³ Meissner, *loc. cit.*, 186.

⁵⁴ *NH* ii 5.30.

⁵⁵ i 189-91.

by switching them into the great lake which Nitocris, i.e. Nebuchadnezzar, had prepared, presumably that on the north side of the Median Wall, intended as part of the defences of the city and which Herodotus himself describes, attributing the lake to Nitocris. An account somewhat similar to Herodotus' is followed by Xenophon himself in his *Cyropaedia*.⁵⁶ There is no confirmation of this story from Oriental sources. The *Book of Daniel* does not know it—only that Babylon's fall was by surprise during a feast.⁵⁷ The *Babylonian Chronicle*⁵⁸ describes Cyrus crossing the Tigris at Opis, where he won a battle in the month of Tishri (September), and Sippar surrendered. He then advanced towards Babylon, but did not enter it until the seventeenth day of the month of Mar-Heshvan (October). The delay may be explained if we suppose the story of the draining of the river Euphrates to be true, not by his filling the lake north of the Wall (which would surely have been done already by the Babylonians if Babylon was to be put into a posture of defence), but by the probability that Cyrus sent a detachment on to Falluja to switch the Euphrates (which was then already at low water, being October) into the course of the Pallacottas channel or into the lakes. This would certainly have had the effect desired of emptying the waters of Babylon, and would make much better sense as an explanation than the story reported to the Greeks.

Pirisaboras and Macepracta. From Sippar, another great canal, called the Royal canal, or in Babylonian, *nar šarri*, in Aramaic, *nar malkha*, seems to have taken off in an easterly direction to meet the Tigris at Opis. It was certainly of great antiquity.⁵⁹ By the first century B.C., its point of exit from the Euphrates was called Neapolis, 22 *schoeni* (the equivalent of parasangs) below Besēchana (Al-anbar above Falluja), and according to Isidore of Charax,⁶⁰ this was the route by water to Seleucia. By the late first century A.D., the section nearing the Tigris had silted up and was reopened by Severus and again by Trajan.⁶¹ At the same period, however, it would seem, the first, or original bend of the Euphrates turning sharply eastwards at Al-anbar along the Nahr Isa channel (*alias* the Karma or Saklawiye) was still in full use, as Pliny shows⁶² when he describes how, at the village of Massicé (Isidore's Besēchana, Parthian Mšyk,⁶³ later Pirisaboras) 'the Euphrates divides into two channels, the left one of which runs through Mesopotamia past Seleucia, and falls into the Tigris as it flows round that city. Its channel on the right runs towards Babylon, the former capital of Chaldaea, and flows through the middle of it.' But by Pliny's time, the name Nahr malcha had been extended for some reason to the Nahr Isa channel, for he goes on: *Sunt qui tradunt Euphraten Gobaris praefecti opere diductum esse ubi diximus findi, ne praecipiti cursu Babyloniam infestaret, ab Assyriis vero universis appellatum Narmalchan quod significat regium flumen. Quā dirivatur oppidum fuit Agranis e maximis, quod diruere Persae.* The canal in question would appear to be the Nahr Isa, but Agranis is quite unknown from other sources.⁶⁴

⁵⁶ vii 5.

⁵⁷ ch. v.

⁵⁸ Wiseman, *Chronicles of Chaldaean Kings* (1956).

⁵⁹ According to Pallis, *The Antiquity of Iraq* 10, Herzfeld, *op. cit.*, it is first mentioned in the time of Nazimaruttaš. Pinches, *JRAS* 1917 137, 'An Early Mention of the Nahr Malka', publishes a tablet from Jokha (Umma) of the time of Bur-Sin of Ur, mentioning Sura and the Canal of the King.

⁶⁰ *Parthian Stations* 1.

⁶¹ Ammianus Marcellinus vi 1.

⁶² *NH* v 21.90.

⁶³ For the identification, see Honigsmann and Maricq, *Res Gestae Divi Saporis* 110 ff., from the newly discovered trilingual of Naksh-i-Rustem.

W. B. Henning, 'Βεσήχανα πόλις: ad BSOAS xiv 512 N. 6', *Bulletin of School of Oriental Studies* 1953 xv/2.

⁶⁴ The identification of the site of Agranis is highly obscure, but it should be near Al-anbar. Dilleman (*loc. cit.*) has complicated matters by twisting a passage in Pliny 'sunt etiamnum in Mesopotamia oppida: Hippareni, Chaldaeorum doctrina et hoc sicut Babylon—juxta fluvium qui cadit in Narragam, unde civitati nomen' (*NH* vi 30.123) to bring Agranis into connection with Sippar. In about 1090 B.C. Tiglath-pileser I marched against Babylon and captured the cities of Dur-kurigalzu (= Aqar Qûf), Sippar-of-Shamash (= Abu Habbah), Sippar-of-Anunitum and Babylon, and returned via Opis. The site of 'Sippar-of-Shamash' (Abu Habbah) is well known,

From the mention of 'all the Assyrians', however, it is clear that the standpoint of the description is outside Babylonia, i.e. north of the Nahr Isa. Agranis must therefore have stood near Al-anbar or Pallukat.

In A.D. 363 the Roman Emperor Julian marched down the Euphrates against the Persian army of the Sassanians, and his itinerary is recorded in the eye-witness account of Ammianus Marcellinus.⁶⁵ After passing a city which Ammianus calls Ozogardana (identifiable as Hit, on the Euphrates, by his mentioning the springs of bitumen which are found there), he says they came to the village of Macepracta in which were seen the half-destroyed remains of walls that in former times stretched to a long distance to protect the country from invasions from without. These walls we have discussed. Here, he goes on to say, the river divides into great arms, one leading to the inner regions of Babylonia, the other, which is called the Nahar Malcha, or 'royal river', flows through Ctesiphon. In calling it the Nahar Malcha (which lies further south) some contend Ammianus was mistaken; otherwise he was correct: but as I have shown, the Nahr Isa appears to have been then so called. At the beginning of this stream, there was (he says) a lofty tower like a lighthouse, by which the infantry passed on a carefully constructed bridge. After Macepracta, they came to the city of Pirisaboras and took it. Now, Pirisaboras is without any question Piruz-sabur, 'victorious Sapor', a city previously called Mšyk (or Massicē by Pliny) or Besēchana, rebuilt by Sapor II in the first half of the fourth century, and scholars are on the whole agreed that it is represented by the ruins called Al-anbar, 'the arsenal', just north of Falluja. It may be remarked that Al-anbar, or Pirisaboras, was a centre which included a very important Jewish settlement, called Pumbaditha, famous for its academy of Talmudic learning from A.D. 259 to 342, the identification of Pumbaditha with Al-anbar being well known to the Spanish-Jewish traveller, Benjamin of Tudela, in the twelfth century A.D.⁶⁶

but that of the other Sippar, that of Anunitum, is not. It clearly lay between Abu Habbah and Babylon, but is not mentioned again. It is, however, known that Sippar-of-Anunitum was a next-door neighbour of the city of Akkad (the site of which is likewise unknown), being separate from it only by a canal called the *nār Agādē* or 'river of Akkad' (Ebeling, *Reallexikon der Assyriologie* s.v. 'Akkad'). But the reference in the Bible, 2 Kings xix. 34, to the name of Sepharvaim, a city which the Assyrians claim to have destroyed, being a dual form, has suggested to some that both Sippars existed into the seventh century B.C. In fact, both Akkad and Sippar-of-Anunitum survived certainly into the sixth century B.C., for Nabonidus rededicated an identically-named temple in each (Ebeling, *loc. cit.*). Sippar-of-Shamash lasted certainly into Achaemenid times. While, therefore, it would seem that Sippar-of-Anunitum formed a twin city with Akkad, it is not clear that Sippar-of-Anunitum had any intimate geographical connection with Sippar-of-Shamash. Dilleman, however, claims that Sippar(-of-Shamash) formed a double city ('ville jumelle') with a non-existent city, Agané (which is evidently a misreading of the name Agādē = Akkad), and sees in a hypothetical **nār-Agane* the interpretation of both Agranis and 'Narraga' of Pliny. But Andrae and Jordan examined the terrain around Sippar(-of-Shamash) in 1927 in detail and could find no trace of a second twin city ('Abu-Habbah-Sippar', *Iraq* i (1934)). It is, however,

perfectly possible that Pliny, in mentioning in his almost certainly garbled passage the river Narraga, near Sippar, was referring to the *Nār-Agādē*. The passage should probably be amended: '*Hippareni, Chaldaeorum doctrina et hoc sicut Babylon juxta fluvium Narragam qui cadit in <Euphratem> unde civitati nomen*', the last statement being perhaps a clumsy attempt to derive Hippareni from Euphrates; it may even reflect some confused knowledge of the fact that the Euphrates was once called 'the river of Sippar'.

It is usually assumed that Hippareni refers to Sippar (Ptolemy's Sippara), but even that requires proof, since the change of 's' to 'h' is strange. In 1921 Andrae and Jordan, a short distance to the east of Sippar, examined another massive ruined site named Tell-ed-Deir, surrounded by a wall, dated at least to the 1st dynasty of Babylon.

To the east side, the still-visible defences of Tell-ed-Deir are formed by a dried-up stream bed. Whether another 'twin' city lay on the far side of the bed cannot be stated.

⁶⁵ See Dilleman, *loc. cit.*, for a detailed evaluation of Ammianus' and Zosimus' testimony.

⁶⁶ Elkan Adler, *The Itinerary of Benjamin of Tudela* 53 (34). 'Thence (from Rahbah) it is a two days' journey to Karkisiya. [Circesium, Deir-ez-Zor.] Thence it is two days to El-anbar, which is Pumbedita in Nehardea.' Benjamin evidently travelled by boat or raft. Nehardea is used apparently to describe a district.

Pumbaditha means in Aramaic, 'mouth of separation', i.e. where the rivers divide. The chief watercourse which took off then from the Euphrates was the navigable canal now called Nahr Isa or Saklawiya, but called in Sassanian times the Šunaya canal, from the city of that name situated on the Tigris, where the canal debouched. The Šunaya at its exit from the Euphrates was crossed by a great bridge called the Kantara Dimimma. This is, of course, the bridge mentioned by Ammianus. At Pumbaditha was a population of 90,000 Jews. It was the seat of an autonomous Jewish community under the Exilarch, or ruler of the Exile who resided there with a bodyguard of 400 men officially recognised by the Sassanian king.⁶⁷ The size and substantial character of the remains near Falluja (i.e. at Al-anbar?) may be gauged from the account of a sixteenth-century German traveller, Dr Leonhart Rauwolff, who was so impressed that he thought he was in ancient Babylon. His German manuscript, formerly in the Arundel Library of Gresham College, was translated and published by John Ray in 1693.⁶⁸ In 1573, Rauwolff, on his way to Baghdad, travelled downstream from Bir by river to Falluja which he calls Felugo (or Elugo). He describes seeing, a little above Felugo, remains of an old bridge, 'pieces and arches of which were still remaining, of burnt brick, of great strength'. He remarks that this was the only bridge to be seen in the whole stretch from Bir, and is astonished at its construction, the river being there at least half a league broad and very deep; further, he saw just before the village of Falluja (i.e. at Al-anbar?) the hill 'whereon the castle did stand in a plain, whereon you may still see ruins of the fortification, which is quite demolished and uninhabited; behind it, pretty near to it, did stand the Tower of Babylon . . . this we see still, and it is half a league in diameter, but it is so mightily ruined and low, and so full of vermin that have bored holes through it, that one may not come near it within half a mile, but only in two months in the winter when they come not out of their holes'. If this was Al-anbar, the bridge, the citadel and the tower may have been those dating from the time of Julian. Remains of this great town can still be clearly seen from the air, north-east of Falluja, even the ancient streets being discernible, the ancient bed of the Euphrates can be clearly seen diverging from its present course, and the double city wall and citadel.

But let us return to the canals. Between Dûr-kurigalzu and Sippar, the names of at least two important canals are known. One of these was called the Patti-Bêl, on which Tukulti-Ninurta II rested in 884 B.C. when marching from Dûr-kurigalzu (Aqar Qûf) to Sippar.⁶⁹ Another was the Patti-Enlil, perhaps the same as a canal the name of which in earlier times was written as ME-Enlil. This, it has been suggested,⁷⁰ was a name for the section of the Euphrates between Dûr-kurigalzu and Sippar. But there seems little proof of this, and the Sumerian documents (to be quoted below) suggest rather that it ran on an easterly course between the Pallacottas and the Tigris. In addition to these, a famous canal called Libit-ġegalli, or Banitum, ran from Babylon through Kish to the Tigris. The diagram of the Euphrates canals below Sippar published by the American expedition shows there to have been two or three further networks, for which we await names. The canals which flowed from the Tigris have not yet been mapped. In the light of these facts, the statement in Xenophon's text regarding the three great canals must clearly be treated with respect.

Kunaxa. According to Xenophon, the battle took place at an unnamed spot 360 stadia (about 30 miles), but according to Plutarch, 500 stadia distant from Babylon. The

⁶⁷ Obermayer, *op. cit.*, 70 ff.

⁶⁸ John Ray, *A Collection of Curious Travels and Voyages*, 2 vols. (1693).

⁶⁹ See note 6. The Patti-Bêl was also called the river Pittia, Waterman, *op. cit.*, 883.

⁷⁰ Meissner, *loc. cit.*; according to Pallis, *op. cit.*, 10, the ME-Enlil was the name of the section from Pallukat to Sippar. This seems a little difficult.

Jacobsen claims the ME-Enlil 'left the Euphrates' right bank at Kish, 'The Waters of Ur', *Iraq* xxii (1960) p. 176 n. 1, p. 177. He bases this statement ostensibly on Kraus' article in *Zeitschrift für Assyriologie* li 57. But neither Kraus nor the interesting Sumerian text he published say anything of the kind.

battlefield is customarily identified with Tell [Aqar] Kuncesha (Kuncise) on a slope near the river, 57 miles north of Babylon (= 500 stadia).⁷¹ But I find this identification difficult to accept, as I shall explain later. If we take Xenophon's usual ratio of 30 stadia to a parasang, 360 stades would be about 12 parasangs, i.e. about 30 miles by road from Babylon, but Kuncise is much farther (57 miles). Major Mason, following Chesney, put the battlefield near Mufraz, north-west of Sippar.⁷² Herzfeld placed it at Falluja.⁷³

It is suggested by some that Pliny's Agranis or Agranum lived on, as Hagraunia, into the third century A.D.; this commonly-accepted identification may be, or perhaps more likely, is not, true, but in any event, Hagraunia abutted on the great Jewish centre and academy of Nehardea, to which it formed a citadel.⁷⁴ Nehardea, in fact, was the centre of the Jewish diaspora from the time of Josephus until it was sacked and destroyed by the

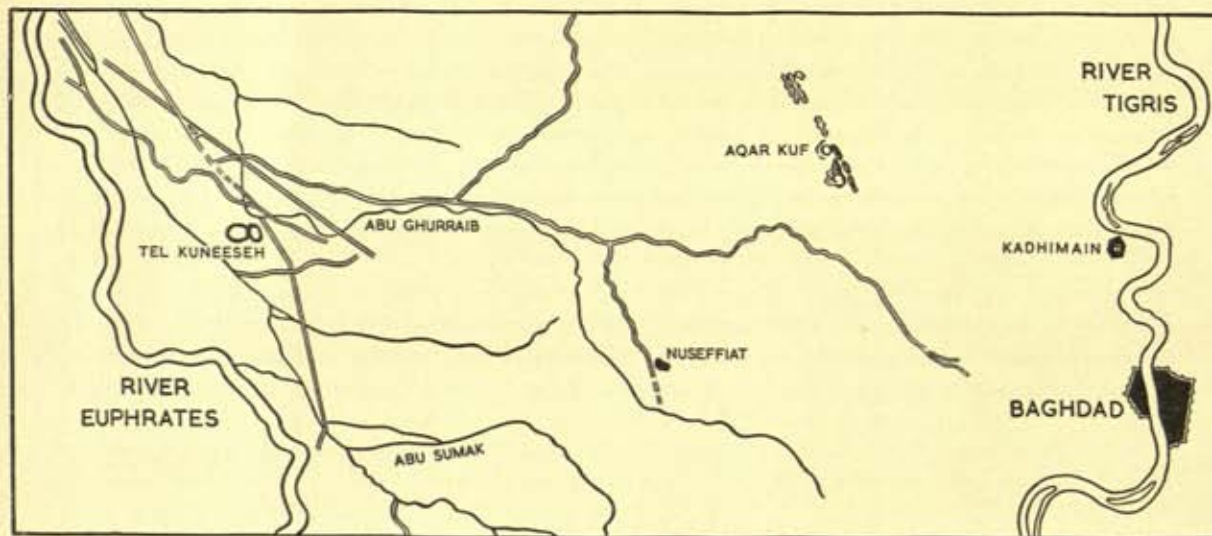


FIG. 3

Sassanian general, Papa bar Nasr, in A.D. 259; its place in Jewish life was then taken over by Pumbaditha, situated at Al-anbar.⁷⁵ North of Nehardea, however, at a distance estimated as 18 parasangs from Babylon and a day and a half's journey southwards from Pumbaditha, was the site of a synagogue, Kenishta dē Safyatib. It was a spot of particular sanctity to the Babylonian Jews, being said to contain stones and earth brought from the Temple at Jerusalem at the time of its destruction in 597 B.C.⁷⁶ I suggest that it is to be identified with the village of Al Nasiffiat or Nuseffiat, about 50 miles north of Babylon, which still, in 1861, preserved the remnants of its original name [*ku]neise-safyat[ib].⁷⁷ Further, I believe that this, not Tell Kuncise, is the true site of the battle (FIG. 3). The name Kunaxa is accepted as a Grecised rendering of the aramaic *Kenishta*, meaning 'a synagogue',

⁷¹ Colonel Lane, *Babylonian Problems*, calls it Tell Aqar Kanisah, and adds, 'it is 32 metres high, 182 ft. above sea level'. He, however, gives its distance from Babylon as 57½ miles. Lieut. J. B. Bewsher, 'On part of Mesopotamia contained between Sheriat el-Beytha on the Tigris and Tel Ibrahim', *JRGS* xxxvii (1867) quoting Chesney, gives it as 51½ miles in an air-line from Babel.

⁷² Major Kenneth Mason, 'Notes on the Canal system of ancient sites of Babylon in the time of

Xenophon', *JRGS* lvi (1920) 468 ff. He concludes that the battlefield cannot have been more than 35, or less than 28 miles from al-Anbar, the site of Cyrus' review.

⁷³ *Op. cit.*

⁷⁴ Obermayer, *op. cit.*, 244-78.

⁷⁵ *Ibid.*, 254.

⁷⁶ *Ibid.* 73 n. 1; 248-9.

⁷⁷ Selby and Bewsher, *Survey of Mesopotamia; Sheriat el Beythra to Tel Ibrahim* (1862-5).

and it is clear that the memory of the battle was preserved by the Babylonian Jews until Plutarch's time, or that of his source—Dinon or Ctesias.

For this is the lie of the land and course of the streams which must have faced Xenophon and the Greek army on their march to the battle and afterwards in their retreat, and failure to grasp it has vitiated all previous studies of their route. If the site of the battlefield is placed at Tell Kuneesha-Kuneise, it would imply that they had crossed the Euphrates. But there is no statement that they did so at all, either on their way to the battle or after: the contrary is implied. Admittedly, the passage about the canals⁷⁸ which follows the mention of the Median Wall contains a reference to bridges over the canals, and might conceivably be held to imply that the Greeks crossed the Euphrates by such a bridge at Al-anbar and Falluja and marched down to Tell Kuneise *between* the Pallacottas and the old Euphrates course running through Sippar. Yet this is disproved by the fact that Clearchus, as Xenophon tells us,⁷⁹ held the right wing in the battle with his Greeks flanked by the Euphrates, so it must have been fought either at a point on an east-west line between Al-anbar and Aqar Qûf (or perhaps if the Euphrates was already then running in the Abu Ghureib channel, between Falluja and Aqar Qûf, though this is less likely); alternatively, the battlefield has to be sought further south but still to the east of the old Euphrates course, between Dûr-kurigalzu/Aqar Qûf and Sippar. In fact, the Greeks, after the battle, were in a position 'surrounded by impassable rivers',⁸⁰ i.e. between the Tigris and Euphrates, which they had therefore still not yet crossed. We may work out their itinerary from Pylae as follows:

Pylae (Al-aswad) to <i>x</i> : (site of review):	3 days' march, 12 parasangs.
<i>x</i> to Trench to <i>y</i> :	1 day's march, 3 parasangs.
<i>y</i> to Kunaxa:	2 days' march, (?) 8 parasangs.

Passing the trench must have slowed matters down, but the march after the review when the army was in battle order, leaving the baggage train some way behind, is likely to have been somewhat faster than the usual rate in this region of 3 parasangs to a day. In the next two days, therefore, they may have accomplished 8 parasangs, say about 28 miles. The total march from Pylae (taken as Al-aswad) will then be 23 parasangs, say about 83 miles. The distance from Al-aswad along the river to Al-anbar is fully 40 miles, while that from Al-anbar to Aqar Qûf is about 33; from Aqar Qûf to Al-naṣiffiyat, our suggested battlefield, is about 10; total 83. Al-naṣiffiyat, at the distance from Babylon prescribed by Plutarch (50 miles or 350 stadia), thus fits the site of Kunaxa. Xenophon's informants, for good reasons, understated the distance to Babylon so as to encourage him.

Kunaxa to Sittace. After the battle, their itinerary seems even more obscure, but luckily, something can be done to interpret it. The king withdrew his main army across the Tigris.⁸¹ But the Greek force, which had distinguished itself in the fighting, proudly refused to consider itself defeated, and, after some delay, decided to retreat, but by a different route from that which they had come, which, though it would be longer, would be assured of supplies. Joined by the army of Ariacus, who had held the left wing, they marched to the baggage which they had left at the last camp, 4 parasangs back (say 14 miles: i.e. 4 miles along the road west from Aqar Qûf to Ad-dam), then made for some unspecified villages in the Babylonian countryside, marching swiftly for a whole day 'with the sun on their right hand', i.e. in a northerly or north-easterly direction, crossing two canals. The next day, they pressed on over ditches and canals to other villages where they found enough provisions including palm wine to last them for a stay of twenty-three

⁷⁸ i 7.14.

⁷⁹ i 8.4.

⁸⁰ iii 1.1; cf. ii 2.3; 4.5.

⁸¹ ii 4.5.

days⁸² while a truce was being negotiated. Let us assume they marched north 4 parasangs on the swift march of the first day, and, hampered by the canals, only 3 on the second, a total of 7 parasangs (about 25 miles) north or north-east from Ad-dam, which would bring them to the neighbourhood of Al-ehmedi or Megasse.⁸³ After this, the Greeks and Ariacus' native army agreed to march away under a safe conduct and, in a three days' march (say 12 parasangs = 45 miles) reached the 'so-called Wall of Media' and passed over to the other (inner) side of it. The distance of 45 miles from Al-ehmedi brings us roughly back to Seleucia and the region of the Wall. If this is so, however, they must have passed through the Median Wall, not from south to north, but from north to south (εἴσω) 'within', as he says, and within the lands it protected, or was meant to protect, and were marching away from the Median Wall in a south-easterly direction.

The Wall of Media. This wall, Xenophon says, was made of baked brick laid in bitumen, and was 20 ft. thick, 100 ft. high, and *was reputed* to be 20 parasangs long (about 75 miles!), and lay quite close to Babylon;⁸⁴ some of these figures are palpably exaggerated. Then, in the course of a two days' march of 8 parasangs, they crossed two canals, after which they struck the Tigris near a place called Sittace.⁸⁵ Another four days' march of 20 parasangs brought them to the River Physkos, at the mouth of which was a great city named Opis.⁸⁶ Now it is unfortunate that the positions of neither Sittace nor of Opis are, as yet, positively known, nor is the River Physkos elsewhere mentioned. The important questions of their positions will be discussed later. But what was meant by this 'Median Wall', or 'Wall of Media'? We must remember with Herodotus, that 'the Medes under Cyaxares had conquered all Assyria up to Babylonia πλὴν τῆς Βαβυλωνίης μοίρης' (i 806) which elsewhere he defines as the richly-irrigated area of Mesopotamia, wholly cut up by canals (i 193). It is impossible not to connect this wall with a great defensive wall which, as has been known since 1912,⁸⁷ Nebuchadnezzar (604-561 B.C.) constructed as a sort of Babylonian equivalent of the Great Wall of China; whereas that was intended to keep out the Mongols, this was intended to keep out the Medes, who were then threatening his Empire. 'In the district of Babylon from the chaussée on the Euphrates bank to Kish, 4½ bēru long, I heaped up on the level of the ground an earth-wall and surrounded the City with mighty waters. That no crack should appear in it, I plastered its slope with asphalt and bricks. To strengthen the fortification of Babylon, I continued, and from Opis upstream to the middle of Sippar, from Tigris bank to Euphrates bank, 6 (?) bēru, I heaped up a mighty earth-wall and surrounded the city for 20 bēru like the fullness of the sea. That the pressure of the water should not harm the dike, I plastered its slope with asphalt and bricks.'

Here are some fairly explicit statements. The wall, running from Opis to Sippar, was 6 (?) bēru (about 25 miles) long, and Sippar was at that time on the Euphrates. This indeed was a fact so important to the Babylonians that the name of the river Euphrates was written in cuneiform as ID.UD.KIP.NUN.⁸⁸ literally 'the river of Sippar', UD.KIP.NUN being the Sumerian name of Sippar. It is well to remember this, for the site of Sippar is almost the only known fact in a welter of confusion. It is located by the evidence of excavations at Abu Habbah, now 10 miles to the east of the Euphrates, which has clearly changed its course. Nebuchadnezzar's Wall thus formed a major part of the defences of Babylonia, and, though it proved of no protection against Cyrus the Great when the day of reckoning came in 539 B.C., and Cyrus marched down to Opis and against Sippar and Babylon, nevertheless its fame reached Greek ears. Herodotus⁸⁸ has a long

⁸² ii 3.14.

⁸³ Al-ehmedi: Musil, *op. cit.*, 149.

⁸⁴ ii 4.9, 12.

⁸⁵ ii 4.13.

⁸⁶ ii 4.25.

⁸⁷ Wadi Brisa inscriptions: Weissbach, *Die In-*

schriften Nebukadnezars II in Wadi Brisa (1906). A similar text on a cylinder from Dêr is published by S. Levy, 'Two Cylinders of Nebuchadnezzar II', *Sumer* iii (1947).

⁸⁸ i 185-6.

description of Nebuchadnezzar's works, disguised as the defences built by 'Nitocris', and Strabo likewise knew the Wall, but as the Wall of Semiramis.⁸⁹ By Xenophon's day it had come to be regarded as an advanced frontier protecting the Achaemenid province of Media. But the site of the ancient city of Opis (Akshak) is not exactly known. It has been assumed that Opis lay not far from Seleucia (Tell Umar) on the Tigris,⁹⁰ a site about 18 miles due east of Sippar. Opis, we know, was on the Tigris. It is coupled by Strabo⁹¹ with Seleucia as the limit of navigability of the Tigris in Alexander's time. According to Herodotus,⁹² it lay a little distance downstream below the junction of the Tigris with the Gyndes, perhaps the Diyala (called in Assyrian Turnat, Pliny's Tornadotus).⁹³ Opposite it was a ferry or crossing called Bab-bitki,⁹⁴ but it is not clear on which bank it was; Tiglath-pileser I (circa 1100 B.C.) says Opis is on the far (i.e. east?) bank.⁹⁵ but Nebuchadnezzar (in the text quoted above) seemed to imply it was on the west bank. For Xenophon (see below) it was evidently on the east side of the river: and this indeed is the consensus of most of the evidence. Strabo, quoting Eratosthenes, says⁹⁶ that 'the Tigris and Euphrates converge in the neighbourhood of the Wall of Semiramis and Opis, from which village' (how the once great city has shrunk!) 'Euphrates is distant more than 200 stadia', i.e. about 25 miles, if we take the ratio of 8 stadia to a mile.

In 1867, Captain Bewsher drew attention to the ruins of a wall, then called the *Habl-es-Sakhar*, which literally means a line of stone or bricks, between Tell Umar (Seleucia) and Khan el Azad (which he calls Khan-ez-zad). He says:⁹⁷

'The ruins of this wall may now be traced for about 10½ miles and are about 6 feet above the level of the soil. It was irregularly built, the longest side running E.S.E. for 5½ miles; it then turns to N.N.E. for another mile and a half. An extensive swamp to the northward has done much towards reducing the wall. The two caravanserais at Khan-ez-zad are also in a great measure built of bricks from it and it has doubtless supplied materials for many other buildings. There is a considerable quantity of bitumen scattered about, and it was probably made of bricks set in bitumen. I can see nothing in Xenophon which would show that this was not the wall the Greeks passed, for what he says of its length was merely what was told him. I think that this must be the ruin of the wall called that of Media which Xenophon describes; but I mention this supposition with much diffidence and for the benefit of those better able than myself to judge of its being correct. . . .'

Both Weissbach (1929)⁹⁸ and Herzfeld (1948) accept this identification: Herzfeld confirmed the *Habl-es-Sakhar*'s existence from the accounts of the engineers building the Baghdad railway in 1913 and indeed, it must be admitted that it seems to be in some ways in the right place for Nebuchadnezzar's wall. But it follows a very strange and erratic course for a defensive wall, only explicable by the presumed position of former swamps; it

⁸⁹ ii 1.

⁹⁰ L. Waterman formerly claimed that two cuneiform inscriptions bearing names of kings of Opis were found in the excavations of Seleucia. The texts (on two basalt slabs) are published, in translations only, by Waterman, 'Preliminary Report on the Excavations at Tel Umar', *Iraq* 1931 6, as 'Urur, king of Sumer, king of Akshak' and 'Undalulu, king of Akshak, six years'. See also *BASOR* 32 (1956) 18; *Archiv für Orientforschung* v 121; vi 35. [But Professor Waterman now kindly informs me that these inscriptions were really too worn to be deciphered, and withdraws these readings.]

⁹¹ xvi 1.9.

⁹² i 189.

⁹³ *Nat. Hist.* ii 30, 31.

⁹⁴ Waterman, *Royal Correspondence of the Assyrian Empire* 89. In the reign of Sharkalisharri, twenty-third century B.C., the king of Elam advanced as far as Akshak; this suggests it was on the east bank. Cameron, *History of Early Iran* 37-8.

⁹⁵ Luckenbill, *op. cit.*

⁹⁶ ii 1.

⁹⁷ Lieut. J. B. Bewsher, 'On part of Mesopotamia contained between Sheriat el Beytha on the Tigris and Tel Ibrahim', *JRGS* xxxvii (1867).

⁹⁸ *Μηδίας Τείχος* in Pauly-Wissowa, *RE*.

would seem to have been intended as an embankment holding in a vast lake north of Sippar which would correspond with that ascribed by Herodotus to Nitocris, and perhaps is not the Wall of 6 *bēru* between Opis and Sippar, but part of that of 20 *bēru* which contained the artificially filled lake. Unfortunately, we have no photograph of it, since none is published. However, if this is Xenophon's Wall of Media, as seems possible, then for a trench somewhere near Falluja to have reached to it, as Xenophon claims, is impossible; and if a wall *had* reached it from Falluja, it would have had no military sense or purpose.

The few clear facts which emerge from this picture indicate that there were, in fact, at least two series of defensive walls which were intended to protect the Babylonian plains: one, the trench of Artaxerxes, the other, the Sippar-Opis wall of Nebuchadnezzar. Xenophon's army passed through both of them, but he does not appear to have quite realised that they were distinct from one another. To take the second line first: the second, inner line of defence was the 'Magenot Line', built by Nebuchadnezzar from Opis to Sippar, which shows that he envisaged surrendering as indefensible a large and important area of fertile and populous country to the north. This shows the weak position which Nebuchadnezzar had already reached. Like the Maginot Line, it proved an illusory protection to a determined attacker such as Cyrus the Great, who was able to outflank it. The first line of defence lay much further north, where the alluvium begins on a line drawn very roughly from Ramadi on the Euphrates to Samarra on the Tigris. Inspection of the modern map shows that north of this line signs of ancient cultivation cease, for, apart from a few tells on the banks of the rivers and the Wadi Tharthar, there are hardly any more tells marked on the available maps.

The Northern Fortifications. Fifteen hundred years before Xenophon's time, the Sumerians found themselves faced with the problem of protecting their flourishing countryside and wealthy cities from the incursions of barbarian nomads from the north. The Third dynasty of Ur struggled to keep out the Amorite Bedouin or Martu as they were called, and Shu-Sin, King of Ur, dates the fourth year of his reign (2038-2030 B.C.) by the official description as that in which 'Shu-Sin constructed the wall called Muriq Tidnim—that which keeps out the barbarians'.⁹⁹ I am greatly indebted to Dr C. J. Gadd for generously allowing me to quote an unpublished letter in cuneiform found at Ur, in which the architect or commissioner of a king (presumably Shu-Sin), named Sharrum-bani, describes his building of the wall.¹⁰⁰

- (1) To [Shu-Sin, or an officer of his] say:
- (2) Sharrum-bani, councillor of
the assembly (?) speaks (thus):
- (3) 'To make a great wall, *Muriq-Tidne*,¹⁰¹
- (4) As commissioner I was sent.
- (5) It is now before thee; the Martu
to (their) land they have thrown (back)
- (6) To build a wall, to cut off that raid,
- (7) (So that) Tigris and Euphrates together,
- (8) A breach in them should not overwhelm the fields,
- (9) Thou has sent me an order.
- (10) With my levies . . . [i.e. corvée-workers]
- (11) From the bank of the APKAL-canal . . .

⁹⁹ Edzard, *Die Zweite Zwischenzeit Babylonien* (1952). Gadd and Kramer, *Ur Excavations, Texts*, vol. viii.

¹⁰¹ See above.

¹⁰⁰ U.16885. This letter will be published in

- (12), (13) - - - - - ?
 (14) That wall, being 26 *danná*¹⁰² . . . [long]
 (15) Between the mountain of Aiabu . . .
 (16) For my building, the Martu . . . (who?)
 (17) dwelt [there?] obedience [rendered?]
 (18) - - - ? - - - -
 (19) Between the mountain and (?) - - - -'
 [Little remains on reverse of tablet]

The purport of this remarkable letter seems to be this: Sharrum-bani has built an enormously long dyke, which serves the double purpose both of protecting the country from the floods of the two rivers and from the incursions of the Martu. Its course runs from the AP.KAL or Pallacottas Canal eastwards by Aiabu¹⁰³ (a small city on or near the Euphrates, the name of which was still preserved in the seventh century B.C. as Iâbi) from Falluja, south of the high ground, rising to 80 feet above the plain, described in exaggerated language as 'the mountain', which lies in the desert plain between Falluja and Aqar Qûf. How the wall can have been 26 *bêru* long, i.e. about 100 miles, is a little difficult to see. One can only imagine that it was continued along the banks of the Tigris and the Pallacottas Canal; alternatively, this figure, which is more than twice the distance between Tigris and Euphrates, represents a double line of wall in some way. It may, nevertheless, be no coincidence that it is the same as the two walls of Nebuchadnezzar added together, consisting of 20 and 6 *bêru* respectively. However, even this great wall was ineffective, and Ibbi-Sin, son of Shu-Sin, who reigned over Ur from 2029-2006 B.C., was already in difficulties by his sixth year. The Martu had penetrated his fortifications and, according to another letter, were said to be seizing one great fortress after another.¹⁰⁴ He was compelled to send a general named Ishbi-Irra to buy grain elsewhere in order to alleviate the famine which their invasion had precipitated. But Ishbi-Irra asks for a fleet of 600 boats to be sent to him by way of the river Euphrates, 'the river of the Mountain', and the 'dug Canals':

'To Ibbi-Sin my king speak; thus says your servant Ishbi-Irra:

'You have charged me with an expedition to Isin and Kazallu to buy grain. The grain has reached the price (of) 1 *gur* for each (shekel) . . . (and to date) 20 talents of silver have been spent for buying grain. But now having heard the report that the hostile Martu have entered your country, I brought into Isin the 72,000 *gur* of grain—all of it. Now the Martu—all of them—have entered the midst of the land (Sumer) (and) have seized the great fortresses one after the other. Because of the Martu I am not able to transport (?) that grain; they are too strong for me (and) I am immobilized. Let my king have 600 boats (with the capacity of) 120 *gur* each caulked; let him (?) . . . a boat (of?) 72 . . .; let him (?) 50 . . . (and) 1 door (and) . . . boat; and let him [collect] all (these) boats. (Then) let them be brought down to the narrow (?) . . . by (way of) the River, "the River of the Mountain" and the dug canals; and I will . . . before him. Put me in charge of the places where the boats are to be moored (and) . . . all the grain will be stored (?) in good condition. If you shall lack grain I will bring you the grain. My king, the Elamites have been weakened in battle, their grain . . . has come to an end. Do not weaken. Do not agree to become his slave, and do not walk behind

¹⁰² I.e., *bêru*, double-hour's march.

¹⁰³ A town called Aiabu is mentioned as on the Euphrates in a letter from Mari, *Syria* xix 121 ff. It is probably the same as Iâbi, mentioned in a text of the seventh century B.C., apparently in the neighbourhood of Ramadi—see Musil, *op. cit.*, 212-13.

¹⁰⁴ This letter is published in part only by Jacobsen, 'The Reign of Ibbi Suen', *JCS* vii 39-40. I owe a complete translation to the kindness of Professor S. N. Kramer. On this period see Jacobsen, *loc. cit.*, and Edzard, *op. cit.*, ch. 5.

him. I have (enough) grain for 15 years (to satisfy) the hunger of your palace and its cities. My king, put me in charge of watching over Isin (and) Nippur.'

Again we see the 'mountain' to be an important feature of the northern defence. In reply, Ibbi-Sin complains bitterly and storms that the 'Commandant of the Fortress before the Mountain' (Bad-Igi-Hur-Sagga) has failed in his duty to hold up the Martu.¹⁰⁵ In a letter to his master Ibbi-Sin, the offending officer, named Puzur-marduk, writes:¹⁰⁶ 'The enemy holds his forces ready for battle. The Wall is not strong enough against him', and he mentions the names of his neighbours, who include Taki-ilishu, Dyke-officer of the AP.KAL and ME.Enlil canals. Six years later, Ishbi-Irra proclaimed his own independence from Ur as King of Isin,¹⁰⁷ and in due course claimed dominion over the whole of Sumer 'from the camps of the banks of the Tigris and Euphrates, and from the banks of the AP.KAL Canal and the ME.Enlil Canal, from Hamazi to the sea of Magan'. This is clearly an expression describing the full length of the country like 'Dan to Beersheba' or 'Land's End to John o' Groats'. Ibbi-Sin, however, continued to reign at Ur, and in his seventeenth year in another date-formula, claims to have defeated the Martu 'who stormed in like the south wind';¹⁰⁸ but in Ibbi-Sin's twentieth year there was a famine again in Ur.¹⁰⁹ Ishbi-Irra's grip on Central Mesopotamia was complete, and soon afterwards the Empire of Ur came to an end.

The impression we receive at this period is that the northern frontier of Sumer lay along the line of the easterly bend of the Euphrates river where it runs from above Falluja towards Aqar Qûf; there it met the ME.Enlil Canal which, I conjecture, joined it to the Tigris. Was it the Jalu canal, now dried up? At some later date—probably under Hammurabi—the defences may have been pitched further north, since Hammurabi established a fortress on the Tigris called Kar-Šamaš, and another on the Euphrates at Rapiqu, usually located opposite Falluja.¹¹⁰

Sittace. The impression must not be given that the Greeks had lost their bearings in marching south-west to Sittace. What they did was for good reasons. As Ariæus pointed out to them,¹¹¹ their army could not retreat the way they came by the Euphrates' right bank, since they had eaten up what provisions were to be found there on the way. They could not ford it in the face of the enemy, to retire up the more fertile left bank. It only remained to take the route up the Tigris. But the route along the Tigris right bank was impossible for an army of any size, involving as it did crossing a desert of six days' march. Antiochus III, marching to relieve Seleucia in 220 B.C. according to Polybius,¹¹² explicitly for this reason gave up the idea of travelling by the right bank of the Tigris as impassable. Tukulti-Ninurta II did it in part in 824 B.C., travelling southwards very fast, but only by mainly following the well-watered Wadi Tharthar, and nevertheless suffered great hard-

¹⁰⁵ Jacobsen, *loc. cit.*

¹⁰⁶ Letter from Kish, quoted by Edzard, *op. cit.*, 47 and n. 208. The AP.KAL and ME.Enlil are again linked in a text of Halium (of Kiš?) a contemporary of Shumu-abum of Babylon, who dammed them. Edzard, 113.

¹⁰⁷ Letter from Puzur-Numushda, governor of Kazallu, to Ibi-Sin, apparently in his twentieth year, quoting Ishbi-Irra's proclamation, and describing the latter's forcible annexation of several cities, Nippur, Subir, Hamazi, Girkal, his pardoning of Eshnunna, Kish and Bad-zi-abba, which have defected to him, and his seizure of the 'banks of Tigris, Euphrates, NUN.ME[AP.KAL] and ME.Enlil canals'; Falkenstein, *Zeitschrift für Assyriologie* xlix 60.

¹⁰⁸ Edzard, 33.

¹⁰⁹ 'In this year in Ur they sold gold and silver and other precious objects in the temples to pay Isin'; Edzard, 47.

¹¹⁰ In his thirty-fifth year. See Meissner, *Babylonien und Assyrien* (1920-5). For the possible site of Rapiqu, see Goetze, 'An Old Babylonian Itinerary', *JCS* vii, map on p. 72.

¹¹¹ ii 2.11-12.

¹¹² Polybius v 51.6. Zeuxis warns Antiochus that if he marches from Liba along the right bank, he would, after six marches, come to the 'Royal Ditch' and would have to return if it were held by Molon and he were unable to force a crossing.

ship. The normal route by which the Assyrian armies in the eighth and seventh centuries used to descend on Babylonia was by the left bank of the Tigris, via Turnât. But for the Greeks to cross the Tigris to the left bank was possible only at Opis or Sittace, which were bridgeheads for the Ekbatana and Susa roads respectively. Certainly at Sittace, perhaps at Opis too, the river was crossed by a bridge of boats; Artaxerxes' army had withdrawn across the Tigris; if it was via Opis, he may have destroyed or dismantled a bridge behind him, if it existed, since Xenophon does not even mention it as he passed it on the left bank. In these circumstances, the invaders had no option but to make for the crossing at Sittace.

In the course of two days' further march from the Median Wall—a distance of 8 parasangs (say 30 miles)—they crossed two canals, one of them by a permanent bridge, the other by a pontoon bridge of seven boats.¹¹³ We are told that these canals were derived from the Tigris and fed a network of irrigation channels. The first of these canals was probably the Archôus (=Arahtu?), which flowed from the Tigris near Opis, and later, through Apameia.¹¹⁴

This march of two days brought them to the Tigris bank, near a place called Sittace, evidently located on an island formed by the second of these canals with the Tigris; here they camped in a large park, well wooded and stocked with game. Just beyond it was the crossing of the Tigris, effected by means of a pontoon bridge of thirty-seven boats. The Persians were said to be afraid that the Greeks might wish to seize and hold the island formed by the canal and the river Tigris.¹¹⁵

Where was Sittace? Herzfeld¹¹⁶ places it near the Islamic city of Wâsit, but this is much farther than 8 parasangs from the region of the Median Wall. According to Pliny,¹¹⁷ it lay, with Sabdata, to the east of the Tigris, opposite Antiochia, 'between' the Tigris and Tornadotus (Diyala) river. Sitacene, according to Strabo, stretched due east of Seleucia. An alternative later Greek name for Sittace was Apollonia, and for Sitacene, Apolloniatis, a district which, according to Strabo, joined with the Zagros to form the southern boundary of Media.¹¹⁸ Thus for Strabo and Pliny, Sittace lay on the east bank, and it would seem that the Tigris here is somewhat unstable. A distance of some 30 miles from Seleucia would bring us to a crossing in the neighbourhood of Aziziye, where the Tigris today describes a big loop towards the east; but a century ago, there was a second loop in the Tigris, now dried up, just south of Aziziye, such as could effectively have formed an island. In the centre of this loop lies a large double mound bearing the name of Humanîyê; it marks the site of a town of the Sassanian or early Islamic period,¹¹⁹ and its security was ensured by a wall which formerly closed off the neck of the loop, according to Lieut. Collingwood's map of 1861 (FIG. 4).¹²⁰ Near the wall was a small fort of *tetrapyrگون* type, similar to that at the ends of the Istabalat and Umm Raus walls. Near the base of the loop were to be seen remains of a huge double canal running north-west to south-east,¹²¹ parallel to the main course of the Tigris embankment. Beneath the town of Humanîyê, it would appear, the site of Sittace should be sought.

The army then crossed the Tigris by the great bridge of boats, wheeled round, and marched for four days towards the north, a distance of 20 parasangs, to Opis, 'a great

¹¹³ ii 4.9-12.

¹¹⁴ Item Apamea, cui nomen Antiochus matris suae imposuit; Tigri circumfunditur haec, dividitur Archôo (Pliny, *NH* vi 21.132). In his campaign against Bit-Yakin, Sennacherib brought Phoenician and Greek shipwrights and sailors to Nineveh and built galleys there, which he sailed down the Tigris to Opis, then dragged them on sledges (?) to the Arahtu to be refloated. There were two Apameas, Upper and Lower. The Upper is identified with Yakut's Zur-Famia (thirteenth century A.D.) and lies near Numaniya; Obermayer, *op. cit.*, 86.

¹¹⁵ ii 4.13.

¹¹⁶ *Op. cit.*

¹¹⁷ *Loc. cit.*, Inter has gentes (the Medes and Adiabenis) atque Mesenen Sittacene est, eadem Arbelitis et Palaestine dicta. Oppidum ejus Sittace Graecorum, ab ortu et Sabdata, ab occasu autem Antiochia inter duo flumina Tigrim et Tornadotum.

¹¹⁸ xi 13.6 cf. xv 3.12; xvii 17.

¹¹⁹ Lestrang, *op. cit.*, 37.

¹²⁰ Collingwood's map, *From Hillah to the Ruins of Niffer* (1861/2).

¹²¹ The Arahtu-Archôus?

city', which lay beside a great river, a *plethron* wide, crossed by a bridge.¹²² Xenophon calls this river the Physkos, but this is only a version of an Aramaic word for 'crossing', from the Aramaic-Semitic root *psh*, also found in Thapsacus. It is clear it was the Diyala.

Opis to the Zab. From Herodotus' account of Cyrus the Great's invasion of Babylonia, it is clear that Opis lay a little below the junction of the Tigris with the Gyndes, (Diyala or perhaps the Adhaim). But the Diyala mouth is hardly more than 40 miles from our presumed position for Sittace. Probably the parasangs were shortened by the delays involved in the crossing. The Greeks again may have made a big *détour*, either because they lost their way, or as is more likely, because they were treacherously misled by Tissaphernes, who was trying to gain time until the fresh army under Artaxerxes' brother should arrive to support him at Opis against the Greeks. In fact, the Persian army arrived, but was overawed, and preferred to keep the truce. From the Physkos bridge they marched northwards rapidly for six days for 30 parasangs (say 110 or 112 miles) through the deserts of the province of Media to the villages of Parysatis.¹²³

At this date, the eastward bulge of the Tigris above Baghdad did not exist, its course running more or less direct through Harba bridge, west of Kadisiyah and Ukbara, its present course being cut only in the tenth century. Now there is no mention of any crossing of the river Adhaim or other streams; as this was the season of low water, lesser streams may well have been dry, including the Adhaim, which, except during the winter rains, used to lose itself in the quicksands of the plain below Dakūk, at least till the fourteenth century.¹²⁴ The villages of Parysatis, where they found corn and sheep, nevertheless, were close to the Tigris, since from there they proceeded along its bank. The villages of Parysatis must thus have been about Dūr or Daur (the '*Dura of the shepherds*' of Talmudic sources);¹²⁵ where the character of the countryside visibly changes and permits sheep grazing. The large and prosperous city called Kainai,¹²⁶ built on the other side of the river from which the natives crossed on *keleks* bringing provisions, may have been Tekrit (the Babylonian Takritāin).¹²⁷ For four more days they marched along the Tigris bank, the alluvial plain having been left behind, for 20 parasangs (say 75 miles), bringing them to the Zapatas river, or Zab.¹²⁸ Here they rested three days, while the tragedy was prepared which resulted in the trapping and assassination of the Greek generals by Tissaphernes and their desertion by Ariacus; after a council of war in which Xenophon was elected a commander, they burnt their waggons and tents and set off in a hollow square and crossed the Zapatas, which was 4 *plethra* wide. They reached a watercourse, and after a rearguard action which took place about a mile beyond the watercourse, they got back to the Tigris and reached the city of Larissa,¹²⁹ which, it may be suggested, represents the Assyrian *al šarruti*, 'capital city'. This was a large (evidently) Assyrian city, by then deserted, with a stone pyramid 50 ft. high and walls 25 ft. broad and 100 ft. high, forming a circuit of 2 parasangs and being made of baked brick on a stone foundation, 20 ft. high. Now there is no suitable Assyrian site north of the lesser Zab: on the other hand, the description of Larissa nicely fits Nimrud (PLATE Ib), ancient Kalhu, where the citadel was built on a stone 'quay wall' which was at least 10 m. high on the west side of the mound (PLATE III), and where a *ziggurat* 70 ft. high still stands in the north-west corner, the circuit of its walls being about

¹²² ii 4.25.

¹²³ ii 4.27.

¹²⁴ Lestrangé, *op. cit.*, 92.

¹²⁵ For references see Obermayer, *op. cit.*, 142. Here the Roman army crossed the Tigris after Julian's death (Ammianus xxv 6.8). 'Da Dura nicht mehr in die eigentliche, reich kultivierte babylonische Zone fällt indem schon unterhalb Dura der reine alluvialboden Babyloniens seine Nordgrenze

gefunden hat, so dürfte in alten Zeiten, ebenso wie Gegenwärtig, die Umgebung von Dura als Weideplatz für Schafherden gedient haben' (Obermayer, *loc. cit.*).

¹²⁶ ii 4.28.

¹²⁷ For Takritāin, or Birtu, see Musil, *op. cit.*, 363.

¹²⁸ ii 5.1.

¹²⁹ iii 4.7.

4½ miles.¹³⁰ But Nimrud is about 10 miles north of the Great Zab, and one is driven to conclude that, perhaps in the excitement of the Generals' ambush and the battle, or at some later date, Xenophon's notes became mutilated or confused and the two Zabs were telescoped into, or misunderstood as one, the intervening 60 miles being omitted. I cannot suppress the suspicion that before emerging as leader Xenophon may have been under arrest (*cf.* his dream, iv 3.7), held *incommunicado* by one of the parties of quarrelling and suspicious Greeks, and that his silence here conceals the fact. Six more parasangs brought them to the vast undefended wall built of bricks on a base of stone full of shells, near the city called Mespila, evidently also abandoned:¹³¹ and the view that this was the site of Nineveh (Kouyunjik), deserted since its capture by the Medes in 612 B.C., must surely be correct.¹³² Kouyunjik is about 20 miles north of Nimrud, though the periphery of its walls is about 7½ miles, not 6 parasangs. Naturally, this figure was only hearsay. Mespila clearly reflects the Assyrian word *mušpalu*—'low', a term applied to the lower town as opposed to a citadel, or to a depression.¹³³ It seems to be preserved in that of Mawsil, the earlier form of the name of Mosul, across the river.¹³⁴

The sternest tests of all still lay ahead for the Greeks. The battles they had fought and the distances which they had covered by forced marches in Mesopotamia were remarkable enough. But any who, like the present writer, may have crossed the truly formidable passes of the Bin Göl Dağ rising to 10,000 ft. in the comfort of a modern Land Rover on a modern road from Bitlis to Erzurum in summer may perhaps gain some better idea of the almost incredible discipline, high morale and almost superhuman powers of endurance of this force, who crossed these mountains of Kurdistan and Eastern Turkey in mid-winter, neither suitably trained, equipped nor clad for the purpose, and subject to intermittent attacks from enemies fighting on their home ground.

To follow this part of their journey, however, is no longer our purpose here. We can only echo the admiration of Plutarch's Antony:¹³⁵ *φθειρομένων δὲ πολλῶν καὶ τῶν Πάρθων οὐκ ἀφισταμένων πολλάκις ἀναφθέγγασθαι τὸν Ἀντώνιον ἱστοροῦσι· ὁ δὲ ΜΥΡΤΙΟΙ θυμαίνοντα τοὺς μετὰ Ξενοφώντος, ὅτι καὶ πλείονα καταβαίνοντες ὁδὸν ἐκ τῆς Βαβυλωνίας καὶ πολλαπλασίους μαχόμενοι πολεμίοις ἀπεσώθησαν.*

R. D. BARNETT.

British Museum.

¹³⁰ Layard, *Nineveh and Babylon* (1853) 123-6 for the ziggurat. For the circuit of the walls, see Felix Jones's map, *Nimrud and Selamiyeh* (1852).

¹³¹ ii 4.10.

¹³² R. C. Thompson and R. W. Hutchinson, *A Century of Excavation at Nineveh* (1929) 138.

¹³³ Thompson and Hutchinson, *op. cit.* Herzfeld, *Encyclopaedia of Islam*, s.v. 'Mosul', doubts the identification.

¹³⁴ Lestrangé, *op. cit.*, 87.

¹³⁵ Plutarch, *Antonius* 45.

ALEXANDER'S MACEDONIAN CAVALRY

SIR WILLIAM TARN devoted some of the best pages in his *Alexander the Great* to Alexander's Macedonian troops. He has made many improvements on the account given by H. Berve.¹ Yet doubts may be voiced or amendments suggested.²

I. PRODROMOI

It is known that the Companion cavalry at Gaugamela were formed in eight squadrons or *ilai* (Arr. iii 11.8), of which one was the royal squadron *par excellence* or the 'agema of the Companions'. It is unlikely that the number of *ilai* had been reduced since 334, as reinforcements had probably more than compensated for losses (section V). Plutarch, however, says in his life of Alexander (16.2) that at the Granicus Alexander plunged into the stream with 13 *ilai*. His account of the battle closely resembles Arrian's, and he cites Aristobulus on the number of Macedonian casualties.³ The statement quoted probably then corresponds to Arrian's (i 14.6) that Alexander ordered the *prodromoi* and Paonians under Amyntas, son of Arrhabaeus, into the river, together with one foot regiment which was preceded by Socrates' cavalry squadron, and that he himself followed with the whole right wing. Plutarch's 13 squadrons may thus be identified with the 8 squadrons of the Companions plus 5 of *prodromoi* and Paonians. The Paonians themselves can be classified, perhaps by error, as *prodromoi* (Arr. iii 8.1) or distinguished from them (i 14.1 and 6; ii 9.2; iii 12.3); they could have formed only a single squadron, distinct from 4 squadrons of *prodromoi* under command of the same Amyntas (Arr. i 12.7) or *sarissophoroi* (iv 4.6) of whom we hear elsewhere.⁴ (The equation of the *prodromoi* or 'scouts' with *sarissophoroi* or 'lancers' is certain though Paonians are never described as *sarissophoroi*.)⁵

Of what nationality were these 4 squadrons of lancers? Berve held that they were Macedonian,⁶ Tarn that they were Thracian. Berve was, I think, right. His view is favoured by the fact that they are often mentioned together with the Companions, and that the ethnic origin is stated in neither case (Arr. i 12.7; 14.1 and 6; ii 9.2; iii 12.3; 18.2;

¹ W. W. Tarn, *Alexander the Great* (Cambridge, 1948) ii 135 ff.; H. Berve, *Das Alexanderreich* (München, 1926) i 103 ff. All references to these authors, unless otherwise stated, are to pages of these volumes.

² I am indebted for critical and constructive comments on an earlier version of this paper to E. Badian and G. T. Griffith, who need not of course be supposed to agree with all that it now contains.

³ Plutarch 16 agrees with Arrian (a) that Alexander crossed the river in the face of Persian opposition, against Parmenio's advice (*contra* Diod. xvii 19); (b) about the combat between Alexander and the Persians, Rhoeasces and Spithridates (*contra* Diod. 20; Curt. viii 1.20); (c) in mentioning the attack on the Greek mercenaries (whose presence Diodorus ignores); he rounds off their casualties at 20,000, whereas Arrian i 14.4 puts the strength of the mercenaries at just under 20,000 and states (16.2) that almost all were killed, except for 2000 prisoners. Plutarch used Aristobulus, whom he cites as stating that on Alexander's side only 34 men were killed, of whom 9 were foot-soldiers. Now Arrian i 16.4 says that 25 *hetairoi* were killed in the first shock (none

later?), that the other cavalry lost 60 and the foot about 30. I take it that the official version of casualties gave Macedonian losses as 25 *hetairoi* and 9 foot, and that Aristobulus ignored other losses, or else that Plutarch overlooked his statement of them.

⁴ The Paonians were commanded at Issus and Gaugamela by Ariston, the other *prodromoi* by Protomachus at Issus and Aretas at Gaugamela (Arr. ii 9.2; iii 12.3 *cf.* Plut. 39.1; Curt. iv 9.24), perhaps earlier by Hegelochus (i 13.1). At the Granicus Amyntas had a general command over both, and over other units (i 14.6; *cf.* 12.7).

⁵ Berve, 129; Tarn, 157; see Arr. i 14.1 with 14.7; iii 12.3; Curt. iv 15.13. The equation is certain, but as Mr Griffith has pointed out to me, puzzling. The *sarissa* used by the infantry required the use of both hands and must have been hard for horsemen to handle; perhaps we should assume that their *sarissa*, though longer than spears of the Companions, was shorter than that of the foot.

⁶ He argued partly from Arr. ii 8.9 which is corrupt and cannot be used.

20.1; 21.2) just as the hypaspists and phalanx regiments are never described as Macedonian, whereas non-Macedonian units are carefully distinguished as Paeonians, mercenaries, Agrianians in the same contexts. Moreover, they were always, so far as we know, stationed with or near the Companions; they were on the right at the Granicus (i 14.6), Issus (ii 9.2) and Gaugamela (iii 12.3). By contrast, the only Thracian (or Odrysian) cavalry of which we hear, commanded by Agathon, son of Tyrimmas, were placed on the left at the Granicus (i 14.3), and Gaugamela (iii 12.4); they are not named at Issus, but the Thracian foot under Sitalces, with whom they were associated at Gaugamela, were on the left there too, and one can hardly believe that the cavalry at first stationed on Alexander's left consisted only of a few hundred Peloponnesians (Arr. ii 9.1). Berve adds that they, unlike the lancers, were heavily armed cavalry. I do not see how this can be proved, but it is at least clear that 'the Thracian cavalry' in Alexander's army were distinct from the lancers.⁷

After 329 the lancers disappear from our records (Arr. iv 4.6). Tarn supposed that they were sent home. But this is unlikely, even on the view that they were Thracians (cf. n. 7). If, however, they were Macedonians, they might have been incorporated in the Companion cavalry; Alexander could now rely on Orientals as light horse.⁸

One objection arises, on which Tarn lays weight. In our text of Diodorus xvii 17.4 we read of *Θραῖκες δὲ πρόδρομοι καὶ Παῖονες ἐννακόςιοι*. It is strange that Tarn who rejects much of the testimony in this chapter should pin his faith to these words. Berve suggested that *πρόδρομοι* should be transposed after *Παῖονες*, Beloch that a reference to the Macedonian lancers and their numbers has fallen out of our text.⁹

II. THE HIPPARCHIES 328-24 B.C.

The Companion cavalry were originally divided into *ilai* (Arr. i 12.7; 14.1; ii 9.3; iii 11.8, etc.). At Susa in 331 Alexander distributed reinforcements from Macedon among the Companion cavalry, and formed two *lochoi* within each *ile* under *lochagoi*; previously an *ile* had not been so divided (iii 16.11). At least one reason for this division, as will become clear later, was the fact that by 331 the number of Companion cavalry was larger than in 334; experience had doubtless shown that the *ilai* had become unwieldy as tactical units. In India we find that the cavalry was divided into hipparchies, each containing more than one *ile* (vi 21.3). The term Hipparchy is also found in earlier contexts; some, but not all, of these passages are anachronistic (*infra*).

Originally the Companion cavalry were under the command of Parmenio's son, Philotas (Diod. xvii 17.4; Arr. iii 11.8), to whom still larger groups might also be assigned (Arr. i 14.1; 19.8). His title is not recorded, but was very probably hipparch. After his execution in 330, Alexander divided the command between two 'hipparchs', Hephaestion and Black

⁷ I agree with Berve, 134 against Tarn, 158 (but cf. 160 n. 1), that the Thracian cavalry probably remained in Media under Agathon (cf. Curt. x 1.1) and were not sent home in 330. Allied contingents were then sent home, but the Thracians were subjects, and Alexander actually received reinforcements from Thrace in 331 and 326 (Diod. 65.1; Curt. v 1.40-1; ix 3.21). The distinction Tarn draws (e.g. 160 n. 1) between Thracian and Odrysian cavalry in Alexander's army is invalid (cf. Arr. i 14.3 with iii 12.4).

⁸ E.g. *hippakontistai* (Arr. iii 24.1) and *hippotoxotai* (v 12.2; 16.4).

⁹ Berve, 134; K. J. Beloch, *Gr. Gesch.* iii² 2.325. It is indeed in my view not necessary to amend the

figures in Diodorus' text (cf. section IV), and Dr Badian has pointed out to me that he need not be anachronistic in making Erigyus, not Philippus, commander of the allied horse; Philippus may have been only in temporary command of them at the Granicus (*contra* Berve, ii nos. 302 and 779, with evidence). Cassander cannot indeed have been in command of the Paeonians and Thracians (cf. Berve, ii no. 414), but Badian thinks that Beloch's correction to Asander (Berve, ii no. 165) is probable 'in view of the context of Parmenio's stranglehold on senior appointments' (cf. *TAPA* xli (1960) 327-8). This, then, is at least one manuscript error, and the transposition of '*prodromoi*' suggested is not hard.

Clitus; we are expressly told that he no longer trusted any of his 'friends' to have so large a command as Philotas (Arr. iii 27.4). It is obvious that so long as there were only one or two 'hipparchs' the term 'hipparchy' was inappropriate for each of the eight units in which the Companions were still formed.

Clitus' murder at Maracanda may be placed late in the campaigning season of 328.¹⁰ References to hipparchies before this date (Arr. i 24.3; iii 29.7; iv 4.6-7; Diod. xvii 57.1) must then be regarded as inaccurate. Alexander apparently appointed no successor to Clitus. Tarn supposes (p. 161) that he himself took over Clitus' old hipparchy, i.e. command of 4 *ilai*. He writes (p. 163) that (in summer 327) when 'he divided his army in the Paropamisadae, sending Hephaestion and Perdiccas by the direct route to the Indus, Hephaestion had half the Companions (iv 22.7), that is, his own four squadrons; Alexander had with himself all the Companions that remained (iv 23.1), that is, the other four squadrons, including the *agema*; this shows, as I mentioned before, that he had himself taken command of the hipparchy (four squadrons) of Cleitus "the Black." This seems dubious. The arrangement imagined by Tarn was hardly convenient. And Arrian does not speak (iv 22.7) of Hephaestion alone having half the Companions; he appears to think of Hephaestion and Perdiccas having joint command of the whole detachment, including half the Companions. Tarn himself points out that in 328-7 (after Clitus' death) Coenus (iv 17.3) and Craterus (iv 22.1) were given independent commands of detachments which included Companions. Perdiccas, Coenus and Craterus are all among the officers later known as commanders of hipparchies. In his account of operations in 327 Arrian designates the cavalry units as hipparchies and implies that they numbered 8 (iv 24.1 with 22.7 and 23.1). Tarn supposed that 'hipparchies' are once again anachronistically substituted for *ilai*, and that the hipparchies were first constituted on the Indus in 326;¹¹ that they included Orientals (a matter to be discussed later), and that they numbered only 5, each composed of 1000 men and divided into 2 *ilai* (vi 21.3); to these 5 hipparchies we must add the royal *agema*, hitherto known as the royal *ile par excellence*.

Now no complete list of hipparchies exists from the period of the Indian campaigns (just as no complete list exists of the *ilai* before 328). There is, however, evidence that Alexander had 8 hipparchies in India. At the confluence of the Hydaspes and Acesines Alexander divided his army into four parts, under Craterus (who had apparently no Companions), Hephaestion, Ptolemy and himself; they were to reunite at the junction of the Acesines and Hydraotes (Arr. vi 5.5-7). Alexander himself took with him 'half the Companion cavalry' for the Mallian campaign (6.1). This must have included the *agema*, but in addition the hipparchies of Perdiccas and White Clitus were with him (6.4), and were detached for independent operations. When Alexander reached the Hydraotes, he was able to use two hipparchies against the Mallians (7.2) and one of these was apparently that commanded by Demetrius (8.2). Even if we suppose that by this time Perdiccas had already rejoined him—he took part shortly afterwards in the attack on the Mallian capital (9.1)—it seems clear that 'half the Companion cavalry' comprised 3 hipparchies, those of Perdiccas, White Clitus and Demetrius, plus the *agema*. This conclusion cannot be escaped by supposing that either Ptolemy or Hephaestion, who must have had the rest of the Companion horse, had yet joined Alexander. Ptolemy was still fighting elsewhere at the time of the capture of the Mallian capital (11.8), and Hephaestion was apparently awaiting Alexander at the appointed *rendezvous* (13.1).

There is further evidence that apart from the *agema* (Arr. v 21.5; 22.6) there were

¹⁰ Curt. viii 1.19; 2.14 places here the appointments recorded by Arr. iv 17.3 and interposes between them the death of Clitus; Arr. iv 8 gives no clear date.

¹¹ Berve, 107 ff., put the reorganisation in 329 on

the strength of a mistranslation of Arr. iii 30.6 (cf. Tarn, 163 n. 4); and thought that the hipparchies numbered 4. For the *agema* as a hipparchy cf. Tarn, 164 n. 1.

more than 5 hipparchies; we have the names of 6 hipparchs, and it is easy to assume that the name of the seventh is lost in our records. These 6 are the following:

Hephaestion	Arr. v 12.2; 21.5
Perdiccas	v 12.2; 22.6; vi 6.4
Demetrius	iv 27.5; v 12.2; 16.3; 21.5; vi 8.2
Craterus	v 11.3
White Clitus	v 22.6; vi 6.4
Coenus	v 16.3

Tarn holds (p. 165) that Coenus and Clitus were successive commanders of the same hipparchy, that Coenus was left behind at the Acesines, and never went farther, and that White Clitus then took over his squadron. This thesis is improbable. Coenus was left at the Acesines to superintend the crossing of the rearguard, who were convoying grain and other supplies to Alexander (v 21.4). But it is expressly said that his regiment was with him. Tarn, like Berve, supposes the *taxis* here mentioned was Coenus' phalanx battalion. Like Craterus and White Clitus, Coenus had originally been commander of such a battalion, for which *taxis* is the normal term. The battalions of Coenus and Clitus still bore their names at the battle of the Hydaspes (Arr. v 12.2), although Coenus, if not Clitus, was then in fact commanding a hipparchy (v 16.3). However, Tarn himself has shown (p. 136) that *taxis* can be loosely used of any 'formation', and in the plural of units of the Companion cavalry (i 15.4; vii 8.2). It seems to me to strain belief to suppose that Coenus alternated between commanding a hipparchy and his old battalion of foot.¹² Moreover, at the Acesines he naturally needed his cavalry for foraging (v 21.4). It is certainly untrue that Coenus never went farther. According to Arrian, it was Coenus who opposed Alexander's wish to go beyond the Hyphasis (v 27) and he died on the Hydaspes, just before Alexander embarked on that river (vi 2.1). Arrian's statements come from his best sources, not from the so-called Vulgate; there is no warrant for disbelieving them. (It is immaterial that Coenus' speech may be invention (*cf.* Tarn, 290), perhaps due to Arrian himself.) Arrian has very little interest in reporting fully the operations conducted by Alexander's subordinates, and it is not then surprising that he has failed to mention that Coenus came on from the Acesines and joined Alexander, just as Porus did, whom Alexander had sent off home, to muster more troops and elephants at the same time that he left Coenus behind (*cf.* v 21.2; 24.4).

It is no doubt disconcerting that at the Hydaspes only 6 hipparchies (including the *agema*) are mentioned, and that in 324 we hear of the addition of a *fifth* hipparchy to the other cavalry units. But even if my view that Coenus and White Clitus were simultaneously and not successively in command of a hipparchy be rejected, it is quite clear that Alexander had 8 hipparchies (counting the *agema*) in India, whatever change may have occurred by 324, and there is no good reason to think that Arrian is anachronistic when he writes of 8 hipparchies in his account of operations in 327. It seems probable then that the division of the cavalry into 8 hipparchies goes back to 328, when the death of Black Clitus necessitated at least some new assignments of commands.

III. POSSIBLE REASONS FOR THE CHANGE TO HIPPARCHIES

There is on the whole a marked contrast between the distinction of the former ilarchs and the later commanders of hipparchies. Of the eight ilarchs named at the battle of Gaugamela (Arr. iii 11.8) only Black Clitus, who commanded the *agema* itself, Hegelochus

¹² F. Schachermeyr, *Alexander der Grosse* (1949) 293. *cf.* n. 192, still follows Berve in holding that former commanders of foot-battalions who became hipparchs also retained their previous commands; I believe

Tarn refuted this (142 ff.), but it is to be regretted that Schachermeyr has not yet fulfilled his promise to develop his views on Alexander's army.

(who had formerly been one of Alexander's admirals) and Demetrius, who was himself one of the later hipparchs, were persons of note. Though the *ilarchs* were admitted to councils of war (Arr. ii 7.3; 10.2; 16.8; iii 9.3), they were mostly nonentities. The commanders of the hipparchies, on the other hand, included some of the most powerful men in Alexander's entourage—Hephaestion, Perdikkas, Craterus, Coenus; White Clitus had a distinguished career after Alexander's death, and only Demetrius, the one *ilarch* who is known to have retained his command of a squadron under the new arrangement, is otherwise unimportant. It seems to me probable that this radical change in the nature of the command of the Companion cavalry, which goes far beyond and in fact underlies the change of name from *ile* to hipparchy, should be explained partly by Alexander's growing distrust of his principal officers. He apparently felt that in dividing the command between two hipparchs he had not gone far enough. Though he repented of the murder of Black Clitus, it cannot have escaped him even in his remorse that the sentiments to which Clitus is reported to have given vent indicated a degree of discontent in an officer of seemingly proved loyalty which might be shared by others of impeccable antecedents to an extent that Alexander could not measure with any certainty. It was then rather dangerous to entrust even half the Companion cavalry to any one Macedonian noble. Better to divide the command still further among officers whose mutual animosities would restrain them from treasonable combination. But in the new division it was natural for Alexander to confer on the new high ranking cavalry commanders the more exalted title of hipparch, which previously had been held only by one or two men. This was also a sop for Hephaestion, who could not be degraded in dignity. Hephaestion, his personal favourite, was indeed perhaps the one man in his court in whom he could put full confidence. Yet it was an inevitable consequence of the new arrangement—this is true, whenever it occurred—that Hephaestion should be demoted from command of half the Companions to command of one-eighth (or, even on Tarn's view, one-sixth). To Hephaestion, however, the reasons that prompted the change could be secretly and candidly explained; and he would also preserve his special role as the king's confidential adviser.

This political explanation of the change does not exclude the possibility that the new hipparchies differed from the old *ilai*. Before Clitus' murder the hipparchy had been a large division of cavalry, comprising *ilai*, and in the new organisation, it was still divided into *ilai* (Arr. vi 21.3). Mr G. T. Griffith writes to me: 'I doubt if it is military practice in general to change or switch old-established names like *lochos*, *ile* (names universal in Greek practice and not only in Macedonian). And I think an entirely new name (hipparchy) ought to predispose us to expecting an entirely new thing, unless and until the evidence contradicts this idea.' These considerations certainly militate against the view to which I originally inclined, that hipparchy was simply a new name for *ile*, and that *ile* in turn came to be used instead of *lochos*, so that whereas the *ilai* had formerly been divided into *lochoi* (Arr. iii 16.11), the hipparchies were similarly divided into *ilai* (vi 21.3). It seems that in fact the hipparchies were larger units than the *ilai* had originally been.

Both Berve and Tarn held that in the Indian campaigns Orientals had already been incorporated in the hipparchies. This possibility will be discussed in section VIII. But it is not necessary to accept it, in order to account for the hipparchies being larger than the old *ilai*. Consideration of the number of Companions with which Alexander had started (section IV) and of reinforcements and losses (section V) will show that originally he had at least 1800 Companions and that he had received 1300 more by 331 and probably an additional 500 in 328–7, about the time when the hipparchies were constituted. Moreover by 328 the 4 squadrons of Macedonian *prodromoi* had probably been incorporated in the Companion cavalry (section I). We have no good evidence for the strength either of an *ile* in 334 or of the later hipparchies, except what may be inferred from these figures

(see section VI). We cannot be certain that the figure of 1800 Macedonian cavalry includes or excludes any who had been sent to Asia in advance by Philip. Diodorus certainly applies it only to Companions, not to *prodromoi* as well.¹³ If it comprises only the 8 *ilai* of Companions, then the average strength of an *ile* of Companions exceeded 200, and we might allow some 800 more for the *prodromoi*. There may also have been some Companions or some *prodromoi* or both in the advance force, not included in this figure. Or again if Diodorus is in error, the figure may be complete for all the Macedonian cavalry Alexander had in 334. By 328 his reinforcements had brought his numbers up to about 3600 (or even perhaps to 4400, if the *prodromoi* were not among the 1800), less men lost or detached for garrisons and colonies. The numbers of these cannot be accurately estimated, but they will hardly have been so great that the 8 hipparchies of the Indian campaigns were not much stronger than the 8 *ilai* of Companions in 334, or even than the 12 *ilai* of Companions and *prodromoi* taken together. One or two hipparchies are now used, together with other troops, for operations independent of the main army, and with such purposes in mind it was natural for Alexander to decide that he needed to form his cavalry in larger units; his reinforcements and the incorporation of the *prodromoi* in the Companions made this possible. Thus the *ile*, though now a subdivision of the hipparchy, may have remained (until further casualties were incurred) not very different in size from what it had been in the past. This suggestion must now be justified by an examination of the evidence on the numbers of Alexander's Macedonian cavalry.

IV. THE NUMBER OF ALEXANDER'S MACEDONIAN CAVALRY IN 334

Diodorus (xvii 17) alone professes to give us the strength of the individual units in the army which crossed the Hellespont with Alexander. According to Tarn (p. 156) he drew his data from the 'mercenaries' source', and this writer could have known the truth only about the Greek contingents. In my view the hypothesis that such a 'mercenaries' source' ever existed is totally without foundation;¹⁴ but even if it did exist, it is not easy to see why such a writer should have found it hard or impossible to ascertain the numbers of Balkan or Macedonian contingents, if he could discover the truth about Greek units. It is more usually held that Diodorus relied for his whole account of Alexander on Clitarchus; the case for this has been admirably restated by Lionel Pearson. Clitarchus has generally been taken to be a contemporary: I agree with Pearson in believing that he wrote after 280.¹⁵ In that event he must himself have drawn on earlier authorities. Whether or not he ranks as a primary or secondary source, it is agreed that his story of Alexander was often defaced by inventions for romantic or dramatic effect. He could, however, have had no motive for such distortions of the truth in merely listing the numbers of men in the various contingents of the army. It is probable that he simply repeated what he was told, or what he read in some earlier book. It does not follow that Diodorus is altogether accurate. There are incongruities in the text of this chapter which show that some errors or confusions are concealed in it. These errors may be ascribed to the carelessness of Clitarchus (or whoever Diodorus' source may have been), of Diodorus himself, or of copyists. We should also reckon with a tendency to minimise the strength of Alexander's army. The smaller the force with which he was credited, the greater his glory in defeating vast armies of the enemy. It is clear that Persian numbers were exaggerated and Alexander's casualties under-estimated for this purpose, even by the authorities followed by Arrian.

Before examining Diodorus' statements about the number of Macedonian cavalry in

¹³ Cf. Section IV.

¹⁴ *CQ* (1962) 143 ff.

¹⁵ *The Lost Histories of Alexander the Great* (Amer.

Philol. Association, 1960) ch. viii. Tarn reached the same conclusion on Clitarchus' date, but for the wrong reasons.

Alexander's army, it will be best to consider what he has to say about the total strength of the foot and horse, since this can be collated with similar statements by the primary authorities,¹⁶ and his reliability can thereby be tested. (See Tables at the end.) Diodorus estimates the number of the foot at 30,000 and of the horse at 4500. But the sum of his totals for the individual contingents amount to 32,000 and 5100 respectively. It is tempting to amend the figures to produce a concordance, to say that in the total for infantry given by our manuscripts the words *δισχίλιοι καὶ* have dropped out before *τριακμύριοι*. But the transmitted figures for the cavalry cannot be reconciled with palaeographic plausibility by amending the total as given by the manuscripts. Here we should have to suppose that any manuscript error lies in one or more of the figures for individual contingents. But emendation may not be justified in either case. The number 30,000 for the foot, which re-appears elsewhere, may simply be a rounding down of 32,000; Justin (who very probably drew ultimately on the same source) also gives that figure, and Arrian, who doubtless relied on Ptolemy, says that Alexander had 'not much over 30,000'. Again, the figure of 4500 for the horse was given not only by Justin but also by Callisthenes—it is presumably rounded down to 4000 by Aristobulus—while the figure of 5100 corresponds to Arrian's, i.e. Ptolemy's, estimate of 'over 5000'. It would seem then that both the transmitted and implied totals in the text of Diodorus agree with good authorities, viz. Callisthenes and Ptolemy respectively, but that Callisthenes omitted some contingent from his computation, which Ptolemy and Diodorus' source included; it can indeed further be conjectured that the latter carelessly copied Callisthenes' total, and then added, perhaps in another place, a detailed and discordant account of the several contingents and their numbers.

This hypothesis does not indeed end the difficulties. Anaximenes, who was also a contemporary and should have been well informed, put the number of the cavalry at 5500 and of the foot at 43,000. Callisthenes' figure for the foot was 40,000, probably rounded down from the same figure as that given by Anaximenes. It may seem hard to suppose that the transmitted total for cavalry in our text of Diodorus comes ultimately from Callisthenes, if both the totals, transmitted and implied, for the infantry are so much lower than that Callisthenes gave. We must also ask which of these estimates is dependable, and how the discrepancies could have arisen.

It is not easy to believe that either Anaximenes or Callisthenes, as contemporary writers, of whom the latter at least was the official historian of the campaign, could have been altogether mistaken. As I have already pointed out, we might expect the Alexander-historians to minimise rather than to over-state the size of Alexander's army; and this consideration might lead us to accept the highest totals, as given by Anaximenes, viz., 43,000 foot and 5500 horse, or to suspect even these of being too low. For such a view there is some confirmation in the estimate given by Arrian (iii 12.5), and doubtless derived from Ptolemy, of the size of Alexander's army at Gaugamela—40,000 foot and 7000 horse.¹⁷ The total recorded reinforcements between the beginning of the invasion and this battle amount to 12,700 foot and 1650 horse.¹⁸ Against these nearly 8000 men (mostly infantry) are said to have been left as garrisons, while losses, recorded only for the Granicus, Issus and Tyre, are given as under 1000.¹⁹ We may doubt if even reinforcements have been fully recorded, or alternatively if even Anaximenes' estimate of the original army is not too low. The Ptolemaic (?) figure for the cavalry at Gaugamela is indeed just reconcilable

¹⁶ See Callisthenes (Jacoby, no. 124) F. 35 = Polyb. xii 19.1; Plut. 327DE for Anaximenes (Jacoby, no. 72) F.29 (cf. Plut., *Alex.* 15.1), Aristobulus (Jacoby, no. 139) F.4 and Ptolemy (Jacoby, no. 138) F.4. Cf. Arr. i 11.3 (Ptolemy?); Justin xi 6.2 (Clitarchus?); Liv. ix 19.5; Frontin., *Strat.* iv 2.4.

¹⁷ Cf. Tarn, 159: 'probably . . . the official figure, but in any case it is not likely to be too high'. *Contra* Beloch, *op. cit.* (n. 9) 33-5.

¹⁸ See next section and table in Berve, 179.

¹⁹ Berve, 179.

with the total he gives and with that of the attested cavalry reinforcements ($5500 + 1650 = 7150$), on the assumption that few of the cavalry had been lost or left behind in garrisons. It is obvious, however, that the tally of losses and garrisons is quite inaccurate or incomplete. Admitted casualties are absurdly low, and were almost certainly exceeded by deaths from disease. Most of the satraps are allowed no troops to govern their provinces, yet we know that they later had fighting to do,²⁰ and even if this were not so, we could not believe that they were not provided with any military forces. They could, and did, recruit mercenaries for themselves, but it would not have been prudent in Alexander to have relied on their raising sufficient men in this way as promptly as they were needed. It is then inherently probable, unless the scale of reinforcements was much greater than we know, that the Grand Army was larger in 334 than in 331, though on the likely assumption that the garrisons were mainly or exclusively drawn from other contingents, it may have included more Macedonians (as a result of new drafts) by 331.

The figures given both by Diodorus and Ptolemy relate expressly to the forces which crossed into Asia with Alexander. But we know that Philip had sent an advance expeditionary force in 336, whose strength Polyænus estimates at 10,000 (v 44.4), obviously a round figure. It has been held that this force was withdrawn in 335, but there is no evidence for this hypothesis.²¹ Of its commanders, Attalus had been murdered, and Parmenio may have been back in Macedon in winter 335-4, and certainly seems to have been with Alexander when he crossed the Hellespont.²² But that does not prove that the army had been withdrawn. The third general, Amyntas, may have remained in Asia in command of it, and Calas is also attested as a general there in 335.²³ It has been urged that Arrian had nothing to say of Alexander taking over a rather large army in Asia. But this argument from silence has little weight, certainly no more than the fact that withdrawal is not recorded: Arrian altogether neglects the earlier operations in Asia, apart from one or two allusions, and he is very incomplete in his account of Alexander's later reinforcements. (It is rash to assume that Ptolemy omitted what Arrian ignored.) Moreover, it is well known that between 334 and 331 Alexander disposed of more mercenaries than can be explained by adding the figures for mercenary reinforcements to the total recorded in Diodorus' manuscripts for the original mercenary contingent.²⁴ Instead of amending Diodorus' text to account for this, it would be better to explain it by recalling that the advance expeditionary force was composed in part, and perhaps in large part, of mercenaries (Diod. xvii 7.10). It is, after all, in the highest degree improbable that this force should have been withdrawn on the very eve of the invasion, when it was available to protect the bridgehead. Finally, the discrepancies in our totals for Alexander's numbers can be best explained, if we assume that some exclude and others include the troops already in Asia. The hypothesis also permits us, in some degree, to save the credit of all the authorities whom we should expect to have been well informed.

We can accept the figures of Ptolemy and Diodorus for the army that actually crossed the Hellespont with Alexander, viz., 32,000 foot and 5100 horse. Callisthenes' figures are harder to interpret. It is important to recall that we do not possess his own words, nor do we know in what context he gave his estimates, viz., 40,000 foot and 4500 horse. Polybius states that he credited Alexander with commanding this army 'for the crossing into Asia'. It is possible that he gave first a detailed army-list for the time of Alexander's mobilisation, and, later, another total for the army strength at or after the crossing. In

²⁰ Curt. iv 1.34-5; 5.13; Memnon (Jacoby, no. 434) F.1, ch. 12.4; Diod. xviii 22.4.

²¹ E.g. by Beloch, *op. cit.* (n. 9) 323; see *contra* J. G. Droysen, *Hermes* xii (1877) 227 ff.; G. T. Griffith, *Mercenaries of the Hellenistic World* (Cambridge, 1935) ch. i.

²² Diod. xvii 16.2; 17.3; Arr. i 11.6.

²³ Justin xi 5.8 names Amyntas, Diod. xvi 91.2; xvii 2.4 ff.; 5.1-2 only Parmenio and Attalus, xvii 7.10 Calas.

²⁴ Griffith *loc. cit.* (n. 21). It is not certain that the 200 horse left at Halicarnassus (Arr. i 23.6) were mercenaries.

the first place he may have missed a contingent of 600 cavalry (conceivably the Thracians), perhaps because they joined Alexander *en route*, between Pella and the Hellespont, or from sheer carelessness, and in the second place he may have happened to give only the total of the foot, including the troops already in Asia, because they were available 'for the crossing'; they were holding the bridgehead. At any rate it is far more likely that he omitted some contingent than that he unduly magnified Alexander's strength. In fact he probably rounded down the true figure for infantry of 43,000, given by Anaximenes. I feel little doubt that Anaximenes' figure for the foot is correct, once the infantry in the advance force has been added to the 32,000 men whom Alexander brought with him. This roughly agrees with Polyaeus' estimate of that force, in round numbers, at 10,000 men.

Is Anaximenes' figure of 5500 for the cavalry acceptable? If Alexander crossed himself with 5100 (Ptolemy), and if Anaximenes sought to give the total of all his forces after his crossing, the advance force included only 400 cavalry. Yet it was well known that the enemy strength lay in its cavalry; so small a contingent would have been inadequate, and bears no relation to the resources in cavalry at Philip's disposal. Some cavalry in the advance force were doubtless mercenaries; no mercenary cavalry appear in Diodorus' army list, and only a few are recorded to have reached Alexander before Gaugamela, when he had apparently a considerable number under his command (see n. 24). But it seems unlikely that Philip spared no *Macedonian* horse for the Asian campaign of 336, and Diodorus states that in 335 there were Macedonians in Asia, whether foot or horse is not mentioned (xvii 7.10). Now Diodorus lists the *prodromoi* separately from the '1800 Macedonian cavalry', and if the Macedonian *prodromoi* are in fact included in his figure of 900, which embraces explicitly only Thracian and Paenian horse, the total is obviously too low. If the '1800 Macedonian cavalry' are all Companions, then each of the 8 *ilai* of Companions averaged over 200 (a conclusion Tarn reached on other but fallacious grounds, cf. section VI), and we might expect the 4 *ilai* of Macedonian *prodromoi* not to be very much inferior in strength; the figure of 900 seems rather too low to comprise them as well as the Paenians and the Thracians, who probably numbered 600 (the contingent Callisthenes probably overlooked). It may then be that some *prodromoi* were already in Asia, and it is perhaps significant that at the Granicus they were under the command of Amyntas, one of the generals of the advance expedition (n. 4). These arguments are indeed plainly indecisive. It is possible that Diodorus, who does not specifically name Companions, includes the Macedonian *prodromoi* among the '1800 Macedonian cavalry', and that his figure of 900 relates to 600 Thracian horse and 300 Paenian *prodromoi* (cf. section I for the probable correction of the text). On the other hand, it makes better sense if the advance expedition included 1000 rather than 400 horse; and it might be that Anaximenes also neglected a contingent of 600 in Alexander's own army and reached his total of 5500 by adding 1000 horse already in Asia to the incorrect figure of 4500 for the cavalry whom Alexander brought over with him. On this view the true total was 6100.

It cannot be claimed that this complex discussion has attained certain results. But it ought to be clear that the common practice of taking Ptolemy's figures as relating to the whole army which Alexander had with him *after* he had crossed into Asia is unjustified. For our present purpose it is important that Diodorus' details of the contingents mobilised *before* the crossing may in general be correct, as his totals harmonise with those given by Ptolemy.

Now Diodorus says that Alexander took with him 1800 Macedonian cavalry and left 1500 with Antipater in Macedon. He also took 12,000 Macedonian foot and left 12,000 to Antipater. Tarn (p. 156) believes that the figure for Alexander's foot happens to be right, but that that for Antipater's 'is merely made up to balance Alexander's figure'. The second assertion is unjustified by Tarn's arguments.²⁵ If it were right, why not 1800

²⁵ Tarn argues (i) that Alexander could not have given Antipater more than half the phalanx; (ii) that

Antipater could not have had any hypaspists, of whom Alexander had 3000; therefore his foot could

horse for Antipater? Tarn says that the 1800 cavalry assigned to Alexander simply corresponds to the 1800 Thessalian cavalry; this is arbitrary and still leaves unexplained the 1200 assigned to Antipater. He in fact rejects this number (p. 155), remarking that 'when the Lamian war broke out Antipater could only muster 600 horse, and it is not known what they were'. In fact Diodorus (xviii 12.2) says expressly that they were Macedonians, and makes it clear that they were a hasty levy; that Antipater advanced into Greece at once, while leaving Sippas with other forces to protect the homeland and to muster more troops. Moreover, though he asserts that Macedon had been drained of soldiers by the large drafts Alexander had made on Macedonian manpower—and this was true enough and justified, as will be seen, by the reinforcements Alexander had received since 334—we find that in the following spring Leonnatus levied 1500 more cavalry, who are described as Macedonian (xviii 14). Beloch indeed said that many of these were mercenaries, Illyrians and Thracians,²⁶ but this is quite unattested, and Thracian recruiting, in particular, seems unlikely at a time when Lysimachus was repressing a Thracian revolt (*ibid.* 14.2). After the arrival of Craterus with 10,000 foot and 1500 horse (16.4), the combined Macedonian strength is said to have been over 40,000 heavy foot and 5000 horse; thus there had been still further levies. It must be pointed out that here Diodorus is drawing, by common consent, on a good source, Hieronymus of Cardia.

In 334 Alexander could not have ventured to leave Macedon without a large force available to beat off barbarian invasions from the north and to keep the mostly discontented Greeks under control. Moreover, in 331 Antipater could muster not less than 40,000 men against Agis, and he doubtless was superior in cavalry to Agis' 2000 horse (Diod. xvii 62.7–63.1); yet in the interim he had sent 1300 more cavalry to join the Grand Army. We are not told how many of his men in 331 were Macedonian, but it is natural to think that they provided most of his cavalry; of the Greek allies only the Thessalians were strong in this army, and the bulk of their horse were with Alexander, 1800 from the first, if Diodorus is correct, and 200 more from 333 (Arr. i 29.4).²⁷

Tarn's rejection of Diodorus' evidence on Macedonian cavalry in 334 is thus based on a serious under-assessment of Macedonian manpower. Diodorus' figures are credible, and there is no particular ground for disbelieving them; they fit in with Ptolemy's reliable total. Only we must doubt whether there were not other Macedonians, not included in his figure of 1800, already present in Asia before Alexander started (*supra*).

V. REINFORCEMENTS AND LOSSES

Arrian (i 29.4) says that Alexander was joined at Gordium by 300 additional Macedonian horse. But Callisthenes, according to Polybius (xii 19.2), said that 800 more reached him before he entered Cilicia. His evidence is often ignored, but without justification. A historian writing in the camp for Alexander's own eyes could hardly have

not have been equal in numbers to Alexander's. But (i) is assumed, and falsified by the evidence on Alexander's reinforcements and the reserves of Macedonian manpower. The common assumption that there were 12 territorial regiments of which Alexander had 6 is mere conjecture; in any case the strength of these regiments must have varied as reinforcements were received, and it is ludicrous to suppose that in 334 or later Alexander completely denuded of young men the territories from which his regiments were drawn.

²⁶ *Bevölkerung der gr.-röm. Welt* (1886) 209. His estimates of Macedonian population, somewhat greater in *Griech. Gesch.* iii².1 294 ff., do not take

adequate account of the levies in and just after Alexander's reign, described in the text.

²⁷ Isocr. viii 118 estimated Thessalian cavalry at 3000. In the Lamian war, a life and death struggle fought in their own country, the Thessalians, most of whom were involved, put 2000 into the field (Diod. xviii 15). Jason allegedly claimed that Thessaly could provide 6000 (Xen., *Hell.* vi 1.8) and is said to have raised over 8000 from Thessalians and allies (*ibid.* 19), but these figures must surely be exaggerated, or include mercenaries (cf. *ibid.* 5); and in other fourth-century data we cannot distinguish Thessalian horse from that of their allies or mercenaries.

been misinformed on this point, and he had no reason for mendacity. No one pretends that Arrian catalogued all reinforcements. I take it as certain that after the arrival of 300 horse at Gordium 500 more arrived before Alexander entered Cilicia.²⁸

Arrian (iii 16.10) notes the arrival of more Macedonian troops at Susa in late 331. He does not say how many, but both Curtius (v 1.40) and Diodorus (xvii 65.1) state that they included 6000 Macedonian foot and 500 Macedonian horse. Though they have fixed the place at which these troops arrived as Babylon, not Susa, there seems to be no reason for distrusting their testimony on numbers. It therefore follows that by late 331 Alexander had taken at least 3100 (1800 + 800 + 500) cavalry from Macedon, and probably more.

This was not all. Tarn has, I think, shown (p. 147) that probably in 328/7 a seventh infantry regiment was formed from new Macedonian troops, which Sopolis, Epocillus and Menidas were sent from Nautaca to bring to him (Arr. iv 18.3). Tarn does not allow for a like reinforcement of the cavalry, but it seems intrinsically probable. If more foot-soldiers were needed and could be obtained, why not more horsemen too? More Macedonians were available, as new age-groups grew to maturity. This explains why, as early as 331, 6000 Macedonian foot and 500 horse could be sent out, in the very year of Agis' rising,²⁹ despite the fact that Alexander had already received 5000 foot and 800 horse as reinforcements in 333 (Callisthenes). The new recruits of 331 were doubtless mainly men born in c. 350-49, and those of 328/7 men born in c. 349-6. Philip's successes in expanding Macedon, settling his people in more productive lands and giving the country more security against the northern and eastern barbarians were now bearing fruit in an amazing increase in population.³⁰ It would not be surprising if the draft that reached Alexander in 328/7 included 500 cavalry, or more; there was now less danger in Greece than in 331.

Alexander naturally received no further Macedonian reinforcements till after his return from India. In the interim his losses must have been very heavy. Casualties are hardly ever given, and are only admitted to have been serious on an occasion when Alexander himself was not present (Arr. iv 6.2 with 3.7). In fact it may well be that relatively few men were lost in the guerilla fighting in Iran and India; the battle at the Hydaspes is another matter. But disease must have taken a heavy toll, aggravated by Alexander's forced marches and persistent disregard of the difficulties of terrain and climate.³¹ In particular, H. Strasburger has proved from the ancient evidence and from modern descriptions of the conditions that the losses in Gedrosia must have been enormous.³² Many provinces too required garrisons in which some Macedonians may have been enrolled; it is at least certain that Macedonians unfit for active service were sometimes settled in the

any case no military expertise was needed to state numbers correctly.

²⁸ Amyntas (Berve, ii no. 57) was sent for them in autumn 332, after the capture of Gaza (Diod. xvii 49.1; Curt. iv 6.30); as they reached Susa by late 331 (*supra*), they must have been sent off long before Agis' defeat in October 331.

²⁹ Cf. n. 26. Berve's *a priori* argument (p. 117) that Alexander did not want reinforcements of politically unreliable Macedonians cannot stand.

³¹ See Arr. iii 20-1; 28.1; 28.8 (cf. Str. xv 2.10; Curt. vii 4. 22 ff.); iv 21 and 23 ff.; Str. xv 1.17 and 27 with Arr. v 27.5-6.

³² *Hermes* lxxx (1952) 456 ff. Ptolemy of course concealed the extent of the disaster and extenuated the motives for the march; Tarn follows him uncritically. Strasburger guesses that Alexander may have lost some 50,000 men in Gedrosia but concedes that the Macedonians may have come off with least loss.

²⁸ H. Droysen's suggestion, *Untersuchungen über Al. d. Gr. Heerwesen u. Kriegführung* (1885) 39, that he included the *neogamoi* presupposes gross carelessness by Callisthenes or Polybius or both. Mr Griffith points out to me that Polybius is unlikely to have misreported Callisthenes here, when he is picking holes in his account. He suggests that Alexander was now able to increase the size of his force, because *phoros* was now coming in, and would increase with further conquests; Macedonian revenue was inadequate to support the large expeditionary force of 334. And in 334 Alexander did not expect to meet the Persian Grand Army and so had at first less need of men. Tarn, 158 n. 1, dismissed Callisthenes' evidence on the ground that he was no 'authority on military matters'; but ancient and modern criticisms of Callisthenes as a military historian seem to me largely misconceived, and in

new colonies.³³ In 325, before the Gedrosian march, the Companion cavalry still numbered over 1700.³⁴ But many must have been lost in Gedrosia, even though Craterus was sent back to Carmania through Arachosia with the 'unfit' Companions (Arr. vi 17.3), and an unknown proportion were thus spared the horrors of the desert march.

At Opis Alexander sent home all old and unfit Macedonians under Craterus (Arr. vii 8.1; 12.1-4).^{34a} Arrian estimates the number at about 10,000. Diodorus says that there were rather more than 10,000 Macedonians (xviii 12.1; cf. xvii 109.1), but elsewhere that the 10,000 were all foot, including 6000 who had been through all Alexander's campaigns and 4000 who had joined him *en route* (16.4).³⁵ If the number 10,000 relates only to foot, it implies that Alexander now demobilised about one-third of all the Macedonians who had served him in the phalanx:³⁶ many others were of course dead.

It is obvious that veterans must have been discharged from the cavalry as well as from the foot, and Diodorus also mentions 1500 horse with Craterus (16.4); though he does not expressly call them Macedonians, this may reasonably be assumed. Alexander had originally taken with him from Macedon 1800 or more cavalry, and reinforcements numbered 1300 down to 331 and probably 500 or more in 327; the total probably exceeds 3600. If Craterus' 1500 cavalry were discharged Macedonian veterans, they constituted rather more than a third of all the Macedonian horse who had served under him. The proportion of surviving veterans discharged from the cavalry is thus rather higher than of those discharged from the foot. But this is not altogether surprising. It is not unnatural to assume that in the cavalry the ratio of survivors was higher, as they might have suffered less from the hardships of the prolonged and difficult marches. On any reasonable view of losses and of the numbers left in garrisons or colonies, Alexander could not, if these figures are right, have retained many of his surviving veterans; and Curtius makes him say that he had discharged the greater part of the army (x 2.19). The speech is fiction, but the statement doubtless corresponds with the facts as known to the historian. Yet Curtius (x 2.8) says that while discharging the older soldiers he proposed to retain 13,000 foot and 2000 horse as an army of modest size, sufficient to hold down Asia. It is obvious that these last figures, if they have any validity at all, must refer only to Macedonians, and not to the large forces of Greek and Balkan mercenaries whom Alexander had attracted into his service,³⁷ nor to the Orientals whom he was incorporating in his Macedonian regiments during 324-3; we hear, for instance, of 20,000 Persians joining his infantry in 323 (Arr. vii 23.1). On the other hand, it is impossible to believe that the survivors of Alexander's old Macedonian troops (at least 30,000 foot and 3600 horse) numbered 10,000 + 13,000 foot and 1500 (?) + 2000 horse. Curtius' numbers, if accurate, must relate either to the total Macedonian forces which he intended to have in his future army, or (assuming that Curtius had misunderstood his authority) to the number of the reinforcements demanded by Alexander in 324-3; in fact the number of Macedonians still with the Grand Army after Alexander's death requires us to accept the second alternative. Since no new drafts had reached Alexander since 328-7, and new age-groups had grown up, Macedon with its large

³³ Arr. iv 4.1, cf. perhaps 22.5; 24.7. Generally the types of settlers are not specified.

³⁴ Arr. vi 14.4 ἀναβιβάσας ἐς τὰς ναῦς τῶν μὲν ἑταίρων ἱππέας ἑπακασίους καὶ χιλιούς. I am now convinced that the partitive genitive implies that there were more Companions at the time (*contra* Tarn, 162, discussed in Section VI). Berve wrongly says (p. 110) that Arrian here designates the Companions as a part of the cavalry and then oddly goes on to infer that in a wide sense Oriental cavalry could be so described. To fit his view, Arrian should have written τῶν μὲν ἑταίρων Μακεδόνας καὶ.

^{34a} On Craterus' slow progress home see E. Badian, *JHS* lxxxi (1961) 34 f.

³⁵ I doubt if half the 12,000 foot with which Alexander crossed to Asia would have survived (but we must allow for some others with the original expeditionary force); perhaps Diodorus' source distinguished between (i) men who had fought at the main battles, Issus and Gaugamela, and (ii) later reinforcements.

³⁶ See table on p. 39.

³⁷ Some 12,000 Balkan troops and 80,000 Greeks (cf. Berve, 182, for table).

population (*cf.* n. 26) could have supplied so many men; and if in fact reinforcements were received from Macedon at this time (before or after the Opis mutiny), we can better understand Diodorus' remark that Antipater found it hard to raise troops in autumn 323 for the Lamian war, because the country had been depleted of manpower by Alexander's requirements. The new drafts doubtless arrived gradually. Not long before Alexander's death we read (*Arr.* vii 23.1) that he was joined by Philoxenus and Menander with armies from Caria and Lydia, and by Menidas with the cavalry under his command. Philoxenus and Menander were respectively satraps of Caria and Lydia,³⁸ but we are not bound to conclude that the troops they brought had been levied within their provinces, though this is possible. They may have been instructed to bring Macedonian troops to Babylon. Menidas had previously been employed in a similar task (*p.* 37). Most of the recruits must have reached Alexander before his death and perhaps even before the veterans were discharged. Immediately afterwards Perdikkas was able to give Pithon 3000 foot and 800 horse, all Macedonian, to repress the Greek revolt in Bactria (*Diod.* xviii 7.3). Of course, he must have retained considerable Macedonian forces in his own hand, for his operations in Anatolia (xviii 16-22) in 323-2, and by 322-1, when Pithon had rejoined him (36.6), he felt strong enough to try conclusions with Antipater and Craterus; half his army under Eumenes was capable of beating Craterus, who had 20,000 foot, 'mostly Macedonian', and 2000 horse (30.4). We must assume that Perdikkas had in his army a strong nucleus of Macedonians, and this is inexplicable except on the assumption that large new drafts from Macedon had reached Alexander in 324-3, or were on their way at the time of his death.³⁹

A table will best illustrate my conclusions.⁴⁰

			Macedonian Foot	Macedonian Cavalry
			?	?
Advance force (336)	12,000	1800 (or more)
Expeditionary force (334)	5,000	800
Reinforcements—333	6,000	500
331	c. 6,000 (or more)	c. 500 (or more)
328-7	? 13,000	? 2000
324-3		
			42,000 +	5600 +

No comparable table of losses can be drawn up; but even though our sources minimise casualties in the years 334-1, it seems likely that they were far exceeded by reinforcements, and improbable that many Macedonians were in this period left behind as garrisons. But after 331 the hardships of the campaigns and the settlement of unfit Macedonians in colonies must have progressively reduced the numbers of both foot and horse.

VI. THE STRENGTH OF 'ILAI' AND HIPPARCHIES

It remains to consider whether these conclusions fit what can be discovered about the strength of the individual *ilai* or hipparchies.⁴¹ Berve held that the 1800 Macedonian

³⁸ Berve, ii nos. 794, 501; the statement that the troops came 'from the sea' (*Arr.* vii 24.1) does not exclude my view.

³⁹ The statement in *Diod.* xix 41 that the 3000 *argyraspides* in Eumenes' army in 316 were veterans of all Philip's and Alexander's wars and men of 60 to 70 is (as Tarn says, *p.* 151) silly; but what doubtless underlies it is that some were veterans of some campaigns conducted by Philip or Alexander;

for instance, men who had fought at Chaeroneia (and then in Asia) need not have been more than 40 in 316.

⁴⁰ We do not know how many Macedonian horse or foot were in Asia before 334, or whether Macedonian *prodromoi* are included in Diodorus' 1800 horse, *cf. supra.*

⁴¹ Droysen, *op. cit.* (n. 21) 237 held that the *ilai* must have varied in strength; his arguments are

cavalry were divided into 12 *ilai*, 8 of Companions, 4 of lancers, and that each had a strength of 150. This involves the dubious assumption that Diodorus' figure for Macedonian cavalry includes the Macedonian *prodromoi*. Even if Berve were right, the actual strength of the units at Issus must have been increased by reinforcements which arrived earlier, and clearly exceeded any losses up to that time. If then Berve were correct in thinking that the 300 horsemen posted on Alexander's right flank at Issus were 2 *ilai* of Companions, we should have to say that Arrian has substituted for the mention of 2 *ilai* the nominal strength of 2 *ilai*, but that the actual strength must have been greater.⁴² Tarn (p. 158) has, however, in my view refuted Berve's opinion that these 300 horsemen were Companions.

Tarn supposes that the *lochoi* into which the *ilai* were divided, when augmented in 331, were more properly designated *hecatostyes* (Arr. vi 27.6; vii 24.4), and that this implies that each *ile* then consisted of two *hecatostyes* of 100 men apiece; he finds confirmation of this (pp. 161-2) in the fact that we hear of cavalry forces of 400 or 600 men, i.e. 2 or 3 *ilai* (iv 17.3; 22.1). He shows, however (pp. 162-3), that the *agema* was 300 strong. Hence he takes Arrian's figure of 1700 Companions on the Indus voyage (vi 14.4) as an anachronistic indication of the nominal strength of the Companions ($200 \times 7 + 300$) before the reorganisation into hipparchies; he thinks that in fact it relates to their numbers in Bactria.

This is clearly unconvincing. Arrian's figure not only professedly relates to a later time, but does not even purport to give the total of all Companions in 326, but only of those transported on ship down the Indus (n. 34). Moreover, Tarn has, of course, not allowed for all the reinforcements for which I have argued, because they do not come from Ptolemy.

As for the *hecatostys*, the name no more implies a unit of 100 men than the name *decad* one of 10 men; decads could number 16 men (Arr. vii 23.3-4). Such names must indeed once have had a numerical significance, but that significance might have been lost in an evolution that went back beyond Alexander.⁴³ Indeed, Tarn himself believes (for inadequate reasons, to which I shall come later) that the term hipparchy always implies 1000 men, and that since the Companions were organised in 330 under two hipparchs, and therefore (?) constituted two hipparchies, the Companions at that time numbered 2000. This argument, like his attempt to show that they were 2000 at Gaugamela (pp. 159-60), has no cogency; but the figures for reinforcements show in fact that for a considerable period from 333 the Companions must have numbered over 2000. And so long as they were 2000 or more than 2000, it follows that the actual average strength of the 8 *ilai*, and later the 8 hipparchies, must have exceeded 200.

Tarn's arguments, moreover, hardly show that at any time the nominal strength was in fact 200. As early as 334 we find that Parmenio was given a force which included 200

unsound. Though they were still basically territorial in recruitment in 331 (Arr. iii 16.11, *contra* Curt. v 2.6), it does not follow that they must have differed in strength with the differing populations of the recruiting areas, as (a) everywhere there were great reserves of manpower; (b) deficiencies in one area could have been supplied from another, without destroying the territorial character of the *ilai*.

⁴² In the Roman army, when the nominal strength of a legion was 6200 men (Festus 453 L), but the real strength often much less, multiples of 6000 are often unrealistically employed to designate the number of legions (e.g. Plut., *Cic.* 36.1; App. *BC* iii 58; *Bell. Mith.* 72). Berve's view of course implies that a nominal strength was given, below the real strength.

⁴³ Anaximenes (Jacoby, no. 72) F.4, by ascribing the institution of *lochoi* and decads, and also the establishment of both *hetairoi* and *pezetairoi*, to a king

Alexander implies at least that all were earlier than Philip, though in my view nothing else can be based on this much-discussed statement. I do not doubt that Anaximenes was fathering on Alexander I the whole organisation of the Macedonian army, as Philip found it, but that this is quite unhistorical; Macedon had no good infantry till Archelaus at least (*cf.* Thuc. ii 100.1-2 and 5; iv 124 ff.); on the other hand we cannot amend to 'Archelaus' (even if that were palaeographically likely), nor assume that Alexander II, who ruled only for one disturbed year, is meant, since the Macedonian cavalry were noted even before Archelaus (n. 47). No one king was responsible for all these measures, and none 'accustomed the most distinguished men to ride horses and called them *hetairoi*'; horse-riding and the name, *hetairoi*, were surely of immemorial antiquity.

Companions (Arr. i 18.1). But it also included 2500 Macedonian foot, and this number hardly corresponds to any single *taxis* of foot, or combination of *taxis*. In this case, then, we have a detachment which probably consists of *vexillationes* from several regiments, and that may be equally true of the later cases cited by Tarn. On another occasion we hear of a detachment of 60 Companions (Arr. iv 3.7). The nominal strength of an *ile* or hipparchy can then be inferred neither from the mention of detachments consisting of 200 men or multiples of 200, nor from the literal meaning of the term *hecatostys*. All that is certain is that the actual strength of the squadrons must have grown or diminished, in proportion to the changes in the size of the Companion cavalry as a whole.

There is then in the other evidence nothing to confirm or refute Diodorus' estimate of 1800 for the Macedonian horse or perhaps for the Companions only in the expeditionary force which crossed the Hellespont in 334; whether this figure includes *prodromoi* is uncertain, but in any event we must allow for some other Macedonian cavalry already in Asia. We cannot deduce from references to units of 200 horse any conclusions about the number of Companions that Alexander had at any particular time.

Tarn's assertion that 'the number 1000 for a hipparchy is certain' (p. 165) rests on the feeblest evidence.

(a) He argues that because Alexander raised 1000 horsemen from the Dahae (v 16.4) this shows that he had 'formed the opinion that 1000 was the right figure for a large cavalry unit'. Comment is superfluous.

(b) Ptolemy (*ap.* Arr. v 14.1) says that he crossed the Hydaspes with 5000 men. According to Tarn this is a round figure for 5300 men, made up as follows: Dahae (1000 men), 4 hipparchies (4000) and the *agema* (300). But in the actual enumeration of the forces employed (v 12.2) we hear of Bactrian, Sogdian and Scythian horse and only 3 hipparchies. Tarn says (pp. 192-3) that Coenus' hipparchy is omitted, and this is plainly true. We know that it was also present, because it took a prominent part in the fighting. But its omission opens the possibility that other hipparchies were also omitted in Arrian's enumeration, and if there were in fact 8, this must be so, even allowing for Craterus' hipparchy remaining behind. Moreover, if the hipparchies were still exclusively Macedonian, none can have numbered 1000 men, and the total of 5000 horse must be explained by the inclusion of the Bactrians, etc. Tarn believes that they were already incorporated within the hipparchies. That remains to be discussed (section IX): but it must be pointed out that Arrian here distinguishes not only the Dahae but all the Oriental horse from the hipparchies. Finally, as the number given for the infantry engaged is wrong (Tarn, 193), we cannot place much reliance on the figure for the cavalry.

(c) Hephaestion just before his death commanded 'the most distinguished hipparchy' (Diod. xviii 3.4); he is also named chiliarch (Arr. vii 14.10). But, as 'chiliarch' denotes the 'vizier' of the Persian Court (Diod. xviii 48.5) the minister with 'care for the whole kingdom' (Arr. fr. 1.3, Jacoby, no. 156) this title need not be associated with his hipparchy. Tarn has to assume that Hephaestion now commanded the only hipparchy (though the words of Diodorus imply the reverse), consisting of all the Companion cavalry; he neglects the evidence of Curtius that Alexander proposed to retain 2000 Macedonian cavalry and that of Diodorus that Perdiccas had more than 1000 after Alexander's death.⁴⁴ Moreover, as late as 324, when there were 5 hipparchies, Macedonians were still found in all of them, and no subsequent reorganisation, such as Tarn must assume, entailing the concentration of all Companions in one hipparchy is attested. There is no warrant for Tarn's view.

⁴⁴ Cf. pp. 38-9

VII. THE TERRITORIAL BASIS OF RECRUITMENT

Of the *ilai* that accompanied Alexander into Asia, Apollonia (Arr. i 12.7), Anthemus (ii 9.3) and the otherwise unknown Leugaia (ii 9.3) are each known to have supplied one. In the Balkan campaign Heraclides and Sopolis commanded cavalry from Bottiaea and Amphipolis (i 2.5), and it is assumed that in Asia they were still in command of the same squadrons (iii 11.8). It is thus apparent that the squadrons were enrolled territorially. Both Berve (p. 105) and Tarn (p. 154) suppose that all the squadrons Alexander took with him came from the lands, Chalcidice and the coastal districts, which Philip had annexed. It is of course well known that Philip allotted lands in the newly annexed areas to both Macedonians and foreigners on condition that the fеоffees rendered military service.⁴⁵ But Anthemus was a part of Macedon before Philip's reign,⁴⁶ and during the Balkan campaign Alexander certainly had with him cavalry from Upper Macedon (i 2.5). Berve supposed that all the squadrons from old Macedon were left with Antipater. That cannot be proved, but if it were true, it could be connected with the fact that three of the six phalanx regiments were taken from Upper Macedon; we might conjecture that each part of the kingdom had to make its contribution to the expedition (though no contingents from the lower part of the old kingdom are actually named). I am not clear what Tarn means (p. 155) by saying that 'there is nothing to be said for (Berve's) view that there must have been an equal force of cavalry raised from old Macedonia and left with Antipater'. Since Tarn agrees with Berve that all Alexander's *ilai* came from the newly conquered lands, he seems to imply that old Macedon was incapable of supplying an equal (or perhaps any) number of horse. This is a patent absurdity; Macedon had been noted for its cavalry long before Philip's conquests.⁴⁷ But I need not dwell any more on Macedonian resources in manpower: Alexander clearly did not take with him all, or even the greater part, of the Macedonians fit for military service; and even with Antipater's army mobilised, some men had to be left to till the fields.

VIII. ORIENTALS IN THE HIPPARCHIES

It is of course certain that from the time of Darius' death Alexander made progressively more use of Oriental cavalry. Apart from *hippakontistai* (Arr. iii 24.1) and *hippotoxotai* drawn from the Dahae (v 12.2; 16.4 cf. Curt. ix 2.24), we hear of the following contingents in his service before 324:

Arachosians	}	At the battle of the Hydaspes (Arr. v 11.3; 12.2)
Bactrians		
Parapamisadae		
Scythians		
Sogdians		
Indians		
		(Arr. v 2.2-4; 3.6; vi 2.3)

If his cavalry totalled 15,000 about the time of the voyage down the Indus,⁴⁸ a consider-

⁴⁵ Cf. Theopompus (Jacoby, 115) 225 (b) at end; Diod. xvi 34.5; Ps-Dem. vii 41; SIG³ 332; Arr. *Ind.* 18.4 and 10 (Nearchus at Amphipolis). F. Hampl, *Der König d. Maked.* (1934) 22 ff., adduces some other texts which may be relevant. There is much evidence and exaggeration on the expropriation or enslavement of the citizens of towns taken by Philip; doubtless many pro-Macedonian inhabitants remained (and did service for their lands), but much

was clearly available for distribution to Macedonians and foreigners enrolled in the Companions.

⁴⁶ Dem. vi 20; Liban. *Hyp.* to Dem. i, section 2.

⁴⁷ Thuc. i 61.4; 62.3; iv 124; especially ii 100.5. One Upper Macedonian principality, Elymia, could put 400 horse into the field in 382 (Xen. *Hell.* v 2.40).

⁴⁸ Plut. 66. His figure of 120,000 foot roughly agrees with Nearchus' total of over 120,000 for horse and foot (Arr., *Ind.* 19.5).

able proportion of them must have been Oriental; of these doubtless many were Indians, employed only in their own country.

By 324, at latest, Orientals were actually incorporated in the hipparchies. In recounting the causes of the mutiny at Opis in 324 Arrian first refers to the arrival of the Epigonoι, to Alexander's adoption of 'Median' dress and to the marriages at Susa; of what follows (vii 6.3) I offer a literal translation:

'They (the Macedonian soldiers) were aggrieved by (a) Peucestas, satrap of the Persians, who was adopting the Persian dress and language, because Alexander delighted in his Orientalism, and (b) by the Bactrian, Sogdian and Arachosian cavalry, and the horsemen of the Zarangians, Areians, Parthyaecans and among the Persians the so-called Euacae, who had been brigaded in the Companion cavalry, in so far as any of them seemed to be distinguished by rank, physical beauty or any other merit, and (c) by a fifth hipparchy which had been added to these—it was not wholly barbarian, but when the whole cavalry force had been augmented, some barbarians had been enrolled in that force, and (d) by Cophen . . . (a list of Oriental notables follows), who had been added to the roll of the *agema*, and (e) by Hystaspes the Bactrian who had been appointed as their leader, and (f) by the Macedonian spears which had been given to them in place of Oriental javelins—by all these things the Macedonians were aggrieved.'⁴⁹

When did this change occur?

There was no novelty in Alexander's adoption of 'Median' dress; but on the whole this passage creates the impression that the grievances were new. Certainly the arrival of the Epigonoι (vii 6.1) and the marriages (vii 4) were recent; Peucestas had been satrap of Persis only since 324 (vi 30); Zarangians, Areians and Parthyaecans had joined the Grand Army, for the first time to our knowledge, in Carmania, late in 325 (vi 27.3), and the Euacae probably a little later, when Alexander was passing through Persis (vi 29 ff.). Moreover, the mention of the new fifth hipparchy implies that somewhat before its creation there had been only four (presumably excluding the *agema*), but in 326 there had been eight. The reduction in the number of hipparchies (as Berve saw)⁵⁰ is probably to be connected with the losses in India and especially in the Gedrosian march. It is then natural to think that the whole reorganisation described here belongs to 324, and in that case there is no evidence that hitherto Orientals had served in the hipparchies. It may be noted that even now only an élite are enrolled (*cf.* (b) above).

The reference to the augmentation of the whole cavalry force, and indeed the creation of a fifth hipparchy, does indeed raise a difficulty. If the number of hipparchies was reduced because of losses, how can Arrian say that the cavalry were increased, and why was a fifth hipparchy then created? To this an answer can be conjectured. The losses first led Alexander to consolidate his Macedonian horse in four hipparchies; but Oriental reinforcements (and perhaps Macedonian)⁵¹ once again increased the forces at his disposal, and when he had decided to admit some of them to the hipparchies, he felt able to add another hipparchy. On any view it can only have been for a short time that the hipparchies numbered four.

⁴⁹ (a) Πενέστας τε ὁ Περσῶν σατράπης . . . ἐλάπει αὐτούς . . . (b) καὶ οἱ Βακτρίων δὲ καὶ οἱ Σογδιανῶν καὶ Αραχωτῶν ἱππεῖς καὶ Ζαραγγῶν δὲ καὶ Ἀρείων καὶ Παρθυαίων καὶ ἐκ Περσῶν οἱ Εὐάκαι καλούμενοι ἱππεῖς καταλοχισθέντες ἐς τὴν ἵππον τὴν ἑταιρικὴν ὅσοι αὐτῶν κατ' ἀξίωσιν καὶ κάλλει τοῦ σώματος ἢ τῇ ἄλλῃ ἀρετῇ ὑπερφέροντες ἐφαίνοντο, (c) καὶ πέμπτη ἐπὶ τούτοις ἱππαρχία προσγενομένη, οὐ βαρβαρικὴ ἢ πᾶσα, ἀλλὰ ἐπανξηθέντος γὰρ τοῦ παντός

ἱππικοῦ κατελέγησαν ἐς αὐτὸ τῶν βαρβάρων, (d) τῷ τε ἀγῆματι προσκαταλεγέντες Κοκρὴν . . . (e) καὶ ἡγεμὼν ἐπὶ τούτοις ἐπισταθεὶς Ὑστάσπης ὁ Βάκτριος, (f) καὶ τούτοις δόρατα Μακεδονικὰ ἀντὶ τῶν βαρβαρικῶν μεσαγκύλων δοθέντα, ταῦτα πάντα ἐλάπει τοὺς Μακεδόνας.

⁵⁰ P. 111.

⁵¹ See section V.

I am not indeed satisfied that the passage quoted is fully intelligible; the Greek is clumsy and perhaps it is corrupt. We are told of the fifth hipparchy that it was not wholly barbarian; that might mean that it was more or less Oriental than the other four. One would expect some more precise statement to follow.⁵² But what does follow is a statement that relates to the whole cavalry force and repeats that barbarians were enrolled in it, with the slight addition that this occurred when the force was augmented. We then hear that a few young Iranians were added to the roll of the *agema*. It would make good sense if we could amend the text to secure the meaning that the fifth hipparchy and the *agema* were identical and that this unit was less Oriental than the other hipparchies, including only a few of the Iranian nobility. Any particular textual change must be conjectural,⁵³ but I doubt if the parenthesis in (c) is sound as it stands.

If these suggestions be correct, it was only in 324, not long before the mutiny at Opis, that the hipparchies had been reduced to four, and some Orientals admitted to them; soon afterwards a fifth hipparchy was formed, which also included Orientals and is probably identical with the *agema*, which now probably for the first time became a hipparchy in name and in which a very few Iranians were enrolled. These Iranians doubtless became Companions,⁵⁴ whereas Orientals in the other hipparchies did not;⁵⁵ and it was this hipparchy which Hephaestion commanded (vii 14.10) and in which he was succeeded *de facto*, if not in name,⁵⁶ by Perdiccas (Plut., *Eum.* 1), and then by Seleucus—'the hipparchy of Companions, which was the most distinguished' (Diod. xviii 3.4). (There were presumably Companions in the other hipparchies, but those units were not composed exclusively of Companions.⁵⁷)

Both Berve and Tarn dated the incorporation of Orientals in the hipparchies much earlier, Berve to 329 on the basis of a text which he misunderstood (see n. 11), Tarn (pp. 163 ff.) to 326, the time at which he supposed Alexander to have reorganised his cavalry into 5 hipparchies, together with the *agema*. It has, I hope, been shown above that the true date of the reorganisation of the cavalry—in 8 hipparchies—was 328, but it may be thought that it was also at this time that Orientals were introduced into them, and that Tarn's arguments for the view that they are found in the hipparchies before 324 will still stand, with this necessary modification.

Tarn detects evidence for this view in Arrian's account of the battle of the Hydaspes. But his interpretation cannot be accepted. Arrian tells us that Craterus was left in command of his own hipparchy and of the cavalry of the Arachosians and Parapamisadae, and that Alexander crossed the river with certain hipparchies and the cavalry of the Bactrians, Sogdians, Scythians and Dahae (v 11.3; 12.2). His language suggests that these Oriental horsemen served in units distinct from the hipparchies. Tarn himself believes that the Scythians and Dahae were not in the hipparchies (as they are not named in vii 6), but it is arbitrary to draw any such distinction between them and the other Orientals who are mentioned in the same breath. Again Arrian speaks of the Companions charging (v 16.4);

⁵² This objection applies against Tarn's assumption (p. 165) that the fifth hipparchy had a much smaller Macedonian squadron than the rest. If it was largely barbarian, why are we not told how it was composed, as with the other hipparchies?

⁵³ Perhaps read *ἐς αὐτήν* for *ἐς αὐτό*, and delete *τῷ τε ἀγίματι προσκαταλεγέντες* as a gloss; *τε* is curiously unemphatic, in contrast to *καί*, joining each other grievance. Read in that case: *καὶ πέμπτη ἐπὶ τοῖς ἵππαρχίαι προσγενομένη, οὐ βαρβαρική ἢ πᾶσα, ἀλλὰ ἐπανεξηθέντος . . . ἐς αὐτήν τῶν βαρβάρων Κωσκήν κτλ.* The partitive genitive *τῶν βαρβάρων* now reads better; with the manuscript text *οἱ βάρβαροι* (vel *sim.*) might be expected.

⁵⁴ Oxyathres is attested as a *hetairos* (in the narrower sense) by Plut. 43 and Curt. vi 2.10. But the title cannot have been withheld from other Iranians high in Alexander's service. And Iranians in the *agema* must have been *hetairoi*, at least in the broader sense that all Macedonian cavalry were.

⁵⁵ But cf. Arr. vii 11.3, for Alexander's action in pique when the mutiny broke out; we do not know if he persisted in it later.

⁵⁶ Berve, 112.

⁵⁷ I do not understand Tarn's statement (p. 167) that after Alexander's death Macedonian troopers are not found; they are well attested in the civil war of 321.

but if they were brigaded with Orientals, why do we not rather hear of the cavalry or hipparchies charging? Casualties too are given separately, as if for separate units (v 18.3). It seems clear that in 326 the Macedonian cavalry were still in units distinct from the Orientals who are actually named. Tarn indeed allows for this by supposing that each hipparchy consisted of two *ilai*, one Macedonian and the other Oriental. But the text on which he relies (Arr. vi 21.3),⁵⁸ though it could be read in this sense, is quite ambiguous and does not even prove that there were only two *ilai* to a hipparchy. Tarn's interpretation of it is founded on his belief that 'the final reorganisation is fully described by Arrian vii 6.3, not in its place, but by reference backward on a later occasion'. However, it has already been shown that this passage of Arrian is most naturally understood as relating to a system that was quite recent in 324, and not to one which went back to 326, or rather 328. It plainly refers to a moment when the number of hipparchies had been reduced to 4 and then increased once more to 5. Tarn could only hold that it was applicable to the Indian campaigns on the basis of his hypothesis that the hipparchies already numbered 5, plus the *agema*; but this hypothesis has been refuted. Furthermore, once it is realised that the number of hipparchies was fixed at 8 as early as 328, and remained at this figure in 326, it can be seen that the Bactrians, etc., cannot have been admitted to the hipparchies at the time of the reorganisation, since they were not then subdued. Finally, Arrian's description of the system adopted by 324 is not naturally read as suggesting one in which each hipparchy consisted of one *ile* of Macedonians and one of Orientals; his statement that 'foreigners were mixed up in the ranks of the Companions (vii 8.2)' rather indicates an arrangement such as Alexander made later when he incorporated Persian foot in the phalanx; each decad then consisted of both Persians and Macedonians (vii 23.3-4). It would not be surprising if he had tried out a comparable arrangement with the cavalry rather earlier, since his Oriental subjects could provide better mounted troops than foot.

Thus Tarn's arguments fail. I am, however, less certain about rejecting the views of Mr Griffith, which appear elsewhere in this journal (pp. 68ff). I may remark that there is at least no clear evidence that Persian cavalry were used at all in India or that any Orientals were as yet included in the hipparchies, and that even though it was militarily and politically wise to employ them, it does not follow that they would not all have served in separate units.

CONCLUSION

If the arguments in this paper are right, it was in 328 after Clitus' death that Alexander first organised his Macedonian cavalry in 8 hipparchies. The hipparchies were larger than the 8 *ilai* in which the Companions were previously brigaded, because since 334 Alexander had received considerable reinforcements and about this time he consolidated the Macedonian *prodromoi* in the hipparchies. There is, however, no evidence that as yet the Oriental cavalry whom he had begun to use formed part of the hipparchies. That development cannot be securely placed earlier than 324, when a new organisation, first into 4 and then into 5 hipparchies, had recently been devised and when the intrusion of Orientals first caused complaint. If this be so, it is of some importance for the study of Alexander's policy towards his Oriental subjects. I hope also to have shown that the reserves of Macedonian manpower have often been seriously under-estimated.

⁵⁸ ἀναλαβὼν . . . τῆς ἑπτον τῆς ἑταιρικῆς τό τε ἄγλημα καὶ ἴλην ἀφ' ἐκάστης ἱππαρχίας.

TABLE 1.—DIODORUS' NUMBERS FOR THE FORCES THAT CROSSED WITH ALEXANDER
(xvii 17)

<i>Infantry</i>				<i>Cavalry</i>			
Macedonians 12,000	Macedonians 1800
(Greek) Allies 7,000	Thessalians 1800
Mercenaries 5,000	Other Greeks 600
Odrysians, Triballians and Illyrians			7,000	'Thracian <i>prodromoi</i> and Paeonians'			900
Agrianian Archers 1,000				
<hr/>				<hr/>			
<i>Total</i> 32,000	<i>Total</i> 5100
(as given by Diodorus)	..		(30,000)				(4500)

TABLE 2.—TOTALS FOR ALEXANDER'S ARMY (see n. 16)

<i>Infantry</i>					<i>Cavalry</i>	
Callisthenes 40,000		4500
Ptolemy 30,000		5000
Aristobulus 30,000		4000
Anaximenes 43,000		5500
Arrian	over 30,000	over	5000
Diodorus	{ 32,000	{	5100
				{ 30,000		4500
Justin 32,000		4500
Livy 30,000		4000
Frontinus	In all—40,000		

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EUBULUS

THE politicians of Athens in the 350s and the 340s are a shadowy lot save for Demosthenes and Aeschines, neither of whom was an influential figure before 346: the latter had only just begun his political career by that year, and the former was constantly in opposition and singularly unsuccessful. For the great names, Diophantus of Sphettus, Aristophon, Hegesippus and Eubulus, we have to rely almost entirely on scattered allusions in Demosthenes. No Hellenica survives to provide a framework of events: Diodorus xvi is principally Philippica and even omits entirely the most engaging political affair of the age, the making of the Peace of Philocrates. Except for the few fragments of Philochorus, without which we would be almost wholly without bearings, it is on what we can glean from his greatest opponent that we must rely for our understanding of Eubulus. Small wonder it is that the judgements of many modern historians have been strongly hostile to him.¹ In this way great oratory may persuade posterity where it failed in its own age, and we must beware of judging Eubulus from the standpoint of Demosthenes.

Clearly enough, Eubulus was responsible for major changes in the financial administration of the city and these were of far-reaching importance, but some may doubt whether he was any more than a financier. After all, Plutarch summed him up thus (*Mor.* 812 f.): *πίστιν ἔχων ἐν τοῖς μάλιστα καὶ δύναμιν οὐδὲν τῶν Ἑλληνικῶν ἐπραξεν οὐδ' ἐπὶ στρατηγίαν ἦλθεν, ἀλλ' ἐπὶ τὰ χρήματα τάξας ἑαυτὸν ἡύξησε τὰς κοινὰς προσόδους καὶ μεγάλα τὴν πόλιν ἀπὸ τούτων ὠφέλησεν.* There is, moreover, only one development of Athenian policy that is directly and explicitly attributed to him, viz. the attempt to unite Greece against Philip in 347/6.² Was he then of much importance as a politician in the wider sense?

Plutarch should not mislead us. There was no reason why Eubulus should have sought to be a general. In an age when warfare, like statecraft, was becoming ever more professional, the generalship was for professional soldiers like his fellow tribesman, Chares. Serious politicians did not need office to attain to great influence, and since Plutarch is simply concerned with office, his words are not inconsistent with Eubulus having been of the greatest importance as a maker of policy.

The limits of his eminence are not sure. The fact that Cephisophon of Aphidna was *ἐπὶ τὸ θεωρικόν* in 343/2 (*IG* ii² 223 C l. 5) does not show³ that Eubulus had lost control of the Theoric Fund. For one thing it is not clear that there was only one commissioner (indeed the limited evidence suggests a board)⁴ and even if Cephisophon was an opponent of Eubulus there may have been other commissioners who were not, but also there are no grounds for thinking that Cephisophon was an opponent; if his being general at Skiathos

My thanks are due to Mr D. M. Lewis both for help in the preparation of this paper and for allowing me to see a draft of an article on the powers and position of Eubulus and Lycurgus, on which subject we hold similar but differing views. He is not to be thought necessarily to assent to any part of this paper. I also wish to thank Professor Andrewes and Mr A. R. W. Harrison for helpful criticism.

¹ Motzki, *Eubulos von Probalinthos und seine Finanzpolitik* (1903), esp. 70 f., followed Schaefer, *Demosthenes und seine Zeit* i² 186, 200 and 212 in condemnation. Beloch was exceptional in his favourable estimate (cp. *GG* iii².1 486 and *Att. Pol.* 176 f.),

but even he regarded the Theoric distributions as disastrous (cp. *GG* iii².1 344), and in general I have no sympathy for Beloch's attitude to Athenian policies in this period.

² Dem. xix 10, and 303 f., etc. I have discussed the dating of these embassies and their relation to the negotiations of 346 in *REG* lxxiii (1960) 416 f.

³ Pace Glotz, *Rev. Hist.* clxx (1935) 385 f.

⁴ Aesch. iii 25 and Ar. *AP* 43.1, supported perhaps by Dem. xxiii 209 (*οἱ τὰ κοινὰ διοικοῦντες*), outweigh *IG* ii² 223 C l. 5, and Aesch. iii 24 and ii 149, which are no more than consistent with the office not being collegial.

in 340⁵ and at Byzantium in 340/39⁶ proved that he was 'un homme de parti patriote' and an opponent of Eubulus, by the same proof we would have to count as another opponent Phocion who was with Cephisophon at Byzantium.⁶ For what it is worth the work on the Skeuothekē, which continued to 339/8, is attributed to Eubulus⁷ and perhaps his financial influence continued after he had ceased to govern policy. It is as crude in his case as it is plainly wrong in the case of Callistratus to speak in such absolute terms as 'la période d'Eubule' and 'la grande période de Demosthène'. At the trial of Aeschines in 343 he was still a figure of the first importance (Aesch. ii 184), and that was after the serious change in Athenian policy towards Philip which began in that year and was marked by the execution of Philocrates.⁸ As for his emergence as an important politician, we are equally unsure. The law regulating the use of τὰ περιόντα χρήματα τῆς διοικήσεως appears to have been in operation in 353/2 if the speech *On the Syntaxis* belongs to that year,⁹ and since it is with Eubulus that this matter is associated he was probably responsible for the law.¹⁰ Similarly, in the same speech Demosthenes alludes scornfully (30) to the public works programme with which again Eubulus' name is associated.¹¹ So by 353/2 he had attained eminence as a financier and in this connexion he is alluded to in the *Leptines* (137) of 355/4. But it is doubtful whether he was in general dominant as early as this.¹² The evidence on which is based the assertion that it was Eubulus who made the peace at the end of the Social War is unsatisfactory: the passage in question of the scholiast on the *Third Olynthiac*¹³ probably refers to Eubulus' conduct of affairs after the peace, not to the making of it. Indeed, it is probable that Diophantos, ἐκεῖνος ὁ Σφήττιος (Dem. xxxv 6), was the important figure, financially speaking, immediately after the Social War. He, like Eubulus, was concerned with the distribution of money from the Theoric Fund (Schol. Aesch. iii 24) and is mentioned in 355/4 along with Eubulus as active in financial matters (Dem. xx 137), and it is clear from Demosthenes' reference to him in 343 (Dem. xix 297) that he had earlier been as powerful as Aristophon and Callistratus. So Eubulus may not have risen to great political importance until some time had elapsed after the Social War and until the credit he had gained in the interim as a financier was extended to his counsels on policy. Diophantos indeed was still alive in 343 and had evidently been engaged in politics in 346 (Dem. xix 198), and it is hard to say when Eubulus became the more influential with the δῆμος. The general sent to Thermopylae in 352, Nausicles, was probably the man who supported Aeschines in 343¹⁴ and so presumably a supporter of Eubulus, but Diophantos moved the decree of triumph,¹⁵ and he as much as Eubulus, or even more, may have been responsible for Athens' prompt help for Phocis.¹⁶ The evidence is too thin to say more.

⁵ IG ii² 1623 l. 35 and 1629 l. 484.

⁶ IG ii² 1628 l. 438 and 1629 l. 959.

⁷ Din. i 96 and Aesch. iii 25.

⁸ Dem. xix 116, Hyp. iv 29, Aesch. ii 6.

⁹ Cp. 1 f. Dionysius of Halicarnassus does not furnish a date for the speech. 32 and 33 suggest that it is prior to the expedition against Megara of 350/49 (Philoch. F. 155) and belongs to the period of dispute over the border territory, which had certainly begun by 352 (IG ii² 204, a decree of late 352, which alludes in l. 55 to an earlier decree). Further it is to be noted that the speech alludes to the destruction of the democracy at Rhodes (8), but gives no hint of the appeal for help made in 351, nor does it refer to the stirring events of 352 and later, which eclipsed such trifles as the dispute with Megara and the appeal from Phlius (32). Blass *Att. Bered.* iii² 1 398 f. assigned the speech to 353/2 (without the aid of the fragment of Philochorus).

There is no justification for identifying the speech alluded to in 9 with the First Philippic, as Croiset did (*Demosthène, Harangues* i—Budé edition, 72).

¹⁰ As was known or inferred by the Scholiast to Dem. i 1 (= Dind. viii 33 l. 12).

¹¹ Cp. Din. i 96 and Scholiast to Dem. iii 29 (= Dind. viii 133 ll. 19 and 27).

¹² Eubulus is now known to have been one of the nine archons in 370/69 (*Hesperia* xxix (1960) 25) but this does not add anything of importance.

¹³ Dind. viii 133 l. 8. Schaefer, *op. cit.*, i 187 accepts the Scholiast, but with hesitation. Cp. Sealey, *JHS* lxxv (1955) 75.

¹⁴ Diod. xvi 37-3 and Aesch. ii 184. Cp. CQ xii (1962) 140 n. 1.

¹⁵ Dem. xix 86 with Schol.

¹⁶ The Melanopus who went to Mausolus as ambassador (Dem. xxiv 12, etc.) was a relative of Diophantos (Harpocration s.v. 'Melanopus').

Eubulus was probably both the ally and the natural successor of Diophantus, for the latter was clearly on Aeschines' side in 343,¹⁷ but we cannot be sure about what in the late 350s Eubulus was actually responsible for.

That he became a figure of first importance is clear, if from nothing else, from the manner in which Demosthenes spoke of him in 343. Aeschines concluded his defence (184) by calling to his support *Εὐβουλον μὲν ἐκ τῶν πολιτικῶν καὶ σωφρόνων ἀνδρῶν* - - - *Φωκίωνα δ' ἐκ τῶν στρατηγῶν, ἅμα δὲ καὶ δικαιοσύνη διενηνοχότα πάντων, ἐκ δὲ τῶν φίλων καὶ τῶν ἡλικιωτῶν τῶν ἑμαντοῦ Ναυσικλέα* but it was on Eubulus that Demosthenes had concentrated his attack (xix 288-97): he compared the power of Eubulus with that of Callistratus or Aristophon or Diophantus in earlier periods, and claimed that such power was a menace to the city. Thus as late as 343 Eubulus was the mainstay of Demosthenes' opponents and formidable. The same impression is derived from the references to him in the *Midias* (206, 207). It is Eubulus who is singled out as a politician, and the appeal of *Midias* to Eubulus in the assembly after the Dionysia 348 to which Demosthenes refers (206) suggests that Eubulus was of special importance. His record in the courts supports this. He attacked Aristophon, though this may have been a mere matter of finance,¹⁸ but also Aristophon's favoured general, Chares (Ar., *Rhet.* 1376 A 10), and Cephisophon of Aphidna who was one of Chares' supporters.¹⁹ In turn, Aristophon attacked Philonicus an adherent of Eubulus and made the trial an attack on Eubulus' whole political record (Dem. xix 291). All in all, there is enough to suggest that he was a figure of the greatest importance, and it was no error when Theopompus included him in his survey of the Athenian demagogues.²⁰

His power and influence are best attested in the period 349 to 346. All the leading figures connected with the Euboean expedition, *Midias*, Phocion, Hegesileos, are found linked with Eubulus and his supporters.²¹ Of course, *Midias* had a special importance in advocating the expedition by reason of his connexion with Plutarch of Eretria (Dem. xxi 110), and the particular trouble between him and Demosthenes probably derived from Demosthenes' opposition to the expedition,²² but no doubt the group was united in policy and Eubulus is the dominant figure. This is shown by the direct attack on him in the *Third Olynthiac* (21-9); the attack on those who are influential with the People culminates in a contemptuous reference to Eubulus' programme of public works²³ and in the *Midias* Demosthenes admits that Eubulus is his enemy.²⁴ The Eubulus group continued to be powerful in 348: Demosthenes did not dare to bring his charge against *Midias* to court and the case went unheard.²⁵ In 346 Eubulus led the movement to unite

¹⁷ Schaefer, *op. cit.*, i 205 supposed that Eubulus was an opponent of Diophantus, on the grounds that Eubulus is unlikely to have approved of the expensive expedition to Thermopylae in 352 (*cp.* Dem. xix 84) and that Demosthenes proposed in 343 to call Diophantus to give evidence against Aeschines (Dem. xix 198). But it is to be noticed that Demosthenes says he will 'compel' Diophantus; i.e. he must have been disinclined to help the prosecution. As to Thermopylae, apart from the connexion with Nausicles, there is no evidence and it is mere caricature to suppose that Eubulus opposed all expense. *Cp.* Beloch *GG* iii² 1 486 ('Wo ein wirkliches Lebensinteresse Athens in Frage stand, hat Eubulus nicht gezögert, die ganze Macht des Staates einzusetzen. . . . Nur die nutzlose Zersplitterung der Kräfte suchte Eubulus zu hindern.')

¹⁸ Dem. xxi 218 (with Scholiast), xix 291.

¹⁹ Dem. xix 293, Aesch. ii 73. Eubulus appears to have had a reputation for his ready recourse to the courts; *cp.* Dem. xxi 207 (τὸν ἄλλους ῥαδίως κρίνον).

²⁰ *FGH* 115 FF. 99, 100. (The words τὰς προσόδους καταμυσθοφορῶν in the latter fragment I take to refer to the expenditure on public works. *Cp.* Plut., *Per.* 12 ὅλην ποιοῦσαν ἔμμισθον τὴν πόλιν.)

²¹ For *Midias* see Dem. xxi 205-7, for Phocion Aesch. ii 184, for Hegesileos Dem. xix 290.

²² Dem. v 5. But their hostility was perhaps of longer standing; *cp.* Dem. xix 29 (τούτῳ πολέμῳ). They finally settled their dispute of Dionysia 348 some time after autumn 347 (*cp.* Sealey *REG* lxviii (1955) 96 f.) perhaps as a result of the temporary alliance of various groups in 346.

²³ *Cp.* Scholiast *ad loc.* (Dind. viii 132 ll. 19 and 27).

²⁴ 206 προσκέρρονκεν ἐμοί.

²⁵ See n. 22. Stephanus who prosecuted Apollodorus in 348 (Dem. lix 5) was probably a supporter of Eubulus (*ibid.*, 48), but the attack on Philocrates in the same year (*cp.* *REG* lxxiii (1960) 417 n. 2) was not sponsored by the same group: Aesch. ii 12 shows that some other group was responsible—one might

the Hellenes against the threat of Philip intervening in Greece and this policy of uniting in a κοινὴ εἰρήνη to which the Allied Synedrion continued to give their full support, was not a lame affair that came to nothing out of half-heartedness; but, rather, because the Phocian revolution made it certain that Philip could get into Greece, Eubulus' policy was suddenly rendered impracticable and the way was open for the advocate of peace, Philocrates, supported by the defender of his proposal to negotiate for peace in 348, Demosthenes.²⁶ Eubulus and Aeschines accordingly were less prominent in Elaphebolion 347/6 and the peace forced on Athens by Philip was the work of their opponents:²⁷ Eubulus' only recorded contribution was to join in counselling the people not to follow the few hotheads who hoped to defeat Philip aided by Thebes, south of Thermopylae.²⁸ But the crisis of Skirophorion a few weeks later brought him to the fore again. When the second embassy to Philip returned on the thirteenth of that month, and reported that the peace had failed in its purpose and he was moving south against Phocis, the Council concurred in agreeing to go out to face Philip, but within three days, before the People had had an opportunity to ratify the Council's decision, word came that Philip was in control of the Gates. In the Assembly on the sixteenth only Demosthenes advocated the supreme folly of going out to fight for a Phocis virtually lost, and Aeschines' moderation won the day.²⁹ Perhaps to this assembly should be ascribed Eubulus' decree protesting about Philip's seizure of the Thracian forts, though this may belong later (Dem. xviii 70), and the speech in which he cursed Philip (Dem. xix 292). In 347/6 he made a start with one of his most important public works, the construction of the new Skeuothekē: at any rate the annual ten talents eisphora for metics that was used for it probably began in this year (IG ii² 505 l. 12 f.), perhaps shortly after the decision of Elaphebolion. It was a matter of military importance, and points to his political power in that year, and Aeschines in 343 stood trial for τὰ Εὐβούλου πολιτεύματα.³⁰

It would be exaggeration to claim that this evidence in itself shows that Eubulus occupied a Periclean position in Athens. It shows strictly no more than that Eubulus was one of the leaders of a successful political faction. Yet, when one balances up the frequency with which Eubulus appears in the speeches of Demosthenes with the comparative infrequency of all other politicians of the age and remembers that Theopompus saw fit to include Eubulus in his survey of Athenian demagogues in Book x of the *Philippica* (see note 20), one is justified in regarding Eubulus as the leading politician of the decade before the trial of Aeschines. Good sense and knowledge of Athenian politics at other times should caution against making Eubulus wholly responsible for Athenian policy in that period. Even where names are denied us, we may suspect the presence of Aristophon amongst the backers of 'ἡ νῦν βοήθεια' of the *First Philippic*, i.e. οἱ 'ταχὺ καὶ τήμερον' εἰπόντες (14) or again amongst those who backed the prosecution of Philocrates in 348 for proposing to negotiate with Philip (see note 25): we know he took the radical line in 346 of opposing peace.³¹ No doubt he was not silent in 349/8. None the less, it is legitimate

guess Aristophon. Indeed, if Eubulus had been responsible, Aeschines could hardly have forbore to use the attack on Philocrates to help his proof that he had not collaborated with him. Thus the acquittal of Philocrates was not a defeat for Eubulus.

²⁶ This is the contention of my article 'Aeschines and the Peace of Philocrates' in *REG* lxxiii (1960).

²⁷ Philocrates framed the peace and the work of steering it through the Assembly was left to his defender of 348, Demosthenes, whose role as real patron of the peace is seen in his decrees of 6 Elaphebolion (Aesch. ii 46, iii 36, Dem. xix 234 and xviii 28), and again of 8 Elaph. when he made the first proposal of alliance with Philip (Aesch. ii 61, 65, 109 f.),

as well as provided for special seats for the Macedonians at the festival (Aesch. ii 55 and 110). His guidance was vital on the 18th and 19th (Dem. xix 144, 321 Aesch. iii 71 f.). It would be proper to call it the Peace of Demosthenes.

²⁸ Dem. xix 291. Cf. article cited in n. 26, p. 437.

²⁹ For the elucidation of the events of Skirophorion 347/6 see my article in *REG* lxxv (1962).

³⁰ Aesch. ii 8. By 330 Demosthenes was claiming that he had no part in the proceedings of 346 and ascribing everything to Aeschines, Philocrates, Eubulus and Cephisophon (Dem. xviii 21)! Cf. 23 of the same speech for another piece of fantasy.

³¹ Theopompus F. 166.

to see in Eubulus the real successor of Callistratus and Diophantus and regard him and his group as in general responsible for Athenian policy in these years.

One thing was obvious. Theban power had to be contained, and there can have been little real disagreement about supporting Phocis in central Greece and Thebes' opponents in the Peloponnese. But the war in the north was a grave embarrassment. This was no longer a matter of the vital interest of Athens in protecting her food supply: to judge by the *Aristocrates* (173) there were no further operations in the Hellespont after the treaty made with the Thracian kingdom in 357, until Chares seized Sestos in 353/2 and Cersobleptes ceded all the cities of the Chersonese save Cardia (Diod. xvi 34.3, 4). The reason for this inactivity may be guessed to have been that the treaty sufficed for Athens' purposes and that Chares only intervened so bloodily in Sestos when Sestos by favouring Philip threatened both Cersobleptes and Athens; this explains, at any rate, why Cersobleptes conceded so much after Chares' act of violence. So the Chersonese was forgotten, until Philip's eastward expansion threatened it. With Amphipolis the position was far different. The continuous operations since 368 had accomplished precisely nothing beyond the extension of the war. For while a strong Macedon might tolerate an independent, if friendly, Amphipolis, its strategic importance, quite apart from its economic advantages, made a restoration of Athenian power intolerable for Macedon, and Olynthus was always ready to join in the defeat of Athenian ambitions. Thus in defence of a position militarily strong enough to defy the might of fifth-century Athens strong allies were close at hand to frustrate Athenian hopes in the fourth. The war, in short, had very little chance of a satisfactory conclusion. Nor did the city have any longer strategic value for Athens until in 352 Philip began to be a serious threat: the economic advantages of mines and forests were Athens' sole concern, and these could be gained elsewhere.³² All rational considerations urged the abandonment of a foolish quest. The People, however, could entertain no such idea. The desire and hope of recovering the city had taken too deep root for anyone to propose abandoning it: the proof of this is that, after spending twenty-two years in fruitless conflict and being forced in 346 by fear of invasion to surrender their claim, in 344/3, when Philip offered to demonstrate his peaceful intentions towards Greece by extending the Peace of Philocrates and making a Common Peace,³³ Athens' response was to renew her demand for Amphipolis ([Dem.] vii 24-5). No sane politician can possibly have believed in 344/3 that Philip would dream of ceding the city, but the People was persuaded by Hegesippus to make the demand. This implies nothing short of an obsession and, twelve years previously, it would have been political suicide to counsel the only sensible policy. The nearest alternative was to remain at war and expend the minimum of money and lives, let Demosthenes thunder as he would. If it was Eubulus who most strongly opposed the war for Amphipolis, or if his financial policy aimed at keeping Athens from squandering its resources on such a vain hope, his services to Athens were very great indeed.

The quest for Amphipolis was only a special case of the predominant desire that possessed Athens in the fourth century to revive the glory and recover the assets of the empire of the fifth. It is clear from Isocrates' *On the Peace* that there was a group at Athens that could be called a war-party, οἱ παρακαλοῦντες ὑμᾶς ἐπὶ τὸν πόλεμον, who - - - προσδοκίαν ἐμποιοῦσιν ὡς καὶ τὰς κτήσεις τὰς ἐν ταῖς πόλεσιν κομιούμεθα καὶ τὴν δύναμιν ἀναληψόμεθα πάλιν ἢν πρότερον ἐτυγχάνομεν ἔχοντες (5 and 6). The important words, practically speaking, are those concerning the recovery of possessions: this was what aroused the allies of the Social War, τοῦ κομίσασθαι τὰ ὑμέτερ' ὑμῖν φρονήσαντες as Demosthenes (xv 15)

³² Silver by more careful mining in Attica, timber by importation from Pontus and the West (cf. Michell, *Economics of Ancient Greece* 278 f.). Throughout this period of Macedonian control of Amphipolis,

the number of ships was large and the Athenian navy did not suffer for want of timber.

³³ For this interpretation of [Dem.] vii 30-2 see Wüst, *Philip II von Makedonien und Griechenland* 69 f.

says. The most notable possessions were of course Amphipolis and the Chersonese, but much more was meant. While formally continuing to respect the promises of the Decree of Aristotle,³⁴ in practice the People deserted the principles of the Confederacy after 371 in the resumption of fifth-century possessions. The Samian cleruchy aroused debate, or at least protest, to judge by the remark preserved by Aristotle (*Rhet.* 1384 B 32) of an otherwise unknown figure, Cydias, but it is Isocrates' treatise which best illuminates Athenian policy in this decade. It was, according to Aristotle (*Rhet.* 1418 A 32), an attack on Chares, who had a reputation for violence (Diod. xv 95.3), but it is, in essence, a denunciation of the whole attempt to restore the empire. The failure of the Social War made possible, as well as necessary, a radical change of policy.

The mainspring of the later Athenian imperialism is clear enough. The mass of Athenians looked to empire for wealth since all believed that only empire could bring it. Until Xenophon's *Revenues*, there is not a hint anywhere that the city's prosperity really depended on trade. Cruder notions prevailed, that it was tribute that enriched the city and provided employment in its beautification and in the defence of the source of the profits. Given the opportunity, a Cleon would prefer to continue a costly war out of desire for τοῦ πλέονος (Thuc. iv 21), while the moderate Thrasylbulus led Athens in the hopes of recovering the empire into a war supported by the poor and opposed by the rich.³⁵ This was the practice of the πλεονεξία denounced by Socrates. The only debate was whether to rest content with τὰ παρόντα or have recourse to violence. Xenophon's treatise was written, ἐπεὶ τῶν Ἀθηναίων προεστηκότων ἐλεγόντινες ὡς γινώσκουσι μὲν τὸ δίκαιον οὐδενὸς ἦν τῶν ἄλλων ἀνθρώπων, διὰ δὲ τὴν τοῦ πλείους πενίαν ἀναγκάζεσθαι ἔφασαν ἀδικώτερον εἶναι περὶ τὰς πόλεις (i 1). Isocrates himself seems more concerned with the injustice of the policy of war than with the profitableness of peace. Thus, although the policy of seeking prosperity in empire was in 355 for the time being discredited, no alternative that did not accommodate the People's desire for profits was likely to endure: if peace was to succeed, it must prove not only profitable but tangibly so. Hence the ill-famed distributions of τὰ θεωρικά which were probably indispensable if the Athenian People was to be saved from its own folly.³⁶

The alternative which Eubulus and his supporters proposed to the imperialism denounced by Isocrates appears to have been the establishment of a Common Peace to manage Greek affairs and settle disputes. This was what Eubulus and Aeschines sought in 347/6 both before and during the negotiations for the Peace (see note 26), and in 344/3, as in 354 (Dem. xiv 12), the threat of Persian intervention was met with a proposal to unite the Hellenes.³⁷ That these were not mere expedients to meet an external threat, but rather the approved policy in foreign affairs is suggested by the demand of Isocrates' *On the Peace* for the establishment of a general peace.³⁸ Φημι δ' οὖν χρῆναι ποιῆσθαι τὴν εἰρήνην μὴ μόνον πρὸς Χίους καὶ Ῥοδίους καὶ Βυζαντίους καὶ Κώους ἀλλὰ πρὸς ἅπαντας ἀνθρώπους, καὶ χρῆσθαι ταῖς συνθήκαις μὴ ταύταις αἷς νῦν τινὲς γεγράφασιν, ἀλλὰ ταῖς γενομέναις μὲν πρὸς βασιλέα καὶ Λακεδαιμονίους - - κ.τ.λ. (16). Similarly Xenophon in the *Revenues* advo-

³⁴ Andoc. iii 15 points to 'the recovery of the Chersonese, the colonies, the possessions (τὰ ἐγκτήματα) and the debts' as a leading motive for continuing the war in 392/1, and these were the things renounced in 377 by the Decree of Aristotle. *Cp.* Isoc. xiv 44 καὶ τῶν κτημάτων τῶν ἐμετέρων αὐτῶν ἀπέστητε, βουλόμενοι τὴν συμμαχίαν ὡς μεγίστην ποιῆσαι. Presumably κτήματα were chiefly cleruchies. The fifth-century boundary stone found at Cos (*Inscriptions of Cos* 160 no. 148) suggests that the island had other reasons than the proximity of Mausolus for joining the allies in revolt in 357.

³⁵ Xen., *Hell.* iii 5.10, Aristophanes, *Ecl.* 197, *Hell. Oxy.* 1. The result was the renewal of alliance between Sparta and Persia and the formation of the King's Peace.

³⁶ See n. 92 for Xenophon's notions on τροφή ἀπὸ κοινοῦ.

³⁷ See n. 33 and Dem. xviii 136 for Aeschines' support for Python's proposal; and [Dem.] xii 6 and Philoch. F. 157 for the response to Persia.

³⁸ *Cp.* Momigliano, *Annali della R. Scuola Normale Superiore di Pisa N.S.* v (1936) 109 f. for all this paragraph.

cates the establishment of a Common Peace to deal with the Phocian seizure of Delphi. *Εἰ <δὲ> καὶ ὅπως τὸ ἐν Δελφοῖς ἱερὸν αὐτόνομον ὥσπερ πρόσθεν γένοιτο φανεροὶ εἴητ' ἐπιμελούμενοι, μὴ συμπολεμοῦντες ἀλλὰ πρεσβεύοντες ἀνὰ τὴν Ἑλλάδα, ἐγὼ μὲν οὐδὲν ἂν οἶμαι θαυμαστὸν εἶναι, εἰ καὶ πάντας τοὺς Ἕλληνας ὁμογνώμονάς τε καὶ συνόρκους καὶ συμμάχους λάβοιτε ἐπ' ἐκείνους, οἷτινες ἐκλιπόντων Φωκίων τὸ ἱερὸν καταλαμβάνειν πειρῶντο* (v 9). So perhaps the Eubulus group believed that Athens, avoiding the pursuit of imperial aims, could meet any crisis at the head of the Hellenes united under a Common Peace.

If this was their belief, nothing could be, generally speaking, more naïve. The history of the Common Peaces since 387/6 could give little confidence that large states would always accept the majority decision even if majority decision were the best method of solving political problems. Yet, naïve as such a belief might be, the policy was the best possible for Athens in the age of Philip. The past one hundred and fifty years had shown that the cities of Greece could be kept united only by force or by fear of force. The Persian domination had failed simply because it did not possess or apply the force it pretended to: hence the disillusioning succession of Common Peaces. But the fear of Macedon could act as effectively on the Greek states, given proper statesmanship, as the fear of Persia had acted in the early fifth century. Who was the proper statesman? I have argued elsewhere³⁹ that Demosthenes' policy in the Olynthian War, in so far as it can be discerned, was ill-conceived and likely to result in military disaster: the important point was that Demosthenes wished Athens to deploy its full strength at the full stretch of unsatisfactory communications against an enemy of probably greater strength concentrated near his base; the result might have been a second Sicilian disaster. But politically too the policy of Demosthenes was ill-conceived. To Greece as to Athens the war against Philip was the war for Amphipolis,⁴⁰ the war for the recovery of the principal asset of the fifth-century empire, and this had little appeal for the Greeks. Until Philip tried to enter Greece, Athens could not hope for unity. The words that Demosthenes used in 354 (xiv 12) might well have been used in 349/8: *ἀλλ' οὐπὼ μείζων ἔσθ' ὁ φόβος τῶν πρὸς ὑμᾶς καὶ πρὸς ἀλλήλους ἐνίοις διαφορῶν*. Only in defence of Greece would the Greek cities unite and thus Eubulus' policy of meeting the threat of Philip by uniting the Greeks under Athens' hegemony in defence of Greece itself was the only course with any chance of success. Certainly there is no justification for labelling such a policy pacifism.

It is time to turn to Eubulus' financial arrangements, for it is there that censure is severest. From Demosthenes onwards, the charge has been repeatedly made that Eubulus pandered to the selfish side of the Athenians and so corrupted their will to resist Philip. To this is occasionally added the taint of treachery, not the treachery which preferred the interests of Macedon to those of Athens and about which scholars have shown remarkable faith in Demosthenes' truthfulness, but the treachery which preferred the comfort of a class to the freedom of its country. This might be briefly answered by pointing out that as far as the evidence permits of calculation the amounts normally distributed were small, fifteen talents according to A. H. M. Jones,⁴¹ less according to Kahrstedt:⁴² the debilitating effect of three to five drachmas a head per year is not likely to have been great.⁴³ But this

³⁹ CQ xii (1962).

⁴⁰ Cp. Isoc. *To Philip* 2.

⁴¹ *Athenian Democracy* 34.

⁴² *Göttingische Gelehrte Anzeigen* (Phil.-Hist. Klasse) 129 156 f.

⁴³ The evidence does not justify the view of Francotte (*Musée Belge* xvii (1913) 69 f.) that the Theoric distributions were really the old *διωβελία* in a new guise, i.e. a form of dole. Aristotle (*Pol.*

1267 B 1 f.) speaking about men's insatiability said *τὸ πρῶτον μὲν ἱκανὸν διωβελία* [sic] *μόνον, ὅταν δ' ἦδη τοῦτ' ἢ πάτριον, ἀεὶ δέονται τοῦ πλείονος ἕως εἰς ἄπειρον ἔλθωσιν* and presumably he was referring to Athens, but he may well have had in mind not developments under Eubulus but the later administration of the Theoric Fund in the 330s: Plut. *Mor.* 818 EF suggests that on occasion large amounts were to be expected in distribution and Hyper. v 26

perhaps is to do the Theorikon less than justice. If Demades could call it the 'cement of the democracy' (Plut., *Mor.* 1011 B), it may have been important, and many a reader of Demosthenes must have sympathised with the remark attributed to Gladstone⁴⁴ that 'Athens perished because of its poor public finance'. Eubulus was largely responsible for the finances of Athens in the decisive years, and if Gladstone was right Eubulus, whether pacifist or not, must bear the blame due to his system. However, before judging, we must make clear as far as possible what the system was.

There has been much discussion of the question how Eubulus exercised control. Motzki⁴⁵ was at pains to argue that Eubulus held the powers and office of the later attested Finance Minister, *ὁ ἐπὶ τῇ διοικήσει*, and, with the aid of Aristotle's statement in the *Constitution of Athens* (43.1) that *οἱ ἐπὶ τὸ θεωρικόν* were elected and held office *ἐκ Παναθηναίων εἰς Παναθήναια*, Motzki assigned him a quadrennium of office from 354 to 350, when he was succeeded in the next four years by Aphobetus whose brother, Aeschines, later said of him *καλῶς δὲ καὶ δικαίως τῶν ὑμετέρων προσόδων ἐπιμεληθείς, ὅτε αὐτὸν ἐπὶ τὴν κοινὴν διοίκησιν εἴλεσθε* (Aesch. ii 149) and who was in turn succeeded by Cephisophon of Aphidna (IG ii² 223 C l. 5). The usage of inscriptions shows clearly that the phrase *ἐκ Παναθηναίων εἰς Παναθήναια* does not mean four years⁴⁶ and the neat calculations of quadrennia should be banished for ever,⁴⁷ but beyond that one cannot be sure. Whatever bearing it has on the position and powers of Lycurgus, the absence of *ὁ ἐπὶ τῇ διοικήσει* from Aristotle's list of elective offices (AP 43.1) does not prove that this officer had not existed before the law of Hegemon which rearranged the various financial offices shortly before 335/4:⁴⁸ Eubulus might have secured for himself an extraordinary office. There is, however, little to support such a theory. The words, quoted above, that Aeschines used to refer to his brother's financial activity are far from technical or official usage, and the allusion in the *Aristocrates* (209) to *οἱ τὰ κοινὰ διοικούντες* seems to be to more than the one or two men who could have been *ὁ ἐπὶ τῇ διοικήσει* if such an office had been created after 355. The evidence is hardly precise,⁴⁹ but Eubulus seems to be associated with the Theoric Fund and it would be hard to see why the Theoric Commissioners should have developed such importance *διὰ τὴν πρὸς Εὐβουλον γενομένην πίστιν*, as Aeschines (iii 25) asserts, if Eubulus had not himself occupied the office. Further, if there had been some sort of Finance Minister, he would have managed the varied activities alleged for that Commission, who *σχεδὸν τὴν ὅλην διοίκησιν εἶχον τῆς πόλεως*.⁵⁰ So Eubulus must simply have been one of *οἱ ἐπὶ τὸ θεωρικόν*, a board elected for one year.

may be an instance. Nor is there any reason to suppose that Xenophon's extravagant ideas about *τροφή* in the Revenues (below, n. 92) were ever put into effect. There is no good evidence to connect the Theoric distributions with more than the festivals (see Kahrstedt, *op. cit.*, n. 42). *Etym. Mag.* s.v. 'διοβελία' (*ὀβελοὶ δύο ὅς ὁ δῆμος καθήμενος μισθοφόρει*) is hardly of much value. The two-obol dole, to the best of our knowledge, happened only in the last years of the Peloponnesian War (Bus.-Swob., *Gr. St.* 899 n. 5 for the evidence).

⁴⁴ Quoted by Andreades, *History of Greek Public Finance* 207.

⁴⁵ *Op. cit.*, p. 16 f.

⁴⁶ Cf. Ferguson, *Hellenistic Athens* 474 f.

⁴⁷ Hammond, *History of Greece* (1959) 531 f. is their latest appearance.

⁴⁸ Cf. Beloch, *op. cit.*, 484 n. 6 for the effect of the law of Hegemon. The law was passed before

335/4 (IG ii² 1628 l. 300); cf. Bus. Swob., *op. cit.*, 1043 n. 1 and see n. 52. Hegemon was an opponent of Demosthenes (Dem. xviii 285) and presumably his law sought to curtail the power of the office in which Demosthenes was established in 337/6.

⁴⁹ Eubulus is connected with *τὰ θεωρικά* and Theoric distributions in Schol. Aesch. iii 25, Theopompus F. 99, Schol. Dem. x 11 (Dind. 203 l. 21), and with the building programme in Schol. Dem. iii 29 (Dind. 133) and Din. i 96, which appears to have been the concern of the Theoric Commission (Aesch. iii 25, Philoch. F. 56a).

⁵⁰ Aesch. iii 25. The Eudoxus honoured in IG ii² 223 B and C by the Council because he *καλῶς καὶ δικαίως ἐπεμελίβη ὥν αὐτῶι ἡ βουλὴ προσ[έταξεν] τῆς τε διοικήσεως τῇ βουλῇ καὶ τῆς εὐκοσμίας μετὰ τῶν πρυτάνεων* was presumably an official concerned with the affairs of the Council alone.

How, then, could such limited tenure afford him the opportunity to establish his influence? It may be that one year sufficed for him to win respect and get his supporters elected to succeed him. It may be that before the law of Hegemon the office could be held repeatedly: Aristotle's description of the 320s⁵¹ does not gainsay this, and it need not surprise us if in the difficult days after the Social War Eubulus constituted in the administration of the city the counterpart of Phocion, constantly general.⁵² But the truth may be rather more than either of these two answers.

The inception of the Theoric Fund has been much discussed and necessarily inconclusively. The evidence conflicts and no one solution is likely to command universal esteem.⁵³ But, even if there were Theoric distributions before Diophantus and Eubulus, it seems that Eubulus made some important change. In commenting on the word *θεωρικά*, Harpocration cited a remark of Philinus who appears to have been an orator in the time of Lycurgus: *Φιλίνος δὲ ἐν τῇ πρὸς Σοφοκλέους καὶ Εὐριπίδου εἰκόνας περὶ Εὐβούλου λέγων φησὶν· ἐκλήθη δὲ θεωρικόν, ὅτι τῶν Διονυσίων ὑπογύων ὄντων διένειμεν Εὐβούλος εἰς τὴν θυσίαν, ἵνα πάντες ἐορτάζωσι καὶ τῆς θεωρίας μηδεὶς ἀπολείπηται δι' ἀσθένειαν τῶν ἰδίων.* Here is an orator within a generation⁵⁴ speaking as if the actions of Eubulus had given rise to the very name, which, moreover, had never occurred in Aristophanes. So, whether Eubulus was the first to distribute money at the festivals or not, he was responsible for the name, and one wonders whether the whole Theoric Commission was his creation also.⁵⁵ At some time in the fourth century the surplus moneys of the administration were used for war and the

⁵¹ *AP* 62.3 ἄρχειν δὲ τὰς μὲν κατὰ πόλεμον ἀρχὰς ἐξεστὶ πλεονάκις, τῶν δ' ἄλλων οὐδεμίαν, πλὴν βουλευσά δις.

⁵² Whether Lewis's suggestion that the law alluded to by Plutarch (*Mor.* 841 C μὴ πλείω πέντε ἐτῶν διέπειν τὸν χειροτονηθέντα ἐπὶ τὰ δημόσια χρήματα) is the law of Hegemon (above, n. 48) is accepted or not, the fact that there was such a law suggests that re-election in some financial office had previously been permitted.

⁵³ Jacoby, *Commentary* on Philochorus F. 33 follows Bus.-Swob., *op. cit.*, 899 and Schwahn *RE* vA.2 col. 2233 f. in treating the Theoric distributions as Periclean—a view mainly deriving not so much from the fact that Philochorus possibly discussed Theoric distributions in Book iii of his *Atthis* (for there are good grounds for amending this numeral to vi, i.e. Γ to Ὶ—*cf.* Jacoby, *loc. cit.*) as from Plutarch's attribution (*Per.* 9) and the entry in Hesychius, s.v. *δραχμὴ χαλαζῶσα* (ἐπὶ Διοφάντων—archon 395/4—τὸ θεωρικὸν ἐγένετο δραχμῇ) implying that there had been Theoric distributions earlier. Kahrstedt (*loc. cit.* in n. 42) argues for their institution after 362, for which he cites Justin vi 9.2. I prefer the late date. The silence of Aristophanes in the *Ecclesiazusae* and the *Plutus* is strong in support of Kahrstedt, and the evidence for an early date is far from cogent; Plut., *Per.* 9 is rather general, the Scholiast to Aeschines iii 24 precise but of questionable authority, and the citation from Philochorus, whose full account would have resolved our doubts, is disquieting. For, when quoting this fragment, Harpocration also said that Agyrrhius began distributions; so either he had not read Philochorus carefully or Philochorus did not say that Pericles began the Theoricon. I prefer the latter. After all, the institution was of great interest

to Aristotle's generation and surely he would have mentioned it if Pericles had begun the distributions. Pericles was the first to provide pay for jurors (*AP* 27.3) and he provided money for all in the form of wages under the building programme as he said in answer to Thucydides, son of Melesias (Plut., *Per.* 12) and this was enough, perhaps, to excite an excursus from Philochorus on *μισθοφορία* and to mislead other less exact writers.

As for Harpocration's remark about Agyrrhius, it seems better to relate it to his introduction of pay for attendance at the Assembly (*AP* 41.3) to which the *Ecclesiazusae* refers so often than to the Theoricon, of which it has not a word. Athens did not have money for such luxuries in the Corinthian war, certainly not ἐπὶ Διοφάντων, 395/4, and for this reason the explanation of the notice in Hesychius given by Beloch (*Att. Pol.* 180 n. 4) is probably the right one, viz. that it relates to the distributions made by Diophantus the Sphettian (*cf.* Schol. Aesch. iii 24).

These are substantially the arguments set out by J. van Ooteghen in *Études Classiques* i (1932) 388, who also followed Kahrstedt in taking Justin vi 9.1 f. as evidence that the distributions began after Mantinea. But from 362 to the law establishing the use of the surpluses for the Theoricon Athens was continually at war and money was probably not available for distributions. It seems therefore better to suppose that the whole institution began in 355 or soon after. But this view is not essential to the argument developed in the text.

⁵⁴ For Philinus see *RE* xix 2 col. 2175 f.

⁵⁵ Even those who date the institution of the Theoricon to the fifth century do not necessarily suppose that the Commission pre-dates Eubulus. *Cp.* Schwahn, *loc. cit.*

likely date for such a system to begin would be 378, if not earlier.⁵⁶ From then down to the end of the Social War Athens was hardly ever at peace, and so if there was any money at all distributed it must have been provided for in the diataxis and is unlikely to have been large. If officials existed to distribute moneys they must have been very minor. Yet in Aristotle's day, when the power of οἱ ἐπὶ τὸ θεωρικόν had been lessened by the law of Hegemon, they were elected like the ταμίας τῶν στρατιωτικῶν and the ἐπιμελητής τῶν κρηνῶν and no one else in the civil administration (AP 43.1). Whence did they acquire this dignity? It seems reasonable to suppose that Eubulus was responsible and that the Theoric Commission as we meet it was his creation and to that in part Aeschines referred when he said (iii 25) διὰ τὴν πρὸς Εὐβουλον γενομένην πίστιν ὑμῖν οἱ ἐπὶ τὸ θεωρικόν κεχειροτονημένοι ἦρχον --- κ.τ.λ. That is, the Theoric Commission was the instrument created by Eubulus for the financial administration of Athens. This is perhaps little more than a guess, but the ἐπιμελητής τῶν κρηνῶν is suggestive. It is curious that such a minor official was elected in the 320s (AP 43.1). He hardly appears in the evidence but it is striking that among the public works that Demosthenes scoffs at he twice mentions κρήναι (iii 29, xiii 30). Perhaps the elected Superintendent was also the creation of Eubulus.⁵⁷ A further indication that the Commission was made an important public office by Eubulus may perhaps lie in the strange demand made in Xenophon's *Revenues* (v 1) for the establishment of εἰρηνοφύλακες—πολὺ γὰρ αὖ καὶ αὕτη αἰρεθεῖσα ἢ ἀρχὴ προσφιλεστέραν καὶ οἰκιοτέραν εἰσαφικνεῖσθαι πᾶσιν ἀνθρώποις ποιήσῃ τὴν πόλιν, i.e. a demand for officials concerned to make the city more attractive to trade. Thiel⁵⁸ explained these εἰρηνοφύλακες thus: 'Collegium, opinor, intelligitur quod controversias inter Athenienses aliasque civitates Graecas ortas componere studens bello occurrere conetur.' But this falls flat since there is no reason to suppose that trade disputes were a serious menace to peace nor was arbitration of this sort unprovided for in Athens (AP 59.6, etc.). Another explanation may be suggested. There is only one other known instance of the word and that is in Aeschines (iii 159) where he says that Demosthenes, on his return to Athens from his excursion after Chaeronea to gather corn and money, παριὼν ἡμιθνής ἐπὶ τὸ βῆμα, εἰρηνοφύλακα ὑμᾶς αὐτὸν ἐκέλευε χειροτονεῖν. Of course this may be mere sarcastic abuse, but it seems natural to connect it with the position Demosthenes held after Chaeronea. Of the two offices attributed to him, the post of τεichoποῖός seems ruled out of consideration, for it was probably not elective (cp. AP 43.1) and Demosthenes' work on the walls was hardly indicative of timidity towards the Macedonians nor does he in fact appear to have actually been τεichoποῖός (Aesch. iii 28). I guess therefore that Aeschines referred to his election ἐπὶ τὸ θεωρικόν for 337/6, and that he chose as a sneer the word used twenty years earlier by Xenophon in proposing a new office to make the most of the peace. Here perhaps as elsewhere in the *Revenues* what Xenophon proposed, Eubulus enacted.

The effect of all this is to replace Motzki's Finance Minister with a Theoric Commission which fulfilled a comparable function. The moneys handled appear to have been considerable. The amounts distributed, as I have already remarked, were normally not large, but there is no reason to suppose that the Commission controlled no more than it distributed.⁵⁹ In 353/2, the probable date of the speech *On the Syntaxis* (see note 9), which was delivered during a debate on the distribution of money (1), the surpluses⁶⁰ were no doubt small (3) for the city was still picking up after the Social War, and likewise in 351

⁵⁶ This is discussed in my article 'Demosthenes and the Stratotic Fund' *Mnemosyne* 1962 377 ff.

⁵⁷ In the drought of 361 wells dried up ([Dem.] I. 61). In 357/6 there was a σιτοδεία παρὰ πᾶσιν ἀνθρώποις (Dem. xx 33), perhaps due to drought. The water supply of Athens may have needed special attention in 355.

⁵⁸ In his commentary on this passage.

⁵⁹ Jones, *op. cit.*, 33 f. appears to assume this in belittling the importance of the Theoricon.

⁶⁰ For a statement of how the surpluses arose see Francotte, *Les finances* 151 f.

when Demosthenes wanted ninety-two talents for his standing force in the war for Amphipolis (Dem. iv 28 f.), he probably had nothing to say about the Theoric Fund because it was hardly large enough to matter, but by 349 the position had changed sufficiently for him to look to the Fund to finance the expedition to Olynthus (Dem. i 19 f., iii 10 f.). Demosthenes was presumably not weakening his case by a piece of downright silliness. So by that year there had begun to be considerable amounts in the Theoric Fund. Consonant with this is the important role played by the Commission in public works. In the effort to prove that Demosthenes had rendered himself liable to *εὐθυναί* as a *τειχοποιός*, Aeschines (iii 25) may have gone further in his description of the Commission's function than was justified; the *ἀποδέκται*,⁶¹ for instance, and the *ἐπιμεληταὶ τῶν νεωρίων*⁶² continued to function. But what he said was, in general, true enough: the Theoric Commission clearly had a major part in matters quite unconnected with mere distributions of money.⁶³ Harpocration, s.v. 'Θεωρικά,' says ταῦτα - - - ὕστερον - - - κατετίθετο εἰς τε τὰς δημοσίας κατασκευὰς καὶ διανομὰς τῶν πολιτῶν. Philochorus says that when in 339/8 the Athenians suspended work on the docks and the Skeuothekē, at the same time τὰ χρήματα ἐψηφίσαντο πάντ' εἶναι στρατιωτικά: that is, the Theoric Fund was to be used for war, having been used till then on the buildings (*cf.* Schol. Aesch. iii 24). Indeed the varied activities of the Theoric Commission mentioned by Aeschines are probably demonstrated by Demosthenes' acts when he was a Commissioner; at any rate his work on the walls was certainly not undertaken by him in the capacity of *τειχοποιός*.⁶⁴ This evidence is of course imprecise, but it is sufficient to suggest that under Eubulus the Commission had wide responsibilities, that he financed his work on the battlements from the Theoric Fund⁶⁵ just as Demosthenes was later to use ten talents ἐκ τῆς διοικήσεως for work on the walls (Aesch. iii 31), that the Commissioners in some way were involved in the province of the *ἐπιμεληταὶ τῶν νεωρίων* (and it is no surprise to find that Eubulus had bought a quantity of wood for work in the yards (*IG* ii² 1627 l. 352, etc.) or that Dinarchus (i 96) could speak of triremes being built ἐπὶ Εὐβούλου), and that in addition to managing both important constructions, such as the docks and the Skeuothekē, and minor public works, such as the repair of roads, they exercised a general supervision over finance. If to such a Commission re-election were permitted, Eubulus could have directly controlled the whole financial administration of the city.

⁶¹ They were functioning in 346 at any rate (*IG* ii² 212 l. 43).

⁶² *IG* ii² 1621 is a navy-list of the early 340s of the normal sort. *IG* ii² 1622 of 342/1 is exceptional in form and Mr D. M. Lewis argues that it was drawn up not by the *ἐπιμεληταὶ τῶν νεωρίων* but by some other body and, along with other evidence, indicates that the *ἐπιμεληταὶ* had been temporarily superseded. Discussion of this view must wait until he has published it, but to my mind the onus of proof will be very much on those who claim that this is not a document of the *ἐπιμεληταὶ τῶν νεωρίων*; for no matter how much the Theoric Commission interfered in the finances of the navy, the routine work of the docks had to go on and this probably required a board of officials concerned solely with it. That is, the Theoric Commission could only control, not supersede, and, until cogent arguments to the contrary appear, it is reasonable to suppose that the *ἐπιμεληταὶ τῶν νεωρίων* continued to function in the late as in the early 340s.

⁶³ The supersession of the *ἀντιγραφεὺς* has often been remarked. He is missing from *IG* ii² 223 C

(of 343/2) where one would have expected to find him and Aesch. iii 25 says that before the law of Hegemon his functions were exercised by the Theoric Commission: presumably he was superseded. *Cf.* Bus.-Swob., *op. cit.*, 1043 n. 1. The occurrence of an *antigraphus* in *IG* ii² 244 l. 23 does not argue against this view; the date of this inscription is often given as 337/6 but it may well belong to the 350s (below, n. 109). Nor is the value of Aeschines' remark to be impugned by pointing out that he speaks as if the *ἀντιγραφεὺς* did not exist in 330 when *IG* ii² 1700 l. 217 shows that he did exist in 335/4; for after the law of Hegemon the *ἀντιγραφεὺς*, though he existed, was not important and Aristotle has nothing to say about him in the *Ath. Pol.* (but see Jacoby ad Philoch. F. 198); he was just no longer *χειροτονητός* τῇ πόλει κ.τ.λ.

In this note I am especially indebted to Mr D. M. Lewis.

⁶⁴ Aesch. iii 27 f., esp. 28 where Aeschines tries to deal with the imagined objection of Demosthenes ὥς οὐτ' ἔλαχε *τειχοποιός* οὐτ' ἐχειροτονήθη ἐπὶ τοῦ δήμου.

⁶⁵ Schol. Dem. iii 29 (= Dind. viii 133).

If Eubulus created, or radically reorganised, the distribution of money after the Social War, where does Diophantus' work of distribution belong? The scholiast to Aeschines iii 24 says . . . πολλά ἅμα χρήματα διενείμαντο ἐπὶ τῇ τοῦ θεωρικοῦ προφάσει, τὰ μὲν Διοφάντου, τὰ δὲ Εὐβούλου διανέμεντος, and the relation of the two in finance as elsewhere needs to be considered. Kahrstedt, taking Justin vi 9.1 more precisely than it deserves, argued that the Theoric distribution began after Mantinea and if this were so Diophantus' distributions might come before the Social War. But this would raise difficulties. For if there was a new Common Peace with Thebes in 362, there was war elsewhere as Demosthenes I and xxiii testify and presumably the surpluses were used for the war, not for distribution. More probably, Diophantus played his part after the Social War.⁶⁶ The Theoric Fund and Commission were established in stages; Demosthenes (iii 11) speaks of laws. In the first stage, Eubulus might still be subordinate in influence or equal and Diophantus might have played a part financially before he was eclipsed by his associate.⁶⁷

The Theoric Commission presided in effect over the κοινὴ διοίκησις, as Aeschines (ii 149) termed it, using τὰ θεωρικά as seemed best.⁶⁸ It was essential that their moneys should be safe from rash and ill-considered decrees, and so Eubulus took steps to secure this. It is evident from the *Olynthiacs* that it was not possible in 349 to use Theoric moneys for war without repealing the laws concerning the Theoric Fund. In the first of these speeches (19) he said that either Theoric money should be used to finance help from Olynthus or there would have to be εἰσφοραί, but he shrank from moving a decree. 'Τί οὖν; ἂν τις εἴποι, 'σὺ γράφεις ταῦτ' εἶναι στρατιωτικά;' μὰ Δί' οὐκ ἔγωγε. Why not, unless he feared the law? In the *Third* (10 f.), which, whenever precisely it was delivered, was certainly delivered before the Euboean expedition of February 348 and the celebrated decree of Apollodorus ([Dem.] lix 4),⁶⁹ Demosthenes openly (σαφῶς οὕτως) attacked the laws governing the Theoric Fund and demanded their repeal by those who had proposed them: that was the way to finance the expedition. These passages make clear that the laws establishing the Theoric Fund had precluded its use for war and the history of Apollodorus' attempt bears this out. He did not propose directly that τὰ περιόντα χρήματα τῆς διοικήσεως should be στρατιωτικά but called for a special vote (διαχειροτονήσαι⁷⁰ τὸν δῆμον εἴτε δοκεῖ τὰ περιόντα χρήματα τῆς διοικήσεως στρατιωτικά εἶναι εἴτε θεωρικά). The decree was passed and the people voted as he desired (5). Yet, having gained the approval of the people in this way, he was prosecuted under the γραφή παρανόμων and fined. The speaker of [Dem.] lix would have it thought that Apollodorus was convicted on a mere technicality, viz. ὡς ὥφλε τῷ δημοσίῳ ἐκ πέντε καὶ εἴκοσι ἐτών,⁷¹ and other irrelevancies, but this should not deceive us.

⁶⁶ Cp. Dem. xx 137 for both Diophantus and Eubulus busying themselves with finance at the same time.

⁶⁷ Some confirmation of this is to be found in Ar., *Pol.* 1267 B 18—ἀλλ' εἴπερ δεῖ δημοσίους εἶναι τοὺς τὰ κοινὰ ἐργαζομένους, δεῖ - - ὡς Διοφάντος ποτε κατεσκεύαζεν Ἀθήνησι, τοῦτον ἔχειν τὸν τρόπον. When did he provide the city with these δημοσίοι τεχνῖται? Xenophon in the *Revenues* makes no mention of them despite his advocacy of the idea of having them. So probably Diophantos did it after the treatise was written, perhaps even as a result of it.

⁶⁸ Mr D. M. Lewis's article will establish as a basis for future discussion of Lycurgus' position that the man who was elected [ἐ]πὶ τὴν διόλησιν τῶν αὐτοῦ ἁπασαν [ταμ]ίας in Hyper. v 28 was Demosthenes as Theoric Commissioner.

⁶⁹ Cp. 'The Defence of Olynthus' CQ xii (1962) 130 f. Part iii following Kahrstedt, *Forschungen* 61, on the dating of the *Third Olynthiac*.

⁷⁰ Decision by διαχειροτονία on some minor matters was normal, and possible in major questions, e.g. IG ii² 28. (Cp. Bus-Swob., *op. cit.*, 1000 n. 4.) So no inference can be drawn from its use in 348, though it may well have been that this somewhat unusual procedure stemmed from fear of the consequences under the laws governing the Theoric Fund.

⁷¹ These words came in the manuscripts in 9 where they are 'alienissima', and were inserted by Sauppe in 5 where the scribe of S has marked a lacuna in the middle of the paragraph. It seems to be the custom of most orators of the period to give corroborative detail. So towards the end of 5 some amplification of γενεαί; μάρτυρας might be expected;

Any prominent political figure convicted by an Athenian court could say this sort of thing, for any allegation or slander sufficed for an Athenian legal speech, and he could be unfairly convicted on the strength of it; but Apollodorus was probably not in this plight. For, even if he had been convicted on a mere technicality, no one else dared to follow up his attack on the laws, just as Demosthenes had not dared although he believed the ending of the laws was essential to Athens' safety. The only satisfactory explanation is that Demosthenes dared not brave the law, but that in the critical days when Phocion's army was in peril in Euboea Apollodorus did dare and had to pay for it.⁷²

Furthermore, the speech *On the Syntaxis* shows that the law was in force by 353/2. It speaks of those who distribute and give away τὰ κοινά (1); the income of the city is being squandered ἐς οὐδέν δέον (4), presumably on 'repairs to roads and wells and stucco walls and trash' (30), when there should be adequate preparation πρὸς τὸν πόλεμον (3).⁷³ But never a word of direct proposal to stop it all. The law of Eubulus had curtailed the right to γράφειν.

There would be no need to make this explicit, if the subject had not been befogged over the last century by false theory. The confusion really arose because for long the Euboean expedition was dated before the Olynthian war, and so Schaefer was led to accept the account of the Scholiast on the *First Olynthiac*.⁷⁴ Weil and Radüge set the date of the Euboean expedition aright,⁷⁵ but the fantasy of the Scholiast lingers on. Here is in essence what he says (ad Dem. i 1 = Dindorf Vol. viii 32 and 33).

1. The Athenians, χρήματα ἔχοντες στρατιωτικά have lately (ἐναγκος) made them θεωρικά.

2. Demosthenes wished ταῦτα μεταβαλεῖν εἰς στρατιωτικά, ἐπειδὴ περ νῦν κατέλαβεν ὁ πόλεμος ὁ πρὸς Φίλιππον, but he approached the matter cautiously.

3. Digression on the history of Theoric distributions

(a) Pericles ἔγραψε τὰ προσοδευόμενα χρήματα τῇ πόλει γενέσθαι πᾶσι θεωρικὰ τοῖς πολίταις.

(b) εἶτα ἐπιχειρήσαντος Ἀπολλοδώρου τινὸς πάλιν αὐτὰ ποιῆσαι στρατιωτικά, - - - Εὐβουλος ἔγραψε νόμον τὸν κελεύοντα θανάτῳ ζημιουῖσθαι εἴ τις ἐπιχειροίη μεταποιεῖν τὰ θεωρικὰ στρατιωτικά.

4. Διὸ ὁσάκις ἐν τοῖς Φιλιππικοῖς μέμνηται αὐτῶν ὁ Δημοσθένης, συμβουλεύει μόνον ὥστε αὐτὸν λυθῆναι, οὐ μέντοι καὶ ἐγγράφως λέγει, ὅπερ ἦν ἐπικίνδυνον.

There are here three serious errors, serious, that is, for the Scholiast's credit.

(i) He thinks that the war against Philip has just (νῦν) begun. The war for Amphipolis, as Isocrates (*To Philip* 2) terms the war against Philip, began long before 349. If no other proof of hostilities existed, those recorded in Polyaeus iv 2.22 would suffice, but there is ample evidence that Athens considered herself at war after making peace with the allies in

in 9, even without the words moved by Sauppe, αἰτίαν ψευδῆ is fully amplified. Possibly the false accusation from 5 was written in the margin of 9 as a parallel, and got incorporated later.

This note repeats in substance the opinion of Mr A. N. Bryan-Brown who concludes that 'if a place has to be found for the vagabond clause, Sauppe has probably found the best, but that more than probability is not possible'.

⁷² Francotte, *Les finances* 219 f. long ago clearly, if briefly, stated the view, that it was the Theoric laws which checked Demosthenes.

⁷³ What precisely was the subject of the debate to which this speech belonged? Not the institution of the restriction on the use of the surpluses, for that is presupposed in the speech, but presumably some enlargement of the sphere of influence of the Theoric Commission or the introduction of some law like IG ii² 244.

⁷⁴ *Op. cit.*, 208. Motzki, *op. cit.*, 75 and Pickard-Cambridge, *Demosthenes* 127 reject the Scholiast.

⁷⁵ H. Weil, *Revue de Philologie* N.S. iii (1879) and E. Radüge, *Zur Zeitbestimmung des euböischen und olynthischen Kriegen* (1908).

355,⁷⁶ and the only war in question is the war for Amphipolis which Philip held from 357 onwards. *Nūn* indeed!

(ii) He thinks that such a decree as he attributes to Pericles was possible in fifth-century Athens. Pericles may have moved a decree about the surpluses as Callias did in the first Callias Decree (ἐς τὸ νεόριον καὶ τὰ τείχε τοῖς περιόσι χρῆσθαι χρήμασ[ιν]), but in the terms of the Scholiast never. Perhaps he was only writing loosely, but one suspects his grip of the subject.

(iii) He thinks that the motion of Apollodorus preceded the *Olynthiacs*. (The reference to what Demosthenes said ἐν τοῖς Φιλιππικοῖς makes clear that the *Olynthiacs* were meant.) This is the very reverse of the truth⁷⁷ and devastating for the Scholiast, because he thinks that the allusions of the *Olynthiacs* are to be understood by reference to a law passed after Apollodorus.

So what value are we to give to the assertion that Eubulus got a law passed imposing the death penalty after Apollodorus' decree? One suspects that the Scholiast knew very little of the subject beyond what he read, or rather misread, in Demosthenes. I submit that his evidence is worthless, but, if any would cling to it, they must rebut this dilemma. Either there was no law before Apollodorus in which case the cautious allusions of the *Olynthiacs* and of the speech *On the Syntaxis* require explanation, or there was one law which from 353 to 349/8 inhibited Demosthenes and another law which was passed in 348 merely increasing the penalty, i.e. the law mentioned by the Scholiast, in which case this second law would be trivial. Until this dilemma is rebutted, the Scholiast should be treated as ill-informed and misleading, and this theory of a law in 348 should be renounced.

The root of the trouble, perhaps, for the Scholiast and his followers lies in a misleading remark made by the speaker of [Dem.] lix about the decree proposed by Apollodorus. The claim is made (4) that, when Apollodorus proposed that the People should decide by special vote whether the surpluses of the administration should be used for military purposes or for Theoric, the laws actually required that in time of war these surpluses should be used for the former (κελευόντων τῶν νόμων, ὅταν πόλεμος ᾖ, τὰ περιόντα χρήματα τῆς διοικήσεως στρατιωτικὰ εἶναι). For two reasons this claim simply cannot be taken as a plain statement of fact. First, there was war against Philip from 357 onwards, and if the law had stood as the speaker claimed, there could have been no Theoric distributions at all in this period. Nor can it be claimed that the war in Euboea was a war within the definition of the law when the war in the north was not; for neither is there any ground in the evidence for such an hypothesis,⁷⁸ nor did these alleged laws come into effect when the Euboean expedition began, for an appreciable period of time had elapsed before Apollodorus made his proposal.⁷⁹ Athens was and had long been at war; so the speaker is seeking to mislead. Secondly, if the laws had ordered what is claimed, Demosthenes would have been perfectly free to propose what he in fact so circumspectly advocated but did not propose. The laws cannot have been in 349 what the speaker claimed. Yet some explanation of the speaker's remark is due. He may have been simply lying, but that seems less probable than that there is some shadow of the truth in what he said. The explanation that suggests itself is that he was describing the state of the laws before the Theoric laws came into force. As I have argued elsewhere,⁸⁰ it seems reasonable to suppose that there was from the inception

⁷⁶ Xen. *Revenues* v 12 ἐπεὶ δὲ εἰρήνη κατὰ θάλατταν γέγνηται . . . i.e. war remains elsewhere; Dem. xxiv 95 τοῖς τοῦ πολέμου καιροῖς; Dem. xxiii 107-9; SIG³ 196 l. 40; and above all the whole *First Philippic*.

⁷⁷ Some have put the *Third Olynthiac* after Apollodorus' motion (e.g. Pokorný, *Studien* 118 f.) but no one, to my knowledge, since Radūge, *op. cit.*, set the chronology right, has attempted to post-date the *First*.

⁷⁸ Is it conceivable in view of Athens' bitter experience of what could happen as the result of military operations in the Hellespont that she should not regard a war that constantly threatened the Chersonese as not really a war?

⁷⁹ For chronology of the Euboean war see 'The Defence of Olynthus' CQ xii (1962) 127 f.

⁸⁰ 'Demosthenes and the Stratotic Fund' *Mnemosyne* 1962 377 ff.

of the Second Athenian Confederacy a Stratotic Fund from which the war from 378 on was financed, and that this fund was partly supplied, to adapt our speaker's words, by τὰ περιόντα χρήματα τῆς διοικήσεως, ὅταν πόλεμος ᾖ. If this is right, the laws of Eubulus superseded the financial system of Callistratus, but the speaker of [Dem.] lix, to deceive his jury, was referring to the laws before 355. He may have deceived his jury as he appears to have deceived the Scholiast. Historians should be less gullible.

Another source of confusion about Eubulus' laws is Philochorus' account (F. 56a) of how the Theoric moneys came to be used for war. He recorded for 339/8: *Λυσιστρατίδης Ἀχαρνεύς· ἐπὶ τούτου τὰ μὲν ἔργα τὰ περὶ τοὺς νεωσοίκους καὶ τὴν σκευοθήκην ἀνεβάλλοντο διὰ τὸν πόλεμον τὸν πρὸς Φίλιππον, τὰ δὲ χρήματα ἐψηφίσαντο πάντ' εἶναι στρατιωτικά, Δημοσθένους γράψαντος.* One might think that, if Demosthenes could so propose and the People decree in 339/8, they could have done the same in 349. But this would be bad argument. Between the rise of Demosthenes to power in 343 and the decree of 339/8, the Theoric laws may have been modified. Indeed, one might guess the moment. From 35 to 45 of the *Fourth Philippic* it seems that by 341 no change in the laws had yet been made or was contemplated. In 340/39 Philip declared war. Why was the change not made then? It is hardly likely that Athens was prosperous enough to begin the war and continue, as Pericles a century before had not felt able, to carry out the building programme and use the surpluses for other than military purposes. Since the change was delayed till 339/8, it may be suggested that the Theoric laws had not been changed by the time the war began, and that the revision of the laws in the first prytany of 339/8 was a necessary preliminary to Demosthenes' decree. If Jacoby is right in maintaining⁸¹ that a citation from Philochorus which begins, as this does, ἐπὶ τούτου . . . is his first entry for the year, the decree of Demosthenes in 339 came soon after the start of the year, close upon, perhaps, the repeal of the relevant Theoric law.⁸² Yet, whatever is the truth of this, it should be clear that there is no difficulty in explaining why Demosthenes could do by decree in 339/8 what he could not ten years earlier.

To sum up this part of the argument. I have argued in support of the view that between the end of the Social War and 353/2, the date of the speech *On the Syntax*, Eubulus established, perhaps by stages, a Theoric Commission with a dominant interest in the working of the Athenian financial system, and supplied it with a fund of which a small amount was distributed to the People and the rest used for various public projects, but which could not be used for war before the law was changed.

It may be useful briefly to survey Eubulus' achievement in the economic field, although there is little to add to previous discussions.⁸³ There were two sides to Eubulus' activity. First, he was concerned, as Xenophon was in the *Revenues*, to increase the income of the State and it is possible in general terms to follow the return of Athens' finances to a well-balanced condition. Secondly, but not of secondary importance, was the increase in general prosperity whereby individuals were in a better position to meet the obligations of trierarchies and other public services.⁸⁴ This is not directly attested, but the increase of revenue is an index and must suffice.

In 355 the annual revenue had dropped to a mere 130 talents as Demosthenes says in a passage that seems to relate to the end of the Social War,⁸⁵ and, consonant with this,

⁸¹ *Commentary on Philoch.* F. 157 p. 532.

⁸² For the procedure of νομοθεσία see Hignett, *History of the Athenian Constitution* 299 f.

⁸³ Cp. Motzki, *op. cit.*, 79 f.

⁸⁴ Perhaps Demosthenes' trierarchic law (Dem. xviii 102, etc.) reflects an increase in individual

fortunes which had made reform of the law of Pericles necessary.

⁸⁵ x 37. There has been general concord in referring this passage to the end of the Social War but it may be worth while to set out the grounds for doing so since Didymus comments (8.49) εἴη ἂν

the evidence of the period of the Social War suggests that efforts were made to scrape up money from all possible sources.⁸⁶ Isocrates' *On the Peace* is eloquent testimony to the impoverishment of the city⁸⁷ and Xenophon's *Revenues* is revealing both in general and in detail; for instance, it emerges (vi 1) that officials were not being paid and that there were many vacant houses and sites within the city itself (ii 6). By 346 the revenues had risen to 400 talents a year (Theopompus F. 166). The rate of progress can be seen, as I have already remarked, in the contrast between the financial proposals of Demosthenes in 351 and 349 (see note 60). Since Demosthenes did not demand in 351 the use of the *Theorika*, the surpluses were presumably still small and indeed in the *Aristocrates* (209) he remarked that there was not a day's travelling expenses *ἐν τῷ κοινῷ*, but two years later he looked to the surpluses to finance the defence of Olynthus. Thus by 346 the Athenian economy had gone a long way towards recovery⁸⁸ and only in 348 was there a temporary crisis, when Phocion's citizen army in Euboea was held longer than had been expected. During this πόλεμος δαπανηρός (Dem. v 5) it looked for a while as if the *Theoric* laws would have to be repealed and Apollodorus' proposal was at first well received ([Dem.] lix 4, 5). But the war did not interrupt Athens' steady recovery. The statement of the speaker of [Dem.] xlix

οὗτος ὁ καιρὸς, ἐν οἷ περ[ὶ] Αἰγὸς ποταμὸν ἡττηθέντες ἐτα[π]εινώθησαν καὶ εἰς βραχὺ ὁ δῆμος συνεστάλη τῶν ἐξω[τ]ρικῶν προ[σ]όδων περικοπεισῶν [.σ]φρὲς δὲ τοῦτ[ο] ποιήσει followed by a gap twice as long which must have contained the grounds for his statement. My reasons for differing from Didymus are as follows:

(a) A study of all instances of *πάλαι* in the genuine speeches of Demosthenes shows that it is unlikely that he would have used *οὐ πάλαι* to refer to the end of the Peloponnesian War. Indeed, he normally used *πάλαι* to refer to fairly recent events although he of course used it for events as early as 403 and twice for events still earlier. *Οὐ πάλαι* (where the two go closely together, as here in x 37) he used to mean *ἐπ' ἐμοῦ*. Cp. iii 2 καὶ μάλ' ἀκριβῶς οἶδα· ἐπ' ἐμοῦ γὰρ, οὐ πάλαι γέγονεν --. At xv 2 οὐ πάλαι is used in 351 to refer to the Social War. There is one case where a longer period is involved, xxiv 138 οὐ πάλαι ἀλλ' ἐπ' Εὐάνδρον ἄρχοντος, i.e. in 353/2 of 382/1, but the point remains that by οὐ πάλαι he means in the life-time of either himself or his audience. Cp. xviii 100 ἐξόδους καὶ πάλαι γεγονέναι καὶ νῦν ἐφ' ἡμῶν αὐτῶν. In 341 comparatively few would be present in the assembly with lively memories of 405. Such a date for Demosthenes would be *πάλαι*, certainly not *οὐ πάλαι*, and more certainly still, not *οὐ πάλαι* with *ἐποιοῦμεν*.

(b) When Demosthenes alluded to events before his own life-time, it was his custom, where he did not cite documentary evidence as at, e.g., xix 276, and xx 77, to avoid seeming to talk authoritatively about things beyond his direct knowledge by using phrases such as 'I have heard' or 'you all know', e.g. xx 11, 52, 68, 73. I have collected 15 examples. Sometimes he alludes directly to events before his life-time—ix 23, xiii 21 f., xviii 96, xix 191 and 263, xx 59 and 60 are the cases I have collected. With some of these, for various reasons, his apologetic manner would have been either unnecessary or inappropriate but the important point is that in

none of them does he use the first person, as *ἐποιοῦμεν* here. So the absence of his customary introduction together with the use of the first person combine to show that he is speaking of his own life-time.

(c) *Τριήρεις ἔπλεον* -- πάντ' ἐποιοῦμεν τὰ δέοντα come suitably from the volunteer trierarch of 357 (Dem. xxi 161, etc.), but are quite inappropriate to the period after Aegospotami when Athens was first blockaded and then deprived of ships.

I apologise for labouring what has seemed to many obvious, but teaching experience has shown that it is not always so.

⁸⁶ For the decemviral commission to collect arrears of *εἰσφορά* see Dem. xx 1, 42, 44, 48 f.; xxiv 8, 11 f., 160–75, 197. For similar activities by the *ἐπιμεληταὶ τῶν νεορίων* see Dem. xxii 63. By the end of the Social War the State was virtually bankrupt (cp. Dem. xx 24, 115; xxiii 209) and the national festivals were in jeopardy—hence Aristophanes' commission (Dem. xxiv 11) and the law of Timocrates.

⁸⁷ Cp. 19, etc. Agriculture and overseas trade were depressed, as were ἄλλαι ἐργασίαι, αἱ νῦν διὰ τὸν πόλεμον ἐκτελοῦσιν (20) and traders and aliens, resident and non-resident alike, had left the city (21). Most significant is Isocrates' proposal to plant colonies in Thrace (24). In the *Panegyricus* his hope had been that the poverty of Greece could be cured by occupying part of the Persian Empire (166), and in the letter *To Philip* the same idea recurs (5) as in the *Panathenaiscus* (14). That is, like other Panhellenists (cp. Xen., *Anab.* iii 2.25 f.), he looked forward to the colonies of Alexander as the cure for Greece's economic ills, but in 355 the condition of Athens was such that he for the moment was prepared to settle Thrace and yield to the Persian ultimatum.

⁸⁸ There is no evidence of what the city's revenue had been in the first half of the century but probably in 346 it was higher than it had been at any time since the prosperity of the fifth century.

that there was not enough pay for a jury in the latter part of 349/8 (17) is not to be taken too seriously: there always was a temporary shortage in the latter part of the year until the taxes due in the tenth prytany were paid (Dem. xxiv 98; *AP* 47.4). Only an invasion of Greece in 346 would have interrupted the process of recovery, and by that time prosperity was such that larger building work had been begun: no doubt more than the ten talent eisphora from the metics was devoted to the Docks and the Skeuothekē (*IG* ii² 505 l. 12).

After 346 things improved still more and Demosthenes frequently referred to the healthy state of the city's finances, though of course he will not concede the credit to Eubulus. In 343 there were, he remarked (xix 89), and would be, *διὰ τὴν εἰρήνην* surpluses. In 341 on three occasions (viii 45, ix 40, x 37) he commented on the abundance of money due to *ἡ τύχη καλῶς ποιοῦσα*. Gone are the complaints of the earlier period. The *Fourth Philippic* is most suggestive: Demosthenes had no longer to denounce the Theoric Fund as an intolerable scandal that had to be abolished if the city was adequately to prepare for the war that had virtually begun. For long the remarks in this speech about the Theorica were taken as proof that the speech was not by Demosthenes: how could he have changed his mind in this way? But the truth is that he was less of a model of constancy to his contemporaries than to his latter-day admirers, and *ἡ τύχη ποιοῦσα* had changed his opinion along with the city's finances. The much abused patching and plastering and *λῆροι* of Eubulus had turned out well.

The methods by which Eubulus achieved all this are obscure, and almost all that is possible is to conjecture on the basis of the proposals of Xenophon's *Revenues*.⁸⁹ The right date for that work is immediately after the peace which concluded the Social War,⁹⁰ and it has been generally recognised as a guide to the sort of measures instituted by Eubulus.⁹¹ I have already suggested that Xenophon's demand for a board of *εἰρηνοφύλακες* (v 1) was met by the creation of the Theoric Commission, but it would be absurd to suppose that every proposal of Xenophon was acted on by the politician. Much of the treatise is fantastic. One leading idea seems to be that if the sources of revenue were fully developed it would be possible for every citizen to draw three obols a day,⁹² i.e. in a year a matter of 500 talents for 20,000 citizens: no doubt Eubulus was more hard-headed, and there is no evidence

⁸⁹ For the relation of Xenophon and Eubulus the commonly cited fragment of Istros (*FGH* 334 F. 33), which says that Xenophon was exiled and recalled by decrees moved by the same Eubulus, is of no importance whatsoever: if it is correct, it cannot have been our Eubulus, for the early 390s is too early for him, and the common assumption that Xenophon was recalled about 370 is without foundation—a better case could perhaps be made out for 387/6. The real reasons for supposing that Xenophon and Eubulus were associated are first that the *Revenues*, like Isocrates' *On the Peace*, with which it has strong affinities (*cp.* Momigliano, *loc. cit.*, n. 38), provides a theoretical exposition of the advantages of the policy actually pursued by Eubulus, and secondly that the parallels between the proposals of Xenophon and the brief account of Dinarchus (i 96) are too strong to be mere chance (*cp.* Thiel, *op. cit.*, xxiii f.).

⁹⁰ In *JHS* lxxv (1955) 76 B.R.I. Sealey sought to revive the long unfashionable view that the *Revenues* is to be dated after the Peace of Philocrates. He based this on the statement in v 12 that 'since there has been peace on sea, the city's revenues have been increased'. But this is, strictly, heresy. The whole work is concerned with Athens' impoverish-

ment and how to overcome it and by 346 the city was no longer impoverished. Thus to dwell on the statement at v 12 is to exalt it at the expense of the whole. Thiel, *op. cit.*, viii f. rightly and firmly placed the work at the end of the Social War. There is *εἰρήνη κατὰ θάλατταν* (i.e. with the allies)—and war elsewhere presumably (the North as well as the Sacred War, v 9)—but the peace is very recent (*cp.* *ἐν τῷ νῦν πολέμῳ* iv 40 and see Thiel, viii); it is not clear that the statement about the increase of revenues could not be made in 355, even if it suited 346 better—which to my mind it does not.

⁹¹ Schwahn, *Rh. Mus.* lxxx (1931) argued that Eubulus was actually the author, and was answered by Wilhelm, *Wiener Studien* lii (1934) 18 f. Thiel's demonstration that the work is Xenophonic (*op. cit.*, xiii f.) stands.

⁹² He means to buy up to three times as many slaves as citizens (iv 17), from *εἰσφοραί* (iii 7), for a return of 3 obols a day per citizen (iii 9 and 10)—so there would be *ικανὴν πᾶσιν Ἀθηναίοις τροφήν ἀπὸ κοινοῦ* (iv 33), an affluent society! Wilhelm, *loc. cit.*, denied that Xenophon had in mind direct distributions and that *τροφή* would only come as payment for performance of duties. iii 9 and 10 prove him wrong.

that his distributions were ever more than a small bonus; if *τροφή* was to be got, it would have to be by work on the building programme as in the days of Pericles. Nor is it likely that Eubulus shared Xenophon's optimism about the unlimited resources of the mines (iv 1 f.), even if he was unaware of the economic effects of flooding the market with silver. Nor could the proposal that the State should keep merchant ships for hire (iii 14) have much attracted anyone with practical experience of the difficulties of maintaining the fleet in good shape. All this was naïve and deserved Boeckh's strictures.⁹³ There are in the work, however, three lines of thought sound in themselves and, as far as can be judged, followed by Eubulus. These are the increase of the total derived from the tax on metics, the encouragement of trade and traders, and capital investment to stimulate the economy.

According to Isocrates (viii 21) at the end of the Social War Athens had been deserted by traders and aliens, both resident (*μέτοικοι*) and visiting (*ξένοι*), and Xenophon (ii 1-7) made proposals for bringing the metics back. As far as we know, some of these proposals (the removal of certain *ἀτιμίαι*, the grant of the right to join the Knights, exemption from hoplite service) were not implemented but, since the proceeds of the *μετοίκιον* could be substantial,⁹⁴ it is likely that something was done, and a hint is to be found in Xenophon's proposal (ii 7) that metics should be allowed the right to acquire the many vacant houses and sites in the city. For it is a fact that, whereas before the Social War the conferment of *γῆς καὶ οἰκίας ἔγκτησις* appears to have been very rare, after that war it is conferred, to judge by our epigraphic evidence, very freely indeed.⁹⁵ This of course affected only privileged individuals, but suggests that Eubulus had taken Xenophon's advice, in part, about how to attract metics.

In the encouragement of trade Eubulus' activity is well enough attested. He provided the trading facilities and hostels⁹⁶ that Xenophon had demanded (iii 12 and 13)—a matter of no small importance. Also he may have been responsible for the institution of courts in which commercial disputes had to be settled within a month,⁹⁷ in response to Xenophon's demand for an acceleration of justice (iii 3).

The raising of capital by forced loans for other than military purposes was not unknown to the fourth century. Xenophon's idea (iii 7) was to provide the money necessary for his schemes by *eisphora* and his hopes were mainly concerned with the Attic silver mines.⁹⁸ Eubulus probably was interested: he prosecuted Moerocles in connection with mining contracts (Dem. xix 293) and certainly after the Social War there was a new and considerable attempt to exploit the mines;⁹⁹ by 341 Demosthenes could speak of them as a likely object for Philip to covet (viii 45). Indeed, it is not inconceivable that some slaves were bought for hiring as Xenophon (iv 13 f.) proposed.¹⁰⁰ There is, however, nothing to suggest that Eubulus went in for large scale capital investment in the mines, and one may presume that he was suitably cautious. Yet the notion of capital investment he may well have developed. This he could have done, not by *eisphora* (for Demosthenes could hardly have been silent about such a procedure) but by direct borrowing. Lycurgus certainly

⁹³ *Staatshaushaltung* 2 i 698 f.

⁹⁴ The tax was 12 dr. for men and 6 dr. for women who did not have husband or son at work (Pollux 3.55 *μέτοικος ὁ τὸ μετοίκιον συντελών [τοῦτο δ' ἦν] ἰβ' τῷ δημοσίῳ δραχμαὶ καὶ τῷ γραμματεὶ τριώβολον* and Harpocration, s.v. 'μετοίκιον'). Cf. Bus.-Swob., *op. cit.*, 984 n. 7. At the end of the century there were said to be 10,000 metics (Ath. vi 272c).

⁹⁵ Early grants in *IG* i² 110.30, and ii² 53; grants later than the Social War *IG* ii² 130, 132, 206, 287, 342, 343, 351, 360, 373, etc.

⁹⁶ Din. i 96. *SIG*³ 1216 is a mid-fourth-century lease of some buildings in the Piraeus and shows that something like an 8 per cent return was to be expected

on money invested there (cf. Isaeus xi 42). For a collection of evidence on hostels see E. Ziebarth in *Εἰς μνήμην Σπυρ. Λάμπρον* (Athens, 1935) 343 f.

⁹⁷ [Dem.] vii 12, *AP* 59.5, Pollux 8.63, Harpocration s.v. 'αἷ τε ἐμπορικαὶ καὶ ἐρανικά', Dem. xxxiii 23.

⁹⁸ Not solely by any means (cf. Wilhelm, *loc. cit.*, 31 f.), but the discussion of the mines played a large part in Xenophon's treatise and his expectations were practically unlimited.

⁹⁹ Cf. Hopper, *BSA* xlviii (1953) 251 and n. 376 for suggestion that the exemption mentioned in [Dem.] xlii 17-19 was due to Eubulus.

¹⁰⁰ Ar., *Pol.* 1267 b 18 and see n. 67.

did this¹⁰¹ and, since it was desirable to get the economy moving quicker than the revenues allowed, he may well have raised money to finance the construction of καταγώγια and πωλητήρια.¹⁰² The main source of capital, ἀφορμή, was no doubt the surpluses of the administration, and that is why Demosthenes bade the Athenians ταῖς περιουσίαις ταῖς οἴκοι ταύταις ἀφορμαῖς ἐπὶ τὰ ἔξω τῶν ἀγαθῶν χρῆσθαι (iii 33), but in the early years Eubulus may have begun to practise what Xenophon preached and Lycurgus subsequently carried out on so large a scale.

So much for economic measures, but there was another side to Eubulus' work for which the *Revenues* is no help, viz. the strengthening of Athens' military forces, to which Dinarchus (i 96) refers. Ποῖαι γὰρ τριήρεις εἰσὶ κατεσκευασμέναι διὰ τοῦτον (i.e. Demosthenes), ὥσπερ ἐπὶ Εὐβούλου, τῇ πόλει; ἢ ποῖοι νεώσοικοι τούτου πολιτευομένου γεγόνασι; πότε οὗτος ἢ διὰ ψηφίσματος ἢ νόμου ἐπηνώρρωσε τὸ ἵππικόν; τίνα κατεσκεύασε δύναμιν τοιούτων καιρῶν παραγενομένων μετὰ τὴν ἐν Χαιρωνείᾳ μάχην, ἢ πεζὴν ἢ ναυτικὴν. Little, however, can be said to amplify this. It is clear, of course, that Eubulus' economic measures in themselves must have had important effects for Athens' military power. At the end of the Social War there could not have been anything like the forty talents which were due under the μερισμός to the Knights,¹⁰³ and in fact Xenophon explicitly said that they were not receiving τὰ πάτρια (*Revenues* vi 1); as a result it was difficult to find as many as a thousand citizens to take on the expense (*Hipparch.* ix 3 f.) and the hipparch had either to argue young men into doing their duty or to apply the coercion of the courts (*ibid.*, i 9 f.). Once the state could be counted on to pay its share, no doubt the cavalry force improved. Likewise, as Xenophon had predicted (*Revenues* iv 51-2), military training, and guard and patrol duty in Attica must have become efficient, and in general the city εὐπολεμωτέρα, ἐφ' ἐκάστοις τῶν ἔργων τῆς τροφῆς ἀποδομένης. But presumably Dinarchus had more in mind than this. The building of the docks and the Skeuothekē is adequately attested,¹⁰⁴ but what of triremes or reform of the cavalry or the assembly of large military and naval forces?

As to triremes, Dinarchus may be simply referring to the large increase in the navy between the Social War and Chaeronea. At the beginning of 357/6 Athens possessed 283 ships (*IG* ii² 1611 l. 9); there were probably few built in the Social War, to judge by Demosthenes' speech against Androtion, but by 353/2 there were 349 (*IG* ii² 1613 l. 302) and by 330/29 there were 392 (quite apart from quadriremes; *IG* ii² 1627 l. 269) some of which were no doubt built after 338, but in view of Dinarchus' remarks perhaps not many. Thus in the period of Eubulus' power the fleet was substantially increased, and, although mere increase of numbers was unimportant (for a fleet of 300 was surely ample for any conceivable emergency), the regular provision of new ships was essential if the city was to have an adequate number of seaworthy vessels. The dominance of Eubulus did not bring neglect of the navy. Yet Eubulus may have been more directly involved. Lying in the old Skeuothekē in 330/29 was a quantity of shipbuilding timber remaining 'from what Eubulus bought' (*IG* ii² 1627 ll. 352-4). Presumably, as remarked above, this happened as part of the control of the νεωρίων ἀρχή by the Theoric Commission, to which Aeschines (iii 25) referred. It suggests that Eubulus was actively concerned with the increase of the fleet as well as with the reorganisation of the naval services in the Peiraeus.

¹⁰¹ Plut., *Mor.* 852 B πολλὰ δὲ τῶν ἰδιωτῶν διὰ πίστεως λαβόν, καὶ προδανείσας καὶ εἰς τοὺς τῆς πόλεως καιροὺς καὶ τοῦ δήμου τὰ πάντα ἐξακόσια καὶ πεντήκοντα τάλαντα - - - (but for the numeral cf. 841 D). Cp. *IG* ii² 333 C ll. 7 and 9 προδεδανεισμένα.

¹⁰² Demosthenes' charges that his opponents have become rich quickly may relate to profitable investment in State enterprises (iii 29, xxiii 209) although he may have no more in mind than Isocrates (viii 127) in slandering Eubulus' predecessors.

Perhaps Moerocles' sly dig (*Ar., Rhet.* 1411 a 15) was directed at Eubulus and the profitableness of lending to the State (cp. *Rhet.* 1376 a 10).

¹⁰³ Xen., *Hipparch.* i 19, Dem. xxiv 97. Also see Wilhelm, *Wiener Studien* lii (1934) 34 and 52 on fragments of Lysias, *Against Theozotides* in *Hibeh Papyri* i 49 ff. n. 14.

¹⁰⁴ Aesch. iii 25, *IG* ii² 505, Philoch. F. 56a. Xen., *Revenues* vi 1 anticipates the work.

Of reform of the cavalry there is no mention beyond Dinarchus. The only proposal heard of in this period is that advocated by Xenophon in the *Hipparchicus* (ix 3 f.), to the effect that one-fifth of the cavalry should be made up of mercenaries, and Xenophon's aim in this was more easily to keep the cavalry at full strength. There is, however, no evidence to suggest that this was put into effect. Indeed Philochorus' account of Athenian aid to Olynthus in 348 (F. 51) suggests that by that date, at any rate, no such change had occurred.¹⁰⁵ Tradition was probably too strong for such a change. So the allusion of Dinarchus must remain obscure. There may not have been much to the matter.

Still more vague and less credible is Dinarchus' suggestion that Demosthenes compared unfavourably with Eubulus in the preparation of strong military forces. Memories, politically speaking, were no longer at Athens than elsewhere, and it looks as if Dinarchus has simply forgotten the great achievements of Demosthenes in the late 340s. However, there may be something behind it. In the 330s the Treasurer of the Stratotic Fund is found providing money εἰς τὰς Νίκ[ας καὶ] τὰ π[ο]μ[π]εῖα (IG ii² 1493 ll. 11, 16, 20); so he was probably administering a fund which had a regular income from other than direct levies of eisphora. When did this fund begin to receive regular amounts under the μερισμός? Elsewhere¹⁰⁶ I have proposed that the Fund and its Treasurer were instituted in 378 and, if that is correct, it may also be true that from its inception some money was regularly assigned to the Fund; in which case Eubulus may have increased the amount the Fund received under the μερισμός. Alternately, he may have instituted such regular income. One of these two alternatives seems likely enough, and perhaps it is to this that Dinarchus' extravagant words refer.¹⁰⁷

Finally, there is to be mentioned one matter on which Dinarchus is silent because he could not belittle Demosthenes' work, viz. the repair of the walls and defences of Athens (Aesch. iii 27-31). As the *Revenues* (vi 1) shows, the walls, like the docks, in 355 needed attention, and Eubulus was certainly active in this respect.¹⁰⁸ Indeed the law, which survives fragmentarily as IG ii² 244, ascribed generally to 337/6 but without cogent reason,¹⁰⁹ may well belong to the period of Eubulus, or, if this were unacceptable, the 'earlier law' mentioned in line 13 may so belong.

To sum up, scanty and conjectural as all this is, it is reasonably clear that Eubulus did not confine himself to a mere increase of the revenues of the State. All departments of the State's activity benefited financially, and he did not neglect to prepare for war.

How seriously, then, did the system of Eubulus prevent Athens properly resisting Philip? Obviously the Council and the People were unable suddenly to seize upon the surpluses of the administration. Emergencies had to be met, as they traditionally had been, by εἰσφοραί. But there was nothing to prevent any Athenian citizen seeking to have Eubulus' laws changed or repealed by the normal process of νομοθεσία. Demosthenes advocated just that in the *Third Olynthiac* (10 f.). Although he demanded that those who had gained popularity by the institution of the laws should themselves incur the odium of their repeal, it is clear that he could perfectly well have sought to effect the repeal himself. The truth must be that hardly anyone in Athens wanted to change Eubulus' system. The

¹⁰⁵ Dem. xxi 132 makes a similar impression.

¹⁰⁶ 'Demosthenes and the Stratotic Fund', *Mnemosyne* 1962 337ff.

¹⁰⁷ I see no reason for believing that the ταμίαι τῶν στρατιωτικῶν did not exist before Eubulus. See article cited in previous note.

¹⁰⁸ Dem. iii 29 with Schol.; xiii 30; xxiii 208.

¹⁰⁹ F. G. Maier, *Griechische Mauerbauinschriften* i Heidelberg (1959) 40 discusses the date and decides

that '337/6 remains the most probable date', but the only reason for putting it in that year is the known activity of Demosthenes (Aesch. iii 27-31). On the other hand, Eubulus did something about the walls and there is no reason epigraphic or historical why the inscription should not belong to his period. Cf. Aesch. i 80 for Timarchus' speech about repair of walls in 347/6.

evil, if evil there was, lay in the will of the People, not in the system, for the system could be swept away as soon as the People wished.

Why did the People not accept Demosthenes' opinion? The Theoric distributions, small as they were, were dear to Athenians and important enough to be called 'the cement of the democracy' (Plut., *Mor.* 1011 B), and the customary condemnations of Eubulus suggest that the People was not really free to choose but was corrupted by a paltry bribe into disregarding its duty. Such contempt of the People is not justified, but even if it were the case that the People was bribed into complacency it by no means follows that Eubulus 'bribed' for ill purposes and with disastrous results.

All depends on the view taken of the policy followed by Athens under his leadership between 352 and 346. Later Athens either had peace or followed Demosthenes in bringing on the final conflict, and earlier Philip did not appear a menace, while in 352 Eubulus was strenuous in keeping Philip out of Phocis and ready to defend the Chersonese, just as in 346 he led the resistance to Philip while resistance was practicable. It is on Athenian policy over the war in the north that he must be judged. There are two views. Either, as Demosthenes wished, Athens had to commit her full military and financial power to fighting near Macedon, or she had, as Eubulus wished, to fight in defence of Greece. If the main contentions advanced in this article and elsewhere are correct, namely that the war for Amphipolis was a ruinous luxury and that Athens did all in defence of Olynthus that she could reasonably have attempted, the financial system of Eubulus did not damage Athens' resistance to Philip.

The key to the understanding of the relations of Greece and Macedon is to be found not in the realm of morals and moral decline but in strategy and military power. Two things, however, confuse historical judgment. The first is that Demosthenes' opponents must be judged almost entirely on what he says about them. The second is that Philip won the battle of Chaeronea: the policy of meeting Philip in Greece ended in disaster. These two things combine to suggest that the whole policy was ill-judged at least, perhaps even traitorous, and it is easy to pass to classifying politicians as pro- and anti-Macedonian. This may be improving, but it is not history. The truth is that with the rise of the national state the balance of power in the Greek world altered radically, and no one city state could hope on its own and in its own interests to check effectively the domination of Greece. Eubulus' policy of abandoning mere war of conquest and seeking collective security was the only chance of success and it failed not because it was ill-judged but because the new national state was led by a great general. Instead of denouncing the opponents of Demosthenes, historians would do better to concentrate on the military skill whereby Philip was able to penetrate into Greece in 339/8 when politically he was excluded. Eubulus' policy might well have saved Greece if Philip had not proved himself master of war as well as of politics.

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A NOTE ON THE HIPPARCHIES OF ALEXANDER

By the courtesy of Mr P. A. Brunt whose admirable paper now published (p. 27 ff.) I had the pleasure of reading at an early stage, and of the Editor in allowing me at Mr Brunt's suggestion to add a short note here, I am enabled to write about a point on which I was unable to follow Mr Brunt in his interpretation. It concerns the date at which Oriental cavalry was first introduced by Alexander into the hipparchies of Companion Cavalry, which (following Mr Brunt) I now believe to have become eight in number by a reorganisation of the year 328.¹

About the Oriental cavalry actually named by Arrian (at the Hydaspes battle) as participating in the invasion of India there is no disagreement between Mr Brunt and myself: the Arachosians and the Parapamisadae horse, the Bactrians, Sogdians, Scythians and Dahae are mentioned by Arrian in terms that show clearly that they were *not* included in the hipparchies at this time but were serving as separate units (*A.* v 11.3, 12.2: Brunt, 43 f.).² These people, however, represent only a part of the Oriental cavalry forces available to Alexander, if he chose to use them, at any time after (say) early 330 B.C. for the central satrapies of the Persian Empire (Susiana, Persis and Media), and after the end of 330 for the satrapies occupied with little resistance during that year ('Parthia', Tapuria with Hyrcania, Areia and Drangiane). These Western or Central Iranians are never named by our sources as having participated in any of Alexander's campaigns, though the Far Eastern Iranians (Bactrians, Sogdians, etc.) are so mentioned by Arrian at the Hydaspes battle, and indeed as early as the year 328 (*A.* iv 17.3, Bactrians and Sogdians only). Is the silence of the sources on this matter to be taken as proving that no Oriental cavalry did serve in Alexander's army at all before the final reorganisation of 324, except those troops whom we have noticed as mentioned by name in units outside the hipparchies?

I had thought till recently that the much-discussed passage *A.* vii 6.3 with its allusion to Zarangians, Areians, Parthyaecans and Persian Euacae (as well as to the Bactrians, Sogdians and Arachosians) was supplying the evidence we needed that Western and Central as well as Far Eastern Iranians had served in the army before 324. But I am persuaded now by Mr Brunt's interpretation that this passage must allude to the reorganisation of 324 itself and to events immediately before it, and not to earlier years. Moreover, the absence from our sources of all reference by name to the more westerly Iranians suggests very strongly that these peoples probably never served, like the Bactrians and Sogdians and others who are mentioned by name, in separate units outside the hipparchies. It remains to consider whether it is impossible that they should have served inside the hipparchies, achieving anonymity in this way.

There is one passage in Arrian that suggests that they did. Among the grievances of the Macedonians that incited them to mutiny at Opis Arrian includes *ἀνάμειξίς τῶν ἀλλοφύλων ἱππέων εἰς τὰς τῶν ἐταίρων τάξεις*.³ These grievances (the others concern Alexander's Persian dress and the training of Iranian youths to become Macedonian-type soldiers) had been at work, Arrian tells us, *κατὰ τὴν στρατίαν αὐτὴν πᾶσαν*: this last phrase means the Indian expedition, and not something more recent, because there was no 'campaign' between the return from India and the outbreak of the mutiny. Interpreted literally

¹ Brunt, 28 ff. I do not propose to refer to modern works already cited by Mr Brunt, except where it seems essential. Everywhere I have found the books both of Berve and of Tarn indispensable.

References to Arrian's *Anabasis* are abbreviated as *A.*

² This I take to be the view of Berve, *Das Alexanderreich* 107 f.: though his use of *A.* iii 30.6 to support it is inadmissible, I hope to show that his account of what actually happened was correct.

³ *A.* vii 8.2.

and at its face value, this passage is itself sufficient evidence, if we believe it, for what I want to establish. But the passage follows fairly closely after a longer catalogue of Macedonian grievances occasioned by more recent happenings (vii 6.2 ff.), and it seems possible to argue that Arrian could have mistakenly confused the issues and have turned some immediate grievances of 324 into a more generalised grievance (here) operating throughout the years of the Indian expedition, even though there was then no ground for them in reality. Though I do not myself think that Arrian can have done this, it seems best to discuss first the other arguments and evidence for (and against) Orientals in the hipparchies, before coming back finally to these two 'grievance' passages.

Perhaps the strongest argument in favour of the notion that Arrian was not mistaken or muddled is an argument from general probability and our sense of what was possible or desirable, or even what was essential, for Alexander's plans and policy after the Persian King had become a fugitive and the capital cities of the Empire (Susa, Persepolis, Ecbatana) had been occupied. Both for military and for political reasons, one would think that Alexander must have looked for co-operation from the new subjects. On the military side, though there would be no more Grand Armies to conquer (this side of India at any rate), local resistances were to be expected (and did occur). The vast areas of the Empire still to be occupied, and the character of the opposition that did appear, particularly in Bactria and Sogdiana and the northern steppes, set a premium on the cavalry arm. His own Macedonians had proved invincible, and they were now being heavily reinforced (Brunt, 37), but with much of the fine cavalry of the former Persian armies available now, it would seem surprising indeed if Alexander did not make use of it, always supposing that it was politically sound to do so.

On the political side, to call on the services of these people in his army appeared to him more than just 'sound', probably; it was positively necessary. More than three years earlier, not long after the battle of Issus, rejecting the peace overtures of Darius and claiming the kingdom of all Asia, Alexander had been able to write: 'Those of your men who fought with you and were not killed in the battle but surrendered to me, are safe with me and not sorry to be here: they are serving now in my army of their own free will.'⁴ This was the beginning (with Darius still alive and not finally defeated) of that policy of presenting himself to the inhabitants of the Empire as their new and rightful King, which went forward after the victory of Gaugamela by way of the appointments of Iranian nobles to satrapies and the introduction of Persian ceremonial into his own Court, and later the trial of Bessus before an 'assembly of the Medes and Persians'. If it was important to Alexander to gain the recognition of the new subjects and especially of the Iranians, no more practical form of recognition existed than the obligation of military service.

The first allusion by name, however, to Iranians serving in the army is that to the Bactrians and Sogdians of *A. iv* 17.3, referring to a point of time probably towards the end of the year 328, more than three years after Gaugamela and more than two years after the death of Darius. Yet if Bactrians and Sogdians could be enlisted by 328, when those two satrapies were still very far from 'pacified', it is hard indeed to believe that the satrapies by now long securely held, such as Persis, Media and the rest, had not been called on for levies before this. One suggestion that this was so comes from Arrian's reference to *ἵππακοντισταί* in the army as early as 330 in Hyrcania (*A. iii* 24.1); described here as a separate unit on their own (*ἤδη γὰρ αὐτῷ καὶ ἵππακοντισταὶ τάξις ἦσαν*). No European cavalry unit so armed is known, and Arrian's generalisation (à propos of one phase of the battle of Gaugamela) about *ἀκοντισμός* as a normal feature of cavalry fighting is to be interpreted there as being apposite to the 'Persian' cavalry.⁵ Berve's conjecture that

⁴ *A. ii* 14.7. The authenticity of this exchange of letters has naturally been questioned, but there is no cause to doubt that Arrian's version, derived from

Ptolemy, gives the gist of what was really written. See now L. Pearson, *Historia* iii (1955) 447-50.

⁵ See below, p. 73.

Alexander's unit of mounted javelin-men, since it first appears in Hyrcania, consisted of survivors from the troops with Darius at his death, seems unnecessary;⁶ any or all of the satrapies most recently occupied (Susiana, Persis, perhaps Carmania, Media), could have supplied this cavalry, and one or more of them probably did. It was, we see, a separate unit at this time (its strength never revealed), just as the Bactrians and Sogdians, etc., were separate units both before and after the emergence of the eight hipparchies late in 328.

The only other possible allusion that I have found to Oriental cavalry in the army before this occasion in 328 is Arrian's ὅσοι ἄλλοι ἵππεῖς ἔξω τῆς ἵππου τῆς ἑταιρικῆς (this in Media), where the other European cavalry seem to be all accounted for, in or near the same passage.⁷ I do not stress this, however, because of an evident untidiness here that leaves open a chance that Arrian's ὅσοι ἄλλοι, etc., really refers to a third unit of mercenary horse in addition to the two that he has described as such.

Apart, then, from the *hippakontistai* and the people just mentioned (if they are relevant), either there were no other West- or Central-Iranian cavalry serving before 324, or if they did serve they must be concealed inside 'the hipparchies'. At first sight this last notion seems improbable, since unquestionably the hipparchies when they appear are formations in which primarily the Companions are concerned.⁸ If one looks farther, however, there are points of interest, and perhaps of importance.

First, the use of the word hipparchy 'anachronistically', referring to a time before the hipparchies (as we know them later) had come into existence.⁹ A re-examination leads me to think that these instances (three in Arrian, one in Diodorus) are not mere mistakes of Arrian writing hipparchy where he should have written *ile*, but are perhaps clues by which we may see how the hipparchy principle (if I may so express it) developed. Considering the instances in turn, the single instance in Diodorus is a clear substitution of hipparchy for *ile*: in the same passage a parallel variation of *strategia* and *taxis* (of the phalanx) suggests that Diodorus wrote these things probably because he suffered from the bad writer's frailty of being unable to call a spade a spade twice in the same sentence.¹⁰ But Arrian is made of sterner stuff than this, and is besides a military man not given to purely careless misuse of military terms. I think that when Arrian started to use the word hipparchy he meant something by it, and something different from *ile*.

This is self-evident in the very first instance (i 24.3). 'He sent Parmenion to Sardes' (to spend the winter 334/3), 'giving him a hipparchy of the Companions, and the Thessalian cavalry, and the rest of the allied troops and the baggage train.' Here an *ile* of Companions (200?) would be an incongruous detail, compared with the 1800 Thessalians and the 6000 + allies. Moreover, a study of Alexander's own winter campaign in Lycia and Pamphylia that followed, suggests that what he had given Parmenion was a *group of ilai*, half or more of the Companions; for the Companions win no mention in the narrative of this campaign, except for the remark that on one occasion 'the cavalry were no use to him on this difficult terrain', a remark broadly applicable to the whole winter campaign really.¹¹ Hence the detachment with Parmenion of a *group of ilai*: here, indeed, we see the basic meaning of the word hipparchy.¹²

Next, in Sogdiana Ptolemy is detailed to capture Bessus taking with him '3 hipparchies of the Companions and all the *hippakontistai*' (iii 29.7). This on the face of it *could* be a mere mistake of Arrian writing 'hipparchies' for *ilai*, for at this stage there exist only two

⁶ Berve, 151.

⁷ A. iii 19.5, Thessalians and allies: *ibid.*, 20.1, Companions, *prodromoi*, mercenaries under Erigyus: *ibid.*, 7, mercenaries and Thracians, *καὶ ὅσοι ἄλλοι ἵππεῖς*, etc. (above).

⁸ See below, p. 71 f.

⁹ Brunt, 29.

¹⁰ Diod. xvii 57.1 ff.

¹¹ A. i 28.4: for the winter campaign, i 24.3-29.3.

¹² Best illustrated perhaps by Polyb. x 23.4 *καὶ αναγαγεῖν πάλιν - - - εἰς οὐλαμούς, εἰς Ἰλας, εἰς ἱππαρχίας*. The textbook hipparchy of Asclepiodotus (7.11) contains 8 *ilai*.

'hipparchs' Hephaestion and Cleitus (iii 27.4). But if a mistake, it is a naughty one, the appointment of these same hipparchs having been described so recently. It seems possible here (to me, even probable) that Arrian means by 'hipparchies', again, 'groups of *ilai*': and if this is admitted then it seems to follow that there is perhaps something that prevents him from writing simply '6 (c.g.) or 9 *ilai* of Companions'. Can it be because he knows that these 3 groups of *ilai*, though they contained *ilai* of Companions as their most important ingredient, contained other *ilai* as well? Of Macedonian *prodromoi*, for example, or of Iranians, or of both?

Here the third instance may help, and it needs to be quoted in full.

A. iv 4.6-7. - - - ἀφῆκεν ἐπὶ τοὺς Σκύθας τὸ μὲν πρῶτον μίαν ἱππαρχίαν τῶν ξένων καὶ τῶν σαρισσοφόρων ἱλας τέσσαρας - - - - . Ἀλέξανδρος δὲ τοὺς τε τοξότας καὶ τοὺς Ἀγριᾶνας καὶ τοὺς ἄλλους φιλοῦς, ὧν Βάλακρος ἦρχεν, ἀναμίξας τοῖς ἱππεῦσιν ἐπῆγεν ἐπὶ τοὺς Σκύθας. - - - ἐλάσαι ἐκέλευσεν ἐς αὐτοὺς τῶν τε ἐταίρων τρεῖς ἱππαρχίας καὶ τοὺς ἱππακοντιστάς ξύμπαντας. καὶ αὐτὸς δὲ τὴν λοιπὸν ἵππον ἄγων σπουδῇ ἐνέβαλεν ὀρθαῖς ταῖς ἱλας.

This passage tells us much. (1) The *prodromoi* (= *sarissophoroi*: for the equation, see Brunt, 27) are still here (and *a fortiori* were, still, at iii 29.7) separate from the Companions, and have not yet been included in hipparchies with them. (2) The idea that hipparchy = *ile* in this passage can only be maintained by taking the lowest possible view of Arrian as a writer. Not once but twice in the passage the two words occur closely juxtaposed: only a Diodorus could be using them as synonyms. (3) The 'one hipparchy of mercenaries' shows again that hipparchy = 'group of *ilai*'. This was the start of a big cavalry battle, for which one *ile* would have been not worth Alexander's while to detail or Arrian's to record. (4) The 'three hipparchies of the Companions', with hipparchies now established (I hope) as 'groups of *ilai*', raises again the question, Why did Arrian or his source not write 'x *ilai* of the Companions', like '4 *ilai* of *sarissophoroi*' just above? Is it because not all the *ilai* in this force were Macedonians, so that 'hipparchies' has become an economical way of describing a mixed force in which the Companions were the most important element? Incidentally, if 3 hipparchies = 3 groups of *ilai*, then this detachment amounts to 6 or more *ilai*, and this is probably less than half of the Companions, since the rest are being led by Alexander himself. We are thus led to think in terms of 12 or more *ilai* altogether, with the total much likelier to be near 20 than near 12. This is becoming too many *ilai* for them all to be Macedonian Companions even allowing for the known reinforcements (Brunt, 36 f.). There would seem to be surplus *ilai* here, for the Iranians, if that suggestion were adopted.¹³

The crucial thing is to know for sure what Arrian means when he writes 'the Companion Cavalry', alluding to them in the period after the emergence of the eight hipparchies (328).

(1) There are the allusions where it seems impossible that he can mean anything but Macedonian *ἑταῖροι*. A. vi 14.4, Alexander embarked favoured troops on shipboard (instead of having them march), including τῶν μὲν ἐταίρων ἱππέων ἐπτακοσίους καὶ χιλίους. A. vi 17.3, he sent Craterus back from India commanding (among other troops) τῶν ἐταίρων τε καὶ

¹³ The only 'totals' for troops on campaign hereafter, those for the troops with Alexander himself at the Hydaspes battle, are hard to accept, and seem certainly understated.

The cavalry total (5000, A. v 14.1) includes 1000 Dahae *hippotoxotai* (12.2; 16.4), + the Scythians, + the Bactrians and Sogdians (12.2; certainly 1000 together, and perhaps nearer 2000): this leaves 3000 (maximum, but probably less) for the hipparchies,

of which five are named, but seven may have been there (only one was left with Craterus, 11.3).

Likewise the infantry total (6000—v 14.1; 18.3) will hardly stretch to cover the hypaspists, two brigades of the phalanx, the archers, the Agrianians (12.2), and some *akontistai* (13.4): at the start of the Indian expedition we hear of 'two chiliarchies' of the archers (iv 24.10), and these are not the whole force if Arrian is writing exactly.

ἄλλων Μακεδόνων ὅσους ἐς Μακεδονίαν ἀπομάχους ὄντας ἤδη ἔστελλε. vii 11.3, the mutiny at Opis was finally broken by the news that Alexander, ostentatiously planning to dispense with his Macedonians altogether, was even giving the names of the famous Macedonian regiments, including ἡ τῶν ἐταίρων ἵππος, to the Persian levies: this must imply that hitherto its members have been all Macedonians.

(2) Allusions which 'equate' Companion cavalry and 'the hipparchies'. Such are iv 24.1; vi 6.1, with *ibid.*, 4; vi 21.3. The natural inference from them is that the hipparchies contained only Companions (and all the Companions presumably were Macedonians).

(3) Two allusions to the hipparchies which do not themselves invite the inference just mentioned (v 12.2; vi 7.2); and another which even suggests the opposite inference, that the hipparchies did contain cavalry other than the Macedonian—ἐπὶ μὲν τὸ δέξιον κέρας παρήγαγε τό τε ἄγλημα τῶν ἱππέων καὶ τῶν ἄλλων ἱππαρχῶν τοὺς κρατίστους ἐπιλεξάμενος (v 13.4, describing an early stage of the Hydaspes battle). This description is baffling if it be taken to refer to a selection of τοὺς κρατίστους from the Macedonian Companions themselves; an impossible thing to do on the battlefield itself. It only makes sense if it refers to a selecting of some units and a leaving-behind (temporarily) of some other units of those included in the hipparchies. By Mr Brunt's interpretation (p. 27 f.), it would make sense to suppose that he selected the Companions proper and left behind the Macedonian *prodromoi*. It would also make sense to suppose that he selected the Macedonian *ilai* and left behind the non-Macedonian *ilai* of the hipparchies, if we think that there were any such.

Finally there is the passage (mentioned above, p. 68) which, without alluding to hipparchies, invites us to think that Oriental cavalry had been introduced into the same formations as the Companions before the start of the Indian expedition. vii 8.2 (summarising the grievances that inspired the mutiny at Opis) - - - κατὰ τὴν στρατιὰν ταύτην πᾶσαν πολλοῖς καὶ ἄλλοις ἀχθεσθέντες, ὅτι πολλάκις ἤδη ἐλύπει αὐτοὺς ἢ τε ἐσθλὴς ἢ Περσικὴ ἐς αὐτὸ φέρουσα καὶ τῶν Ἐπιγόνων τῶν βαρβάρων ἢ ἐς τὰ Μακεδονικὰ ἦθη κόσμησις καὶ ἀνάμιξις τῶν ἀλλοφύλων ἱππέων ἐς τὰς τῶν ἐταίρων τάξεις. 'This whole campaign' can only refer to the Indian expedition.

To reconcile this with A. vii 11.3 (cited above), where the plain inference is that up to the time of the mutiny at Opis ἡ τῶν ἐταίρων ἵππος had been a Macedonian corps including no Orientals, is a puzzle certainly, but not perhaps an insoluble one. The clue, I suggest, lies in a close interpretation of one sentence in that passage (A. vii 6.2 ff.) which Mr Brunt has analysed so convincingly in general to produce his account of the final reorganisation of the hipparchies in 324, explaining especially the enigmatic 'fifth hipparchy'. This passage is a catalogue of the Macedonian grievances, but this time of grievances at developments mostly very recent (as Mr Brunt shows, p. 43). The grievances include the recent integration of picked Iranian cavalry (of seven specified peoples) into the Companions, and the decisive words seem to be καταλοχισθέντες ἐς τὴν ἵππον τὴν ἐταιρικὴν. I used the word 'integration' for καταλοχισθέντες, to denote the closeness of the union brought about by this recent reform. καταλοχισθέντες ought to mean either that picked Iranians were introduced inside the λόχοι, two of which made up each *ile* of Companions, or else at the very least that one λόχος of each *ile* now became a λόχος of picked Iranians.¹⁴ It is this close integration, exemplified too by the introduction of picked individuals into the *agema* itself and their arming with Macedonian lances instead of their barbarian javelins (*ibid.*, 4-5), that stung the Macedonians on this occasion, and incidentally that distinguishes this recent innovation from the earlier development that had irked them 'all through the Indian campaign', the ἀνάμιξις τῶν ἀλλοφύλων ἱππέων ἐς τὰς τῶν ἐταίρων τάξεις (vii 8.2). These two

¹⁴ For the λόχοι, A. iii 16.11.

descriptions cannot (except by a blunder of Arrian) be descriptions of the same development, for the first refers to a recent development of 324, while the second refers explicitly to an earlier one. Therefore τῶν ἑταίρων τάξεις are not to be equated with the *ilai* of the Companions. (If they are so equated here, there is no ground left for the new Macedonian grievances of 324: the damage would have been done already.) Therefore they *are* to be equated with the hipparchies, of which the Companions form a part (the most important part, naturally). The Oriental cavalry must have been 'mixed into' the hipparchies of the Companions in the sense that one or more *ilai* of Orientals were added to *ilai* of Macedonians to make up the hipparchy. The first 'mixture' was more cautious and less thorough than the final one.

The object of my earlier remarks, on the development of the hipparchies in general, was to support Arrian's categorical statement about ἀνάμειξις - - ἐς τὰς τῶν ἑταίρων τάξεις, and especially to disarm the criticism that Arrian when he wrote οἱ ἀνάμειξις in this context was merely making a mistake. Myself, I do not think he was, even though this obliges me to face the consequence that in those passages where he equates the hipparchies with the Companions, he is really equating the whole with the part (admittedly with the most important part). Of the two historical inaccuracies the second seems to me both the more venial and the more likely.

It remains only to consider (briefly) why Alexander introduced *ilai* of Iranians into the hipparchies alongside the *ilai* of Companions, if he did do this. Because it was safer to use Oriental troops in this way? The appearance, early on, of a separate corps of Iranian *hippakontistai*, and later of Bactrian and Sogdian and Arachosian and Parapamisadae cavalry outside the hipparchies, and of the *hippotoxotai* in India also as a separate corps, proves that this is not the explanation. Was it, then, because the Orientals inside the hipparchies were not, like those just enumerated, horsemen who used missile spears, but were lancers like the Companions themselves? But the evidence seems strong in favour of the missile spear (or the bow) as the weapon *par excellence* of the Iranian horseman, and in the absence of evidence for Iranian lancers of any consequence, I should hesitate to build on this hypothesis.¹⁵ I see this development rather as an answer to problems raised by the conditions of warfare first in Sogdiana and later in India, requiring the army to operate more often divided than united, and with frequent demands for detachments of cavalry (among others) from the main body. Tactically there were advantages perhaps, on such occasions and especially against a very mobile enemy, in having missile-armed cavalry together with lancers. And besides there was always some advantage in husbanding the Macedonians, for they had their limits both in numbers and in their capacity for work. To guess at the numbers of *ilai*, Macedonian and Oriental, that made up a hipparchy would be idle. There is not even any need to suppose that the number was either constant or uniform.¹⁶ The appearance of the word chiliarchy as a synonym for hipparchy in

¹⁵ Hdt. v 49.3, with vii 84 and 86, v 61-7: Xen. *Anab.* i 9.5; cf. *ibid.*, i 8.3 and 27. See in general A. T. Olmstead, *History of the Persian Empire* 338-46.

At Cyrus the Great's review as described by Xenophon (*Cyrop.* viii 3.15 ff.) there were 4000 δορυφόροι and 2000 ξυστοφόροι, but 40,000 Persian horse whose style is not named and presumably *not* lancers, as well as Medes, Armenians, Hyrcanians, Cadusians, and Sacae (with no figures given): though the figures here are valueless, the proportion of lancers to others may perhaps reflect the conditions of Xenophon's own day.

¹⁶ It seems even possible that the naming by Arrian of only 6 hipparchies (out of 8) at the Hydaspes could be due to a temporary amalgamation, if

(e.g.) the hipparchies were under strength. Some Macedonians could be among the 3500 cavalry, 10,000 infantry left as army of occupation in Bactria-Sogdiana (*A.* iv 22.3), where nationalist or independent spirit had been greatest.

Berve's view that the mysterious ἑκατοστῆες of *A.* vi 27.6 (cf. vii 24.4) may have been the Oriental units inside the hipparchy, while the *ilai* remained Macedonian, is attractive, but depends overmuch on his belief (based on vi 27.6) that the ἑκατοστῆες cannot be a subdivision of the *ile*. Myself I think that the passage does admit of the interpretation that Berve excludes, and that vii 24.4 makes it probable that the cavalry λόχος came to be called ἑκατοστῆς (so Tarn, *Alexander* ii 160 f.).

Alexander's last year does suggest a notional figure of 1000 men (5 *ilai*?) for the five hipparchies of the final reform of 324; but the significance of this must be small for the years that were past, and even for the future not very great.¹⁷ Every general is obliged to adapt, improvise and modify in nearly every campaign.

Perhaps when all is said the main significance of this experiment with the hipparchies is not military but political. Even serving as they did in their separate *ilai*, belonging to the same hipparchy brought Macedonians and Iranians together in a limited form of association. How the Iranians were selected we shall never know, but the Macedonians concerned were those who stood closer to Alexander in the Macedonian social scale than did the infantrymen of the phalanx: the horsemen of the Companions might be expected to be a little the more sympathetic towards his political plans.¹⁸ It may thus be right to see in the composition of the hipparchies an early and cautious move by Alexander towards promoting that *ὁμόνοιά τε καὶ κοινωνία τῆς ἀρχῆς τοῖς τε Μακεδόσι καὶ Πέρσαις* which, among so many things to do with Alexander's aims that must remain problematical, is perhaps one of the certainties that we can count on as something that he did really aim to bring about.¹⁹

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¹⁷ *A.* vii 14.10. According to Berve (p. 112) there was only one hipparchy called 'chiliarchy' (namely the 1st), because there could be only one chiliarch (= Vizier of the Empire). This may be right; but the language of Arrian seems to me to imply that the unit had been called 'the chiliarchy of Hephaestion' before his death, and if so it can only have been to distinguish it from other chiliarchies, one would think.

¹⁸ Thus in the disputed succession after Alexander's death, the cavalry evidently supported the claim on behalf of Roxane's unborn son by Alexander, against that of Alexander's half-brother Arrhidaeus, the infantry's candidate: Arrian *F.* i 1 ff. (Jacoby, *F. Gr. Hist.* no. 156), with Diod. xviii 2.

¹⁹ *A.* vii 11.9.

THE SEAL OF POSIDIPPUS

(PLATES IV, V)

My business is with a poem preserved on two wax tablets from Egypt in Berlin (Inv. no. 14283), dating from about the first century A.D., and first published by Hermann Diels in 1898. Here is a bibliography:

- (1) H. Diels, *S.B. der Berl. Akad.* 1898 847 f. (with photographs of both tablets).
- (2) R. Ellis, *American Journal of Philology* xxi (1900) 76 f. (irresponsible conjectures on the text; utterly worthless).
- (3) W. Crönert, *Archiv für Papyrusforschung* i (1901) 517 f. (brief general comments).
- (4) W. Schubart, *Papyri Berolinenses* (1911) no. 17; photograph of the first tablet with transcript of that part of the text which it contains (all except the last five lines).
- (5) W. Schubart, *Symbolae Philologicae O. A. Danielsson octogenario dicatae* (1932) 290 f. (new transcript, account of the manuscript, brief commentary and critical discussion).
- (6) D. L. Page, *Greek Literary Papyri* i (Loeb Classical Library, 1942; reprinted 1950) no. 114, p. 470 f. (brief discussion, text and translation).
- (7) C. A. Trypanis, *Classical Review* n.s. ii (1952) 67-8 (note on authorship with brief remarks on text).
- (8) W. Peek, *RE* xxii 1 (1953) 430, 440 f., s.v. 'Poseidippos' (general discussion).
- (9) E. Heitsch, 'Die griechischen Dichterfragmente der römischen Kaiserzeit', *Abh. der Ak. der Wiss. in Göttingen, phil.-hist. kl., dritte folge* no. 49 (1961 1) 21 (text and apparatus criticus based on Schubart).

When Diels published the first edition of this text in 1898, papyrology was at an early stage of its development, and even with some assistance from F. G. Kenyon he was able to offer only a very unsatisfactory transcript. The poem gives its author's name as Posidippus, but Diels did not even consider ascribing it to the well-known epigrammatist of that name who flourished during the third century before Christ. The text he printed was full of linguistic and metrical infelicities, and he had little hesitation in ascribing it to an inferior poet living in Egypt during the first century after Christ, the probable approximate date of the manuscript. In ll. 7-8 Diels read *εἰς δὲ τὰ Θήβης τείχε' [Αἰ]γυπ[τ]ίης*, and he consequently supposed that Egyptian Thebes was the home of the poet, whose actual rough draft he took the tablets to be. Crönert took an even more severe view. 'Es ist ein Lied auf das Alter', he wrote, 'aber ohne jede Kunst und verständige Anordnung der Gedanken. Poseidippus, mit mancherlei von den frühern Dichtern erborgten Federn sich schmückend, ohne dabei von richtiger Silbenmessung eine Ahnung zu haben, wechselt zwischen hohlem Pathos und niedrigen Gedanken; erträglich ist noch der Schluss. . . .'

Schubart in his transcription of 1911 improved greatly on the attempt of Diels. Later, he and Wilamowitz wished to print the poem in the *Berliner Klassikertexte*; but Diels objected, and Schubart returned to it only when Diels and Wilamowitz were both dead. In 1932 he offered a new and in most respects improved transcription, and at the same time discussed the poem in some detail. He analysed (p. 296) the writer's mistakes and divided them into two classes, the first consisting of simple errors and the second of deliberate changes in the text, such as those at 8, 12 and 14. 'Hat ein Schreiber oder Schüler', Schubart asked (p. 297), 'ein hellenistisches Gedicht abgeschrieben? Das ist unmöglich angesichts der Verbesserungen der zweiten Gruppe. Eher dürfen wir uns einen bescheidenen Dichter oder einen Studenten vorstellen, der als Stilübung eine Elegie auf den Alter zu

verfassen sucht und als Vorbild die grossen Hellenisten vor Augen hat wie Meleagros, Asklepiades, Poseidippos.' The writer alleges (5, 9) that his name is Posidippus; Schubart thought he might have had an actual elegy of Posidippus before him as a model; he enumerated (p. 297, n. 1) a number of words and phrases that occur in the known poems attributed to this author.¹ As an alternative possibility, Schubart conceded that a student or an admirer might have been copying out from memory an actual elegy of Posidippus. *L.* 14, he thought, fitted in well with this notion; *l.* 8 seemed to him not inconsistent with it; but the many careless errors seemed to him to make against it. Since the carelessness may have been that of the copyist rather than that of the poet, this argument is not a very safe one.

The question of where the writer lived is bound up with the difficult problem of what must be read in *l.* 8. Schubart in 1911 had rejected Diels' reading *τείχε'* [*Al*]γυπ[τ]ίης in favour of *τείχε'* Ἀγην[ο]ρέης; in 1932 he read *τείχεα Πιπ[λ]είης*. At the end of the line, Schubart conjectured (and read in both his treatments of the text) <Κα>σταλίδες. He took Πιπ[λ]είης as going with Θήβης, and supposed (296 f.) 'Pipleian Thebes' to be a way of referring to the Macedonian town of Dion, of which place he imagined the poet to have been a native. Page (p. 471) rightly objected to this unconvincing speculation. Yet Schubart was right to point out that during the first century A.D. Egyptian Thebes was not likely to have been the home of a poet who could expect to be honoured with a statue (*l.* 16 f.), and that the outlook of *ll.* 15-16 is not that of a man writing from an Egyptian town at this date. Like all editors up to that date, Page does not hesitate to ascribe the composition to the first century after Christ, nor does he even discuss the possibility that it might be the work of the third-century Posidippus.

Yet in *Hermes* 53 (1918) 437 f. O. Weinreich had published an inscription of the Aetolian League from Thermum recording a grant of proxeny at Delphi to Ποσειδίππῳ τῷ ἐπιγραμματοποιῷ Πελλαίῳ.² Another person honoured at the same time was called Asclepiades; and since the poet of that name has long been thought to have been closely associated with Posidippus, there can be little doubt that the Posidippus mentioned in this inscription is the well-known poet of that name. This was at once observed by Weinreich, and was repeated by Wilamowitz in 1924 (*Hellenistische Dichtung* ii 148); but Trypanis was the first to point out its relevance to *l.* 16 of our poem, in which the poet says that he originates from Pella. But Trypanis' article shows no awareness of the formidable difficulties still to be overcome before such an attribution can be accepted; the texts published by Schubart, Page and more recently Heitsch contain linguistic and metrical anomalies scarcely to be credited to a competent writer of the third century before Christ. Peek, who

¹ Here is Schubart's list; the fragments of Posidippus are cited from the edition contained in the Berlin dissertation of P. M. Schott, 1905. 6 *σελίσιν*: cf. fr. 6 = Athenaeus 596 C Σαπφῶναι . . . αἱ λευκαὶ φθελγόμεναι σελίδες. 7 *σκοπιᾶς*: cf. fr. 1 = D. L. Page, *Greek Literary Papyri* (Loeb Classical Library) no. 104 a, 1.3 *σκοπιᾶ*. 8 *βαίνετε*: cf. fr. 2 = Page, *op. cit.*, no. 104 b, 8 Ἑλλήνων ἀναῖ βαίνετε θυγατέρες (we may also compare Page, *op. cit.*, no. 105 a, 15 . . . μετ' εὐνομίης βαίνετε κρηναῖδες (the poem may be by Posidippus). 14 *ἐπὶ ἡΐσων* cf. fr. 1.3 (see also fr. 21 = AP ix 359.4; fr. 26 = AP v 209.5). 15 *ἡρόνος*: cf. fr. 26.18. 17 *εἰν ἀγορῇ* cf. fr. 21.1. 18 *ἀηδόνι*: cf. fr. 15 = AP xii 98 τὸν Μουσέων τέττιγα. Schubart also compares the optatives at *ll.* 12, 16, 21 and 22 with those at fr. 8 = AP v 134.4; fr. 17 = AP xii 131.3; fr. 25 = AP v 202.3. We may add that ὦ ἀνα, restored with certainty at *l.* 13, occurs at

fr. 1.1. *ναί* in *l.* 20, compared by Schubart with fr. 14 = AP xii 45.1, is an impossible reading.

All this amounts to very little; in some cases, for instance that of ἡρόνος at the end of the pentameter in *l.* 15, the 'parallels' are of little significance. The style of the poem does not resemble that of the epigrams in general; no wonder, in view of the different nature of its content. It does to a certain degree resemble that of the epigrams on the Pharos and the Arsinoe temple (frs. 1-2 + Page, *op. cit.*, 104 a, b); R. Reitzenstein wrote of 'Posidipp, welcher . . . in den eigentlichen Aufschriften . . . prunkvolle Sprache durchaus nicht verschmäh't'; but that might be due to the comparative similarity of their subject-matter.

² Posidippus is called ὁ ἐπιγραμματογράφος by Σ on Apollonius i 1290, p. 116 Wendel (= fr. 4 Schott, p. 106). See Peek, p. 429.

wrote without knowledge of Trypanis' article, also observed the relevance of the proxeny decree and pronounced the poem to be the work of Posidippus. He adopted the view, allowed by Schubart to be possible, that the poem was copied out from memory; the beginnings and ends of poems, he observed, are often better remembered than their middles, and this might explain the poor state of preservation of the text. Would an imitator, Peek asked, have claimed to be Posidippus of Pella? But there were several places in the text where what was on the tablets could hardly be original; apart from 11, 13, 16 f., this was true of 8, where Schubart could not be right in thinking Dion was referred to. 'Vor dem Schluss', Peek wrote (p. 441), 'der sich von der weichen Stimmung der Verse 18-20 zu gefasster Haltung zurückruft, fehlen ebenso sicher zwei Verse wie nach 10. Mir scheint aber, dass noch darüber hinaus mit Lücken und vielleicht noch Verstellungen gerechnet werden muss.' Peek ends by promising a detailed discussion of the problem; but this has not so far appeared.

The author's claim to be Posidippus of Pella has made necessary a detailed re-examination of the problems presented by the poem. The tablets are extremely hard to read. By the kindness of Professor Werner Hartke, President of the German Academy, and of Dr Wolfgang Müller, Director of the Papyrological Section of the Berlin Museum, I have been provided with excellent new photographs (PLATES IV, V). In dealing with the palaeographical problems presented by the tablets I have been almost wholly dependent on the generous help of my friend and colleague, the Reverend Dr J. W. B. Barns, who has used the new photographs together with those published by Diels and Schubart (1911) in making the transcription of the text which appears below. After the transcription I print Dr Barns's palaeographical comments; then a new text with apparatus criticus; then a line-by-line commentary.

The tablets are each 24×10 cm. in area and 1.5 cm. thick; on the outsides are accounts and the mysterious word *ANTAΣΜΟΔΙΑΝ*. Not only are they imperfectly preserved, but the writer was careless and much of his writing and spelling faulty. Apart from itacistic errors (4, 15, 22, 24), he four times writes δ for τ (5, 12, 14, 15); he five times confuses case-endings (1, 3, 5, 6, 16); he twice inserts a superfluous letter (7.21); he three times omits a letter or letters (9, 13, 18); in l. 7, he writes λ for ν, in l. 16 ν for ρ. As far as l. 14, the writing is in a careful and well-formed literary hand; but at that point, while he was writing the word *Μακηδόνες*, the scribe must have realised with annoyance that he had left himself too little room in the second column of the first tablet; for at that point he goes over to a wildly untidy cursive hand. Like his literary writing, this cursive is fluent and practised; that is why it is not likely to be a schoolboy's hand. But it is often desperately hard to read, and would be so even if the surface of the tablet were less rubbed. The writer repeatedly corrects what he has written, using the cursive for this purpose; sometimes he writes in a correction above the line, and sometimes over what he has originally written. Lack of space has forced him sometimes to write above and sometimes, perhaps, below the line (see 4 and 13 f.); sometimes he runs over on to the raised wooden frame of the first tablet. The last word of l. 16 is written as if it were part of l. 15. In ll. 12-16 the writing on the frame is just like the writing on the wax of the depressed surface, so the frame probably had wax on it at this place. But at l. 20 the writing seems to have finished on the bare wood; the faint remaining traces on the pane are straight scratches on a hard, dry surface. Lines 11 to 14 have several scratches through them. Page says they have been 'savagely crossed out', but to me this looks more like the result of an accident. In the middle of l. 10, the writing breaks off. Diels, who took the tablets to contain the author's original rough copy, thought the author stopped because he could not make up his mind what to write. But if the writer was writing out the poem from memory, he will have stopped because his memory failed him.

- A i 1 ειτικαλονμουσαιπολιητιδεσηπαραφοιβω
 2 χρυσολυρεωκαθαροισουασινεκλ[υ]ετε
 3 παρνησουνιφοεντοσαναπτυχ[α]σηπαρο[υ]υμπω^λ
 4 βακχωταστρειετισαρχομεναιθυμελ[. []]
^σ
 5 νυνδεποσε[ι]διππουστνυγερονσυναεισαδεγηρασ
 6 γραψαμεναιδελτου[σ]ειχρυσεαισσελιν
 7 λιμπανεντεσκοπιασελικωλιδεσεισδεταθηβησ

^{πι-[λ]ε ιη} ^{ετε . . . αλαδες}
 8 τειχεα[. . . [.] . αι]σβαιν[ευ. [.]ηστηλεδα . []]
 9 καισυποσιδιππονποτεφιλακυνθιελητους
[?]
 10 νεκαε . . εβ (rest blank)
 10A [.] . [.] [.] [.]

^{νι}
 ii 11 φημητι[μη]φιεντοικειατουπαριου
^{'σμ'} ^{ναχησαι}
 12 τοιηνεκχρηζωντεκαιεξαδυνδωνκα|

^ε
 13 φωνηναθατηνω . . . ακαικατ[ι]μου
^{ω . . .} ^{οιδε|πιν . . .}
 14 οφραμετιμησ[ου]ντεμακηδονεσ|
^{'το' ε}
 15 οιδασισησπασησγιν[.]σηιονοσελίσσ|ων
 16 πελλαιουγενοσαμονεοιμιδεβιβλο|ν
 17 αμφωλαοφορωικειμενοσειναγορ| . .
 18 αλλεπιμενπαρηιδοςαηδονιλυγρονεφ|. . . .
[?] ^{ναχεω|[ν]} [?]
 19 νημακατακλυνεωνδακρυνακει|[. . .]ν

^{|.}
 20 καιστεναχωνδιεμονδεφιλονστομα|. . . . []
 20A . . . [.] []
 20B [.] []
 B 21 μηδετισουνχευαιδακρυνοναυταρεγωι

- 22 γηραιμιστικονοιμονεπειραδαμανθυνηκοιμην
 23 δημωικαιλαωιπαντιποθεινοσεων
 24 ασκειπωνενποσσικαιορθοεπησανομειλον
 25 καιλειπωντεκνοισδωμακαιολβονεμον

Underlines show cursive writing.

| = edge of frame.

3 λ written on top of ν.

4 Several letters scratched out at end, and σ written for some reason below (though there is room for it above) the line.

6 σ struck through. δελτων not written.

8 Dr Barns now thinks Πει[λ]ειης right as the corrected reading. He had at first doubted the second π, but found other cases of π with curved right limb; he had also doubted ειης owing to what seemed to be a κ read in the original writing underneath; it is α. After βαν he reads not ο under ε, but another ε. Before αλαδεις is a letter which in this writer's cursive style could be ε, π or σ. It is not τ; the curve to the right which must be what led editors to read it as τ is, Dr Barns thinks, part of σ in the original writing. The letter before αλαδεις is preceded by what might be ε or σ: before this there is a hole in the wax, having below it the tail of a letter (ι, ρ, ν, φ?): it slopes, so that since this writer's literary hand is upright or even slopes backwards a little, it presumably belongs to the cursive, connected writing. At the end, the original writing reads τηλεδα[: the rest is scratched out (cf. l. 4). The last letter (after α) might be almost anything.

10 All the letters suit ν<ι>· ἐκαε[ργ]ε except that one would have to suppose the ρ to have been very depressed. But all the letters in this line are very badly formed; the line, too, is crooked, sloping sharply downwards; it may represent the writer's unsuccessful attempt to recall a line he had at first altogether forgotten. The γ also is very oddly shaped. The β, if rightly read, is of the cursive type found in βίβλον in l. 17; then perhaps ε; then a trace conceivably of λ, but most uncertain.

10A Before ρ: η or π, or two letters? Before ω: ν seems clear in Schubart's photograph, but less so in the new one. Penultimate letter of line: θ, ο or σ?

11 νι written (apparently) on top of μη.

12 ι of και evidently added later. σμ written above ζ, clear both in Schubart's photograph and in the new one. But there are a couple of unexplained marks above σ, to right and left. Dr Barns sees no trace after ναχησαι: if there were any trace, it would be on the wooden pane.

13 Above αθατην, no supralinear addition. After τηνω, either π or τι, not ν: if it were ν, there should be a trace of the diagonal. Then α, rather small: then κα, the α being cursive; the scribe is still writing in his literary hand, but has lapsed into cursive while writing και, a slip not uncommon with this word in literary manuscripts. Above πα, something may have been added above the line, perhaps ε: cancellation scratches are so thick here that they might cover it. Then κα—faint, but in the new photograph certain: Trypanis' suggestion (p. 67 n. 7) that we read ποτ' is as objectionable on grounds of palaeography as it is on grounds of sense.

The ε after καρ was altered, either from ι or from a letter left unfinished.

14 Above the line, where σι has been read, there seems to be three letters. But perhaps the first is a fortuitous mark: the other two might well be σι.

'το' ε

15 γιν[.]σ: ε written on something (perhaps ο) that was begun but not finished.

17 After ἀγορ, undistinguishable scratches on the wooden frame (perhaps ηι?).

18 After εφ, α or ω, hardly ο. On the frame, some very faint scratches; conceivably ρασ, but what can the triangular object at the end be?

19 Dr Barns thinks the first letter is a ν so imperfectly formed that it looks very like λ: but the new photograph reveals a trace of a final up-stroke not visible in those of Diels and Schubart. After δακρυα, κε is certain, exactly as in κειμενος (l. 17) and ασκειπων (l. 24). Of the following letter only a trace is visible, but this agrees well with ι. Above, before χ, α is very likely; the letter before that is very faint, but may be ν. Scratched on the frame, after about three letters' space and near the edge, is what looks like an ν. This is puzzling; did the scribe write ναχεων twice?

20 δια certain. After στομα, the writing continued on the dry wood of the frame. Parallel with the end of the line on the wax is a rough hard grain in the frame, impossible to write on; but above this as well as below it there seems to

- be writing. In the upper line there are very faint scratches: . . κα .? Below, clearer but puzzling, is a group of marks: is this ασκ[?]?
- 20A The first letter may be α or λ: the second ε or σ: the third τ. Then a gap of three letters: the letter after that may be ρ. Then a gap of about five letters: the next letter may be α. Then a gap of about two letters: then perhaps ο or σ.
- 20B Gap of two letters, then perhaps μ. After one more letter's space, perhaps ωρ: two more letters' space, then perhaps ν. Nothing more is visible.
- 1 Φοῖβον Diels. φοῖβου II.
 3 Ὀλύμπου Ll.-J.: Ὀλύμπου ceteri omnes.^{2a}
 4 τριε<ε>ίς Diels.
 5 Ποσε<ι>δίππων Diels. συναείσατε W.-H. Friedrich ap. Heitsch: συναείσατε Diels.
 6 δέλτων Schubart: δέλτους Diels.
 7 λιμπάνετε Diels. Ἐλικωνίδας Schubart.
 ετε . . . αλαδες
 8 βαν[ευ . [.]ηστηλεδα.[.]] Barns: βαινετεσταλαδες
 ex βαινουσηστηλεδα factum legit Schubart.
 9 κει Ll.-J. ἐφίλα<ο> suppl. Diels.
- 10 υ<ι> Schubart. ἐκάε[ργ]ε β . . . Barns.
 12 ἐκχρήσ<α>ις Schubart (qui legit εκχρηζοντ):
 ἐκ χρησμών Barns (vid. transscr.). καναχήσαις
 Schubart. ἀθανάτην suppl. Diels. ὦ ἄνα Beazley
 ap. Page (ὄνα Diels): ὦπα aut ὦτια Barns: ὦτια
 Schubart. κατ' Barns: [κα]τ' priores.
 14 τιμησώσι (ex τιμησωντε factum) legit Schubart.
 οἱ τ' Schubart. ηῃσ[ων] Schubart: ν . . . Barns.
 15 οἱ τ' Schubart.
 16 Πελλαῖον Schubart.
 17 ἄμφω obscurum.
 18 Παρ<ι>ηι Ll.-J.
 19 ναμα legit Diels: νημα legit Barns: λημα legit
 Schubart. γληνέων Schubart. κεινα Barns: θε[ρ]μά
 priores. χέω[ν] Schubart (1911), Barns: χέω
 ceteri.
 20 στενάχων Diels. δι' Schubart (1911): στενάχοι,
 ναί Schubart (1932).
 21 ἐγὼ Diels.
 22 μυστικὸν Diels. οἶμον Ll.-J.: οἶμον ceteri.
 24 ασκιπων Diels. ἀν' ὅμιλον Schubart: ἐν ὁμίλῳ
 fort.

Εἴ τι καλόν, Μοῦσαι πολιήτιδες, ἢ παρα Φοῖβου

χρυσολύρεω καθαροῖς οὔασιν ἐκλ[ύ]ετε

Παρνησοῦ νιφόεντος ἀνὰ πτύχ[α]ς ἢ παρ' Ὀλύμπου

Βάκχῳ τὰς τριε<ε>ίς ἀρχόμεναι θυμέλα[ς,

5 νῦν δὲ Ποσε[ι]δίππων στυγερόν συναείσατε γῆρας

γραφάμεναι δέλτων ἐν χρυσέαις σελίσιν.

λιμπάνετε σκοπιάς, Ἐλικωνίδες, εἰς δὲ τὰ Θήβης

τεῖχεα Πιπ[λ]εῖης βαίνετε † . . . αλαδες†

κεῖ σὺ Ποσ<ε>ίδιππὸν ποτ' ἐφίλα<ο>, Κύνθιε, Λητοῦς

10 υ<ι> Schubart. ἐκάε[ργ]ε β . . .

10A [.] . [.] ραν[.]νω

†φημητινιφιεντοικεια† τοῦ Παρίου.

τοῖην ἐκχρήσ<α>ις τε καὶ ἐξ ἀδύτων καναχήσαι[ς

φωνὴν ἀθανάτην, ὦ [ἄν]α, καὶ κατ' ἐμοῦ,

ὄφρα με τιμησώ[σι] Μακηδόνες οἱ τ' ἐπὶ ν[ή]σων

15 οἱ τ' Ἀσίης πάσης γ<ε>ίτονες ἡῖόνος.

Πελλαῖον γένος ἁμόν · ἔοιμι δὲ βίβλον ἐλίσσων

ἄμφω λαοφόρῳ κείμενος εἰν ἀγορ[ῇ].

^{2a} But see p. 99 n. 38.

ἀλλ' ἐπὶ μὲν Παρσίῃ δὲ ἀηδόνι λυγρὸν ἔφ' . . .

νᾶμα κατὰ γληνέων δάκρυα κεινὰ χέω[ν

20 καὶ στενάχων, δι' ἐμὸν δὲ φίλον στόμα[

20A . . .[. . .][

20B [. . .][

μηδέ τις οὖν χεύαι δάκρυον. αὐτὰρ ἐγὼ

γῆραι μυστικὸν οἶμον ἐπὶ 'Ραδάμανθυν ἰκοίμην

δήμῳ καὶ λαῷ παντὶ ποθεινὸς ἑών,

ἀσκήπων ἐν ποσσὶ καὶ ὀρθοεπὴς ἄν' ὄμιλον

25 καὶ λείπων τέκνοις δῶμα καὶ ὄλβον ἐμόν.

Words and phrases capable of having been used by a good writer of Hellenistic date are often, of course, used by later writers, and Schubart has actually contended that the author of the imperial period to whom he ascribed the poem was trying to copy the manner of Hellenistic elegy and inserted phrases taken from Posidippus and perhaps others among its celebrated exponents into his work. The presence of a number of such phrases is not sufficient to prove the authorship of Posidippus. To establish this it would be necessary to show that the poem contained no feature impossible in an author of Hellenistic date that could not be accounted for by its imperfect state of preservation, and also that it contained features hard to explain if it were the work of a late imitator.

COMMENTARY

1. *πολιήτιδες*. For this word in this place in the line, cf. Anyte, *AP* vii 492.3; see also Apollonius, *Arg.* i 867. Why does the poet so address the Muses? A native of Pella might presume to do so on the score of that place's neighbourhood to Pieria, which is usually regarded as their birthplace (see on *l.* 8 below). But in *ll.* 7-8 the poet summons the Muses to Thebes; and if Boeotian Thebes is meant, we must reckon with the possibility that he is a native of Boeotia who is claiming them as fellow-citizens on the score of their being at home on Helicon. See the notes on that passage.

2. *χρυσολύρεω*. This epithet of Apollo is found first at Aristophanes, *Thesm.* 315 (Pindar, *Pae.* v 41 is wrongly cited by *LSJ* as an instance of *χρυσολύρης*: in fact it has *χρυσοκόμας*). It recurs at *Hy. Orph.* 34.3 and in some elegiac verses in an epigram of A.D. 263 (*SEG* iv 467.20). Orpheus is so called in two epigrams (1. *Ps.-Arist., Peplos* 48.1 (Diehl, *Anth. Lyr.* ii 178) = Kern, *Orph. Frag.*, Test. 124. 2. Lobon fr. 7 Crönert = *AP* vii 617 = Diog. Laert. 1.5). Apollo's lyre is golden first at [Hesiod], *Scut.* 203; see O. Schroeder on Pindar, *Pyth.* i 1; his *πλήκτρον* is golden at *Hy. Hom. Ap.* 185, Euripides, *Heracles* 351, etc. See on *l.* 6 below, and cf. Callimachus, *Hy.* 2.32 f. with Wilamowitz, *Hellenistische Dichtung* ii 83.

καθαροῖς οὖασιν means not 'pure, holy ears' but 'clean ears'. People who are slow of apprehension are said to have ears that are blocked up with wax; see Sophocles, fr. 858 with Pearson's note, and add to his examples Eupolis, fr. 213 Kock; *Com. adesp.* 620 Kock; Lucian, *Lexiphanes* 1. Conversely, a person who is quick in the uptake is said to have clean or well-drilled ears; Plutarch, *Quaest. symph.* 631 D says *τετρυνπημένον* . . . *ἔχεις τὸ οὖς*, and Antipater (of Thessalonica?), *AP* vii 409.3 says *εἰ τορὸν οὖας* | *ἔλλαχες*, meaning 'if you are an acute critic'. When at *Mil. Glor.* 774 Periplectomenus says *perpurigatis damus tibi operam auribus*, the expression probably comes 'aus der italischen Volkssprache' (so

Kiessling-Heinze on Horace, *Epist.* i 1.7); for all we know, it may have existed independently in both languages. But Persius (5.63; 86) probably took it from Horace (*loc. cit.* and *Epist.* i 2.63) and the Augustan poets will have found it in their Alexandrian exemplars. When Propertius³ (ii 13.12) writes

*me iuuat in gremio doctae legisse puellae
auribus et puris scripta probasse mea*

he is complimenting Cynthia not on her purity, but on the critical judgment proper to a *docta puella*; he may well have found a similar phrase in Hellenistic poetry. If our poet is a late imitator, he must have felt satisfaction at working in this highly idiomatic expression.

3. The name of Parnassus has regularly an *eta* in Hellenistic hexameter and elegiac verse, as in most manuscripts of the *Odyssey*, at Hesiod, *Theog.* 499 and in the Homeric Hymns to Apollo (269, 282, 396, 521) and Hermes (555). It has two sigmas at Callimachus, fr. 75.27 (*P. Oxy.* 1011) and at Apollonius *Arg.* ii 705, but one at Callimachus, *Hy.* 4.93 and Theocritus vii 148; so the single *sigma* may be kept.

Diels compares Sophocles, *OT* 474 τοῦ νιφόμεντος . . . Παρνασσῶ. κατὰ πτύχας occurs at this place in the verse at *A* 77 and at Apollonius, *Arg.* ii 992 and iii 113; cf. *Hy. Hom. Ap.* 269, *Hy. Hom. Merc.* 555 ὑπὸ πτυχὶ Παρνησοῖο.

4. ἄρχομαι in this sense normally takes a genitive; but Diels quotes Pindar, *Nem.* iii 10 ἄρχε δ', οὐρανοῦ πολυνεφέλα θυγάτηρ, δόκιμον ὕμνον.

Note the transitive use of κατάρχεσθαι at Euripides, *Orestes* 960 and its use as a passive at *Heracles* 750, 891; and also the ritual use of ἐνάρχομαι and ἐξάρχομαι with κανοῦν or κανᾶ as object.

A. S. F. Gow (*JHS* xxxii (1912) 213 f.) first showed that the basic sense of θυμέλη is equivalent to that of εἰσχάρα. Fernand Robert, who takes the same view, gives a detailed description of the word's use (*Thymélé* (Paris, 1939) 259 f.). 'Il vint un moment', he writes (p. 289), 'où on qualifiait de θυμελικός, par opposition à σκηνικός, une partie de ce qui, au théâtre, était démonstration musicale, oratoire ou poétique, sans rapport avec l'art dramatique proprement dit'. This moment seems to have been as early as the fourth century before Christ.⁴ We first learn of θυμελικοὶ ἀγῶνες from the inscriptions relating to the reorganisation during the twenties of the third century of the Museia at Thespieae and the Ptoia at Ptoion.⁵ Auletic and aulodic contests formed part of these trieteric festivals in honour of Apollo, Dionysus and the Muses. Robert has observed (p. 295) that the earliest inscriptions that speak of θυμελικοὶ ἀγῶνες all refer to Boeotia. In *ll.* 1-4 it is clearly not dramatic activities, but lyre-playing and pipe-playing, that are in question; and if this poem is really by Posidippus, it contains the earliest known use of θυμέλη with reference to such activities. It would, of course, be absurd to suppose that Posidippus could have imagined Apollo and Dionysus as taking part together with the Muses in the Boeotian festivals of his own time. But if he wrote this poem, and if he was at Thebes when he wrote it (see on 7-8 below), it seems possible that his choice of the word θυμέλας was dictated by this local association. A poet of the imperial period might have taken the word θυμέλη from earlier verse without knowledge of its special history; but this seems less likely, as will be clearer at the end of our examination of the text.^{6a}

³ Cf. D. R. Shackleton Bailey, *Propertiana* 284 (though the quotation from Achilles Tatius is not an instance of this usage). We may compare the use of 'naris emunctae', applied to Lucilius by Horace (*Sat.* i 4.8) and to Aesop by Phaedrus (3 3.14).

⁴ See J. Frei, *De certaminibus thymelicis* (Diss. Basle, 1900) 14 f.

⁵ See M. Feyel, *Contribution à l'épigraphie béotienne* (Le Puy, 1942) 88 f., 113 f.; and, for the most

up-to-date text of the important Amphictyonic decree, see J. Bousquet, *BCH* lxxxv (1961) 78 f. H. Usener's remarks at *Rh. Mus.* xxix (1874) 37 f. = *Kl. Schr.* iii 389 f., are still useful.

^{6a} We have scraps of what is probably a Hellenistic poem which seems to allude to these festivals in P. Heidelberg 189 (E. Siegmänn, *Literarische Griechische Texte der Heidelberger Papyrussammlung* (1956) 25). Note l. 4 Ἀρσινόη: l. 11 Ἐλικόνιον:

1-4. Let us now survey the opening lines from a more general aspect. They take the traditional form of the κλητικός ὕμνος.⁶ The appeal to the Muses to give the poet his song rests on the ancient notion that the Muse, or Apollo, teaches the poet (*Odyssey* viii 488; *Hy. Hom. Ap.* 518). The many poetic invocations of the Muses have lately been studied in detail by the late Walther Kranz.⁷ He observes (p. 5) that the invocation of the Muses is often combined, from Hesiod's time on, with his introduction of himself. The Muses are often called on to inspire the poet with his song; that rests on a conception of them that is old as the *Odyssey* (iv 888), the proem to Hesiod's *Theogony* and the Homeric Hymn to Apollo (518). In this case the invocation takes the conventional form of the ὕμνος κλητικός (see notes on 7-8, 9-10). Apollo as Μουσηγέτης is a natural source of the Muses' inspiration; and Dionysus also has a close connexion with the Muses;⁸ the use of the word θυμέλη may, as I have suggested above, recall the Boeotian musical festivals in honour of these deities.

In l. 3, the words παρ' Ὀλύμπῳ raise an awkward problem. Trieteric festivals in honour of Dionysus were held in many parts of Greece; but none, so far as we know, was held on Olympus, and the best known of all, the subject of innumerable allusions in poetry, was held upon Parnassus. From the point of view of sense the words are suspect. They are also suspect from that of language; for to express the sense 'on Olympus' one may say ἐν Ὀλύμπῳ or κατ' Ὀλυμπον, but not παρ' Ὀλύμπῳ. Is this awkwardness to be set down to the incompetence of the poet? There is an easy way of removing it; which is to suppose that, just as in l. 1 the scribe has written Φοιβῶν for Φοιβον and in l. 5 he has written ποσειδιππου for ποσειδιππῳ, so here he has written οὐλυμπῳ for ολυμπον, this being the name not of the mountain in Thessaly but of the Phrygian pipe-player, the father or in some accounts the lover of the satyr Marsyas. The poet is expressing the familiar disjunction 'a song either for the lyre or for the pipe'. The lyre is the instrument proper to Apollo, the pipe to Dionysus. At Apollo's festival the god himself provides the music; at that of Dionysus, another player must be found. Which famous piper of mythology is this to be? Pan as a divinity in his own right is not suitable; Marsyas as the adversary of Apollo is ruled out. We are left with Olympus, whose celebrity as a piper cannot be questioned. Pindar (fr. 157 Snell = 175 Turyn) showed him in conversation with Silenus; the fact might be held to indicate a Dionysiac connexion, but since he was a Phrygian Olympus may have been present simply as a member of the entourage of Midas. Euripides, *IA* 576 takes Olympus as the paradigm of the great pipe-player of mythology: he describes Paris on Ida as

βάρβαρα συνρίζων, Φρυγίων
αὐλῶν Ὀλύμπου καλάμοις
μιμήματα πνεύων.⁹

l. 13 ἤρισα Θεσπιάδαις; l. 17 χάριτες; l. 18 κλοῖραν; Ὀρχομενοῦ. In l. 11, we must surely read πενθερόν. See *Gnomon* xxix (1957) 426.

⁶ On the ὕμνος κλητικός in general, see Eduard Fraenkel, *Philologus* 86 (1931) 3 f.

⁷ In his article 'Sphragis' in *Rh. Mus.* 104 (1961) 3 f. and 97 f., esp. p. 4 f. See also W. F. Otto, *Die Musen und der göttliche Ursprung des Singens und Sagens* (Düsseldorf, 1955), and E. R. Curtius, *Europäische Literatur und Lateinisches Mittelalter*, 235 f.; F. Jacoby, *S.B. Berlin* 1931, 102 f. = *Kleine Philologische Schriften* i 360 f. For the appeal to the Muse to come bringing a particular song, cf. Cratinus fr. 222.1 ἔγειρε δὴ νῦν, Μοῦσα, Κρητικὸν μέλος; Ar., *Ach.* 665 f. See Fraenkel *loc. cit.*, p. 6.

⁸ On this connexion, see E. Maass, *Hermes* 31 (1896) 375 f.

⁹ Aristophanes, *Eq.* 9 uses in a trimeter the epic form of the name: ξυναλῶν κλαύσωμεν Οὐλύμπου νόμον. Why? 'Because the rhythm and tone are tragic', says R. A. Neil in his commentary (Cambridge, 1901; a book that deserves to be better known outside England than it is). But even in a tragic trimeter one would not expect this epic form. Was the epic form of this name regularly used during the fifth century, perhaps because the person in question was best known from epic verse of some sort? In the Euripidean passage just quoted, Heath's conjecture Οὐλύμπου may be correct.

On Olympus, see further A. Hecker, *Commentatio*

This yields a far more satisfactory pair of alternatives; and the words *Παρνησοῦ νιφόεντος ἀνὰ πύχας* can be taken with both clauses.

5-8. In view of the possibility that the poem was written out from memory, we must consider the case for believing that 7-8 should properly precede 5-6. It might be contended that the command to the Muses to leave their present haunts and repair to where the poet is would more naturally precede the request to help him with his song. Further, with the lines in their present order we have *δέ* in *apodosis* at the beginning of *l.* 5 and asyndeton at the beginning of *l.* 7; whereas if we transpose we have an *apodosis* following the 'if' clause in the normal way in the former place and *δέ* serving to attach the next sentence in the normal fashion in the latter. These considerations do not suffice to make out a case for transposition. There is no imperative reason why the request to the Muses to leave Helicon should not follow the prayer for help. *δε* in *apodosis* is an epicism that is several times affected in the hexameter and elegiac verse of the Hellenistic age. For instances of it following an 'if' clause, see Theocritus i 11 (with Gow's note); xxix 17; Callimachus, *Hy.* iii 85; Apollonius, *Arg.* i 681; in view of the features of epic style presented by this poem, it may well have occurred here.

5. *στυγερόν . . . γῆρας*. *γῆρας* is called *στυγερόν* by Homer, *Il.* xix 336, Apollonius, *Arg.* iv 872; *λυγρόν* by Homer, *Od.* xxiv 250; *ὀδυνηρόν* by Mimnermus, fr. 1.5; *ἀπεχθόμενον* by Pindar, *Nem.* x 85; *βαρύ* by Euphorion, fr. 53; cf. the *locus classicus*, Sophocles, *OC* 1236; on Euripides, *Heracles* 638 f. and Callimachus, fr. 1.33 f., see below.

The tablet's *συναείσαδε* is emended by Diels to *συναείσατε*. This word is found in an appeal to the Muses at Theocritus x 24; *Hymn. Epidaur.* 3 (= *IG* iv 1.131) = fr. adesp. 935 Page (*PMG* p. 502), *l.* 3 (in *l.* 1 Peek reads *Πιμπληιάδε]s θεαί*, Wifstrand *ὦ Πιερίδε]s θεαί*): Musaeus 14; see also Nonnus, *Dionys.* 8.24. All subsequent editors have put *συναείσατε* in the text; but W.-H. Friedrich (ap. Heitsch) has pointed out that *συναείρατε* is a possible reading that is worth considering.¹⁰ *συναείσατε* certainly presents some difficulties. Firstly, it seems to imply that old age is the subject of the poem, and this is not the case. Secondly, can the poet really say to the Muses, 'If you have heard any beautiful song from Apollo or Dionysus, then sing with me of old age'? How can he be sure that whatever song the Muses may have heard will suit this subject? Thirdly, can the poet say to the Muses, 'Sing with me of old age, writing down the song on the golden columns of your tablets'? Is it not more natural for him to say, 'Help me to bear the burden of my old age, writing down that song . . .'? *συναείρατε* is rather further than *συναείσατε* from the letters in the manuscript; but the argument so far seems to indicate that it should be preferred. But we must suspend judgment on this point. Suppose *ΓΗΡΑΣ* were the title of a collection of poems of which this was the *σφραγίς*, or seal-poem, *συναείσατε γῆρας* would be a natural expression; and I shall argue below that this poem was in fact the seal-poem of a collection. See p. 96 below.

The tablet, like the roll, is a familiar attribute of the Muses in Greek art from the fifth

critica de Anth. Gr. i (1852) 60 f.; Preller-Robert, *Griechische Mythologie* i 732; Weizsäcker in Roscher's *Lexikon*, s.v.; Wegner in *R.-E.* xviii 1 321 s.v. 'Olympos' (26).

Dionysiac music is regularly played by Phrygian flutes. See Euripides, *Bacch.* 126 f., with Dodds's note. Cf. 159 f.

ἐν Φρυγίαισι βοαῖς ἐνοπαῖσί τε,
λωτὸς ὅταν ἐνκέλαδος
ἱερὸς ἱερὰ παίγματα βρέμῃ . . .

Dodds, *ad loc.*, says, 'ἐνοπαῖ are always loud or excited cries'. Not always; like *βοή* (*Il.* xviii 495;

Pindar, *Ol.* iii 8; and *Pyth.* x 39), ἐνοπή can be used of the sound of musical instruments (*Il.* x 13 and now Pindar, *P. Oxy.* 2451, fr. 14, i 9 (see Lobel, *ad loc.*, Pt. xxvi 171). I think it likelier than not that the sound of the pipes, and not human cries, is referred to by βοαῖς ἐνοπαῖσί τε here. Cf. Euripides, *Trö.* 544-5 Λιβὸς τε λωτὸς ἐκτύπει | Φρύγιά τε μέλα.

¹⁰ The middle of this verb is far commoner than the active in the sense of 'help'; but cf. Dio Cassius 55.66; *id.*, 46.3; Philoponus in Procl. 2. In this instance the active would present no particular difficulty.

century on.¹¹ Objects belonging to the gods are often called golden from Homer on (Apollo's lyre and plectrum are notable examples; see p. 81 above); and when the poet writes 'in the golden columns of their tablets', he is employing an easily understood hypallage and means 'in the columns of their golden tablets'.¹² Just so an inscription of the second or first century before Christ in Cos speaks of the 'golden tablets' of Homer (Peck, *Griechische Versinschriften* i 1729.1-2)

[π]ρὶν μὲν Ὀμήρειοι γραφίδες φιλοδέσποτον ἦθος
Εὐμαίου χρυσέαις ἔκλαγον ἐν σελίσιν.

For σελίς and δέλτος in the context of a poet's initial invocation to the Muses, cf. *Batrachomyomachia* 1-3; for parallels, see Ludwig *ad loc.* (p. 319).

7. The appeal to a god to leave his accustomed haunts and come to where his worshipper is praying to him is too familiar to need illustration. In primitive times the worshipper thought it necessary to call upon the god by all his possible names, and also to specify all the places where the god might be in order to make sure of being heard; and long after this belief was extinct, both practices survived as a regular feature of the ὕμνος κλητικός. We may compare *Il.* xvi 514 f.; Aeschylus, *Eum.* 287 f.; Aristophanes, *Nub.* 270 f., etc.; at Theocritus i 123 a god (Pan) is asked to leave one of his haunts and come to where the singer is, just as the Muses here are asked to leave Helicon and come to Thebes:

ὦ Πάν Πάν, εἴτ' ἐσσι κατ' ὄρεα μακρὰ Λυκαῖαν,
εἴτε τύγ' ἀμφοπολεῖς μέγα Μαίναλον, ἔνθ' ἐπὶ νᾶσον
τὰν Σικελάν, Ἑλίκας δὲ λίπε ρίον αἰπύ τε σᾶμα
τῆνο Λυκαονίδαο, τὸ καὶ μακάρεσσιν ἀγῆτόν.

Because of the frequency of these two modes of address, clauses coupled by εἴτε . . . εἴτε . . . or *sive* . . . *sive* . . . are particularly common in the ὕμνος κλητικός: see E. Norden, *Agnostos Theos* 144 f. The opening sentence of our poem (*Il.* 1-4) is not an instance of either; but it is possible that the form it takes, that of a hypothetical clause stating two alternatives (εἰ . . . ἢ . . . ἢ . . .) is suggested by the prevalence of similar clauses at the beginning of ὕμνοι κλητικοί.

λιμπάνω occurs first in epic verse at Aratus, *Phaen.* 128; but it occurs in *P. Tebtunis* 266 as a variant at *Il.* xi 604; and cf. ἀπυλιμπάνω at Sappho fr. 94.1 and καταλιμπάνω *ibid.*, 5 (the latter not in the *index verborum* of Lobel-Page, *PLF*). προλιμπάνω occurs at Peck, *op. cit.*, 1752.1 (Demetrias, third or second century B.C.) and καταλιμπάνω *ibid.*, 1861.7 (Leontopolis, first century A.D.) (this for lexicographers).

Schubart's suggestion that Ἑλικωνίδας should be read may well be right; but it is not quite safe to put in the text. If the Muses are being addressed as Ἑλικωνίδες, as they very often are, it will be clear which peaks are meant. Euripides, *Heracles* 791 is wrongly given by LSJ as an instance of Ἑλικωνίς; in that place Ἑλικωνιάδων is right, but at Sophocles, *OT* 1108, where the manuscripts have Ἑλικωνιάδων, Ἑλικωνίδων is proved right by the responson. Cf. Limenius, *Paeon Delphicus* ii 3 (p. 149 Powell): Πιερίδες, αἱ νιφοβόλους πέτρας ναίειθ' [Ἑλ]ικωνίδ[ας, βαίνετε. See p. 76 n. 1. Gods are of course regularly asked to come to particular places in cletic hymns: see F. Jacoby, *S.B. Berlin* 1931, 96 = *Kleine Philologische Schriften* i 353, n. 4.

7-8. τὰ Θήβης | τείχεα. Cf. Theognis 1.1209 πόλιν . . . εὐτειχέα Θήβην. At the beginning of *l.* 8 Diels read τείχε' [Αἰ]γυπ[τ]ίης. Schubart in 1911 read τείχε' Ἀγυπ[ο]ρήης.

¹¹ See O. Bie, *Die Musen in der griechischen Kunst* (Berlin, 1887) 15 f., 69 f.; *id.* in Roscher's *Lexikon*, s.v. 'Musen' (especially the vase Berlin 2388, reproduced on p. 3246); Maximilian Mayer, in *R.-E.* xvi (1935) 692 f. (s.v. 'Musen'). Cf. J. D. Beazley, *AJA* lii (1948) 337 f.

¹² See P. Jacobsthal ap. Dodds on Euripides, *Bacch.* 553-5; Shackleton Bailey, *op. cit.*, p. 296 (add to his instances Ovid, *Ars. Am.* ii 493-4).

Since Agenor was the father of Cadmus, this epithet would have suited Boeotian Thebes; but in 1932 Schubart read Πιπ[λ]είης, a lection which Barns thinks probably correct.¹³ Schubart took for granted that this word was attributable to Θήβης. 'Pipleian Thebes', he suggested, was a poetic way of referring to Dion in Macedonia, which was not far from the Muses' Macedonian home in Pieria, in which Pi(m)pleia and Leibethron were situated. Against Diels's view that Egyptian Thebes was meant Schubart objected that in the first century A.D. Egyptian Thebes was an unimportant place, unlikely to have statues of poets in its market-place. Page accepted 'Pipleian Thebes', but took it to refer to Thebes in Egypt. He reasonably observed that the statue the poet asks for is in all probability meant to be not at Thebes, but at Pella. But Schubart further argued that the outlook of 15-16 was that of a man living in Greece, not in Luxor or Karnak. 'The outlook of the Macedonian', Page replies, 'does not change because he happens to be staying for the time in Egypt.' In favour of Schubart's view one may point out that the mention of the Macedonians, the islanders and the inhabitants of the coast of Asia Minor seems somewhat more appropriate to the political conditions of the Hellenistic age than to those of the early imperial period; but the argument is not decisive.

Schubart, Page and Heitsch all assume that Πιπ[λ]είης is attributive to Θήβης. Can we be sure of this? At the end of the line, they both put in the text Schubart's conjecture <Κα>σταλίδες. A glance at what is actually to be made out on the tablet shows that this conjecture is very far from certain. The writer seems to have begun by writing what looks more like a hexameter; he then altered the reading to βαίνετε followed by a word ending in αλαδες. I have failed to find any satisfactory solution of this problem. But I should expect Πιπ[λ]είης to have been followed either by a noun meaning something like 'daughters of', 'dwellers in', or (likelier) 'ones born in'¹⁴ (sc. Pipleia), or else perhaps an adverb or a group of words containing an adjective or adverb on which Πιπ[λ]είης depended. If the former possibility were correct, the line would somewhat resemble the two lines quoted in n. 1.

The second possibility may derive a faint encouragement from the presence of τηλεδαπ among the letters written first and then deleted. But in the present state of the evidence we cannot know what was written; and it is wrong to put <Κα>σταλίδες in the text.¹⁵

It seems probable that the poet is summoning the Muses from Helicon to Boeotian Thebes. If he was the well-known Posidippus, this is surprising; for we have no reason to connect him with this city.

Still, we have no reason to assume that he was never there; and there is even a faint possible indication that he may have taken an interest in the place. Athenaeus 591 C says that Posidippus wrote an Αἰθιοπία (or, if Kaibel's conjecture is right, an Αἰθιοπῆς), but at 491 A that he wrote an Ἀσωπία. Schott (*loc. cit.*, p. 99 f.) ingeniously argued that both titles were corrupt for Αἰσωπεία; his conjecture rests on the fact that Posidippus mentioned Doricha, otherwise known as Rhodopis, whom we know from Herodotus to have been a fellow-slave of Aesop (ap. Athenaeum 596 c = fr. 6 Schott = Geffcken, *Griechische Epigramme*, no. 256). This ingenious speculation received the blessing of Wilamowitz (*Hellenistische*

¹³ Schubart thought it necessary to read Πι<μ>π[λ]είης: Page and Heitsch have done likewise. But it looks as if the form without μ were possible; cf. Hesychius Πιπλῖαι · αἱ Μοῦσαι ἐν τῷ Μακεδονικῷ Ὀλύμπῳ, and the Piplei or Piplea of the manuscripts at Horace, *Od.* i 26.9.

¹⁴ The nymph Pimpleis is the mother of the Muses according to Epicharmus fr. 41 Kaibel. But 'you that were born in' is likelier. The Muses are at home on Helicon, but they were born, according to the best

authorities, in Pieria; cf. F. Jacoby, *S.B. Berlin* 1931, 96 = *Kleine Philologische Schriften* i 353 n. 4. See Callimachus, *Hy.* 4.7-8.

¹⁵ Although I think Κασταλίδες an unlikely conjecture, I do not share Peek's view that this word is 'nicht akzeptabel' as a designation of the Muses. J. Schmidt, *Rh. Mus.* xlv (1890) 148 made it probable that at Theocritus 7.148 Νύμφαι Κασταλίδες Παρνασσίων αἶπος ἔχεισαι refers to the Muses; see Gow, *ad loc.*, who quotes Martial iv 14 *Sili, Castalidum decus sororum*.

9. If we keep *καὶ* before *σὺ* it must be emphatic, not connective. The asyndeton that this involves has a somewhat abrupt effect; and in view of the general nature of the context it seems highly probable that *καὶ* is a mistake for *κεῖ*. If so, we have in these lines an instance of the *εἴ ποτε* formula that is a common feature of prayers and *ὑμνοὶ κλητρικοί*; and we may note a certain resemblance to Horace *Od.* i 32 (on which see E. Fraenkel, *Horace*, 168 f.).

φίλα <ο> (Diels) seems unavoidable; note the use of this aorist form in a similar context at *Il.* v 117; x 280; Hesiod, *Theog.* 95–6 ὁ δ' ὄλβιος, ὄντινα Μοῦσαι | φίλωνται. It occurs in Callimachus *Hy.* iii 185, 189, fr. 43, 53; in Apollonius, *Arg.* iii 66, 1002; iv 990; and at *AP* xi 191. 5 εἰ δέ σε φίλατο Καλλιόπη . . . mentioned in connexion with Apollo at *Hy. Hom. Ap.*

Λητοῦς | υ<ι>'. Cf. *Hy. Hom. Ap.* 545 Διὸς καὶ Ἀθητοῦς υἱέ; Theognis 1. ὦ ἀνα, Ἀθητοῦς υἱέ.

Midway through l. 11, the writing breaks off. Probably the writer had forgotten how it ended.

¹⁸ IG ix 1.17. The second of the lists recorded is dated ἐπὶ στραταγοῦ Πολυκρίτου, i.e. in 263/2 B.C.; so that the first list, the one that includes Posidippus, will belong in 264/3 or a little earlier.

preferable to that of Schubart, which Page and Heitsch both put in the text, the former without warning the reader that it was a conjecture. Schubart emended the text to read *τῇ νυφόντ' οἰκία*: the *τῇ* presumably introduces a relative clause whose verb has to be supplied from the preceding main clause. It is just possible to imagine such a sentence as Schubart's conjecture assumes we have to reckon with; but since it is only a conjecture, it is preferable to refrain from putting so severe a strain on our imaginations.

Whatever case of the word *φήμη* stood in the text clearly had reference to an oracle. That oracle was somehow connected with someone called 'the Parian'. That person was almost certainly the most famous person connected with Paros, the poet Archilochus. This supposition is strongly confirmed by the sense of *ll.* 13-14: 'Even such an oracle, O king, pronounce with regard to me *also*'. The *καί* in *l.* 14 indicates that the poet has just mentioned a person with regard to whom Apollo has in the past pronounced such an oracle as the poet is now asking him to pronounce with regard to himself. That person is, without any doubt, Archilochus.

Which of the various oracles connected with Archilochus does the poet have in mind? We have long known of five such oracles (see A. Hauvette, *Archiloque* 43 f.; H. W. Parke and D. E. W. Wormell, *The Delphic Oracle* ii nos. 4, 5, 230-2; and the Budé edition of Archilochus, frs. A 11-14), but since 1952 we have known that at some time during the third century before Christ Apollo did give an oracle commanding that men honour Archilochus. In that year (*Arch. Eph.* 1952 32 f.) N. M. Kondoleon published a new inscription from the Archilocheion on Paros. The well-known inscription inscribed by order of Sosthenes and published early in the century by F. Hiller von Gaertringen¹⁷ dates from 100 B.C.; the new one was inscribed by order of Mnesiepes, and is dated by the lettering to about the middle of the third century B.C. At the beginning Mnesiepes states that the Archilocheion and the altars it contained were built by him in response to the command of a Delphic oracle.¹⁸ Was this the oracle in question? The rhetor Alcidas, who flourished towards the end of the fifth century before Christ, says that the men of Paros have 'honoured' Archilochus (ap. Aristotle, *Rhetoric* 1398 B 11 = fr. 5 Müller = Archilochus fr. A9 Lasserre). F. Lasserre (Budé Archilochus, p. lxxii) may well be right in supposing that some form of hero-cult already existed as early as the fifth century, and that Mnesiepes merely re-established an existing institution. If so, the original foundation of the cult may well have been commanded by an earlier oracle; it is clear that Delphi's patronage of Archilochus dates from very early. Whatever the date of the oracle our poet had in mind, it is clear that he knew of the establishment of the Archilocheion, and had enough confidence in his own celebrity to ask Apollo to pronounce an oracle demanding similar honours for himself. An obscure local poet living in Egypt during the first century A.D. would hardly have ventured on so impertinent a request. But Posidippus of Pella in the third century might well have done so.

12. Schubart read *εκκρηζον* corrected to *εκκρησις*, and interpreted this as *ἐκκρήσ<α>ις*. This seems to me to accord with the traces; but Barns reads *εκκρηζων* corrected to *εκκρησμων*. If Barns's reading were certain, we would have to accept *κρησμοί* in the sense not of 'oracular pronouncement', but of 'oracular sanctuary'. I can find no instance of such a use, but do not feel certain it cannot have occurred '*libenter Posidippus vocabulorum sensum commutat*' (H. Ouvré, *Quae fuerint dicendi genus ratioque metrica apud Asclepiadem Hedylum Posidippum* (Paris, 1894)). But I prefer Schubart's *ἐκκρήσ<α>ις*, which makes good sense; the verb occurs at Sophocles, *OC* 87. There is no need to consider Trypanis' *ἐκ Κρίσης*,

¹⁷ Archilochus fr. 51 Diehl³: see literature there quoted.

¹⁸ *L.* 14 f. = *Archiloque*, ed. F. Lasserre, fr. 11 A: *Μνησιπέει ὁ θεὸς ἔχρησε λῶιον καὶ ἄμεινον εἶμεν | τιμῶντι Ἀρχίλοχον τὸν ποιητάν, καθ' ἃ ἐπινοεῖ.*

χρήσαντος δὲ τοῦ Ἀπόλλωνος ταῦτα, τὸν τε τόπον | καλοῦμεν Ἀρχιλόχειον καὶ τοὺς βομοὺς ἰδρῶμεθα | καὶ θύομεν καὶ τοῖς θεοῖς καὶ Ἀρχιλόχοι καὶ | τιμῶμεν αὐτόν, καθ' ἃ ὁ θεὸς ἐθέσπισεν ἡμῖν.

which gives sense no better, if as good, at the expense of much greater alteration (*loc. cit.*, p. 67 n. 6).

πίονος ἐξ ἀδύτου occurs at *Il.* v 512, and Apollo prophesies ἐξ ἀδύτου at Tyrtæus fr. 3 a, 2 Diehl; Theognis 808; Pindar *Ol.* vii 32.

καναχήσαι[s] Prophetic utterances are customarily shrieked; see Fraenkel on Aeschylus, *Agam.* 156 (cf. *ibid.*, 201, 1152); Wilamowitz on Euripides, *Ion* 92, etc. *καναχεῖν*, *καναχή* seem not to be used elsewhere of mantic utterance, but they are used of song; cf. *Hy. Hom.* *Ap.* 185 φόρμιγξ . . . *καναχήν* ἔχεν ἰμερόεσσαν : Apollonius, *Arg.* iv 907.

13. *φωνήν ἀθανάτην*. Apollo's oracles are often called immortal; cf. Sophocles, *OT* 158 ἄμβροτε φάμα: 481 *μαντεῖα* . . . τὰ δ' αἰεὶ | ζῶντα περιποτᾶται. In this case the epithet is specially apt, for this oracle will confer immortal fame upon the poet. A Delphic oracle quoted by Mnesiepes (= *AP* xiv 113; see p. 88 above and cf. Parke and Wormell, *op. cit.*, no. 231) promised Archilochus' father that his son should be immortal (*l.* 50 f.):

'Αθά]νατός σοι παῖς καὶ αἰδιδίμος, ὦ Τελεσίκλεις,
ἔσ]ται ἐν ἀνθρώποισιν . . .

ἄνα. For ὦ ἄνα of Apollo, cf. *Hy. Hom. Ap.* 179, Theognis 1 (quoted above).

τ' ἐμοῦ. See on *l.* 12 above. καὶ [πο]τ' proposed by Trypanis (67, n. 7) gives the wrong sense; it would mean 'at my hands', whereas [κα]τ' suits the sense perfectly. Posidippus uses some Dorisms (see Ouvré, *op. cit.*, p. 39), but these amount to little more than an occasional *a* for *η*.

14-15. Schubart has observed, and Trypanis and Peek have agreed, that these lines scarcely suit a Greek (even a Macedonian Greek) living in Egypt during the first century A.D. In the mouth of a writer of Posidippus' date and reputation, voicing a claim to be honoured equally with Archilochus, they are just what would be expected. Mainland Greece is omitted presumably because the author is there already, at Thebes; the mention of the Macedonians together with the islanders and the inhabitants of the coast of Asia Minor well suits a Macedonian living in the time of Antigonos Gonatas.

15. *γείτονες ἡϊόνος* is to be taken as a single expression, 'dwellers near the coast', on which *Ἀσίης πάσης* depends; *Ἀσίης* is a noun. The word *ἡϊών* (ten times in Apollonius, three times in Theocritus, common in epigrams of the best period) is not so far attested in Callimachus. Schubart draws attention to its occurrence at Posidippus fr. 26 Schott = *AP* v 209.1, 8. In *l.* 8 it falls in the same place in the pentameter as it does here; but this is so common in elegiac verse that it has little significance. The word normally means 'shore', so that there is no reason why it should not be used for 'coast'.

16 f. From his prayer to Apollo to command that he be honoured, the poet passes straight to the description of the statue which he would like to see erected. The inscription of Mnesiepes speaks only of the Archilocheion itself and of altars inside it, not of a statue; the later inscription of Sosthenes speaks only of a picture of Archilochus. But there is some reason for thinking that the Archilocheion had in it or near it a statue of Archilochus; Gow on Theocritus *Epigr.* 21, an inscription for a statue of Archilochus, admits the possibility that the statue was to be erected at the poet's birthplace, like the statues of Anacreon, Epicharmus and Pisander for which *Epigr.* 17, 18 and 22 were written. It may even be that Theocritus' epigram was written for a statue to be placed in the Archilocheion; if so, there would be a special appropriateness in *l.* 4:

ἦ ῥά νιν αἱ Μοῖσαι καὶ ὁ Δάλιος ἡγάπευν Ἀπόλλων.

See the discussion of this epigram and other Hellenistic references to Archilochus by A. von Blumenthal, *Die Schätzung des Archilochus im Altertum* (Stuttgart, 1922) 15 f.

16. *Πελλαῖον γένος ἄμόν*. On Pella as the native city of Posidippus, see above, p. 76. Is the monument which the poet claims that he deserves envisaged as being at Thebes or

at Pella? In sepulchral epigrams the mention of a man's place of origin often implies that he is not a native of the place where he lies buried. But here the case is different. The poet has just mentioned Archilochus, whose monument was situated at his birthplace, Paros; and it is natural to infer that the monument he is claiming for himself will be at his own birthplace, Pella. Not that Posidippus is likely to have expected to be taken at his word.

The distinction between $\acute{\alpha}\mu\acute{o}\varsigma = \acute{\epsilon}\mu\omicron\varsigma$ and $\acute{\alpha}\mu\acute{o}\varsigma = \eta\mu\acute{\epsilon}\tau\epsilon\rho\omicron\varsigma$ implied to exist by Demetrius ap. Σ A on *Il.* vi 424 is not borne out by the manuscripts;¹⁹ but as we must print either a rough or a smooth breathing, we may as well observe it. The word is uncommon in Hellenistic poetry; it occurs at Theocritus v 108 and at Philip of Thessalonica, *AP* ix 742, both times in Doric; but since it is found in Homer, its occurrence in epic verse is no great surprise. Peek (p. 445) has observed that ellipse of the verb to be is common in the remains of Posidippus.

ἔοιμι: cf. Callimachus fr. 162.3 ἔοις, fr. 55.3 ἔοι.

βίβλον ἐλίσσων is a normal way of describing someone perusing a roll; cf. Callimachus fr. 468, with Pfeiffer's parallels.

17. The tablet has $\acute{\alpha}\mu\phi\omega$. Diels emended this to $\acute{\alpha}\mu\phi\omicron\upsilon\upsilon\varsigma$ which he took to mean 'in both hands'. Schubart kept $\acute{\alpha}\mu\phi\omega$, which he took to be indeclinable, having the same meaning ascribed by Diels to $\acute{\alpha}\mu\phi\omicron\upsilon\upsilon\varsigma$. In support of this he quoted *Il.* iii 211; further instances of indeclinable $\acute{\alpha}\mu\phi\omega$ are given by Gow on Theocritus xvii 26, in all of which, except Apollonius, *Arg.* i 165 and Quintus i 261, it stands for a dative. It must be noted that in all these places the presence of another word agreeing with $\acute{\alpha}\mu\phi\omega$ makes it clear what case it represents (except at Quintus ii 460 *Ἐρις δ' ἐπεγλήθειν ἄμφω*), where the case of $\acute{\alpha}\mu\phi\omega$ is made clear by what precedes. It should further be observed though $\acute{\alpha}\mu\phi\omicron\tau\acute{\epsilon}\rho\alpha\iota\varsigma$ occurs several times alone in the sense of 'both hands', neither $\acute{\alpha}\mu\phi\omega$ nor $\acute{\alpha}\mu\phi\omicron\upsilon\upsilon\varsigma$, as far as I know, is ever so used. We cannot be sure that $\acute{\alpha}\mu\phi\omega$ used as an indeclinable adjective and meaning 'in both hands', is not correct; but we cannot exclude the possibility that $\acute{\alpha}\mu\phi\omicron\upsilon\upsilon\varsigma$ is right, nor yet the further possibility that $\acute{\alpha}\mu\phi\omega$ is corrupt for something else. The right editorial procedure is to keep $\acute{\alpha}\mu\phi\omega$ in the text and to signal doubt in the apparatus.

λαοφόρῳι . . . εἰν ἀγορῇ. Mr D. M. Lewis aptly compares *IG* vii 53, 14 *λαοδόκῳ ἔν ἀγορῇ*. H. T. Wade-Gery, *JHS* liii (1933) 96, thus corrects the meaningless *λαοδόκῳ ἀγορῇ* printed by earlier editors.

κείμενος here simply means 'situated in'; we must not suppose that a resting figure is envisaged; cf., e.g., *Il.* ii 688 *κεῖτο γὰρ ἐν νήεσσι . . . Ἀχιλλεύς*, etc.

18 f. All who have edited the poem so far have thought its author capable of making the second syllable of the word short and the third long; to indicate this they have made it properispomenon, an accentuation for which I can find no parallel. Can even a poet of the first century after Christ have been guilty of such an absurdity? The identification of a reference to Archilochus in *l.* 12 gives what seems to me a safe guide to the correct reading here. I suggest that the poet wrote

ἀλλ' ἐπὶ μὲν Παρ<ί>ηι δὲ ἀηδόνι . . .

It has already been suggested that we have here an instance of the practice of treating the word for 'nightingale' as a poetical equivalent of 'poet'. This occurs first in Bacchylides iii 98, who refers to himself as *Κητὰς ἀηδόνας*, and it is common in verse of the Hellenistic period. Simmias, *Ovum* 4, calls himself *Δωρίας ἀηδόνας*. Hermesianax, fr. 7, 49 Powell, says that Alcaeus was in love with Sappho in the words *αὐτὰρ αἰοιδὸς ἀηδόνας ἠράσαθ'*: Ion of Samos calls Euripides *τὸν σκηνῇ μελίγαρυν ἀηδόνα*, echoing Euripides' own famous words about Palamedes. At a late date we find even a prose author so described; Christodorus, *Ecphr.* (= *AP* ii), 377-8, calls Herodotus *Ἀλικαρνησσῶ . . . θέσπις ἀηδών*. It may be alleged

¹⁹ See Kühner-Blass, *Gr. Gr.* i 602 n. 2; Schwyzler, ii 203 n. 1.

that ἀηδών as a description of Archilochus is inappropriate. That is irrelevant: it is clear from these examples that ἀηδών could be treated as a synonym for 'poet'. The poetess Nossis, a good writer of the third century, can describe Rhinthon, the author of φλύακες, as Μουσάων ὀλίγη τις ἀηδονίς (AP vii 414.3). Cf. Wilamowitz, *Die Ilias und Homer* 461 n. 1.

What is the tribute that someone is being told to offer to the Parian nightingale? Line 18 is incomplete, line 19 is corrupt, and certainty is unattainable. Let us first consider the beginning of line 19. Diels read ναμα; Schubart read λημα; Barns reads νημα. νᾶμα, taken with Schubart's emendation of the corrupt letters κατακλυσεων to κατὰ γληνέων would yield good sense: 'But to the Parian nightingale pay the tribute of a mournful flood. . . .'²⁰

19-20. Let us now consider all that lies between δάκρυα in l. 19 and στενάχων, δι' in l. 20. Previous editors have read δάκρυα θερμά; Barns reads δάκρυα κεινὰ. For κεινός, the epic form of κενός, in Hellenistic verse, cf. Euphorion, fr. 141 Powell = AP vii 651.1, 5 (where R. Keydell, *Byz. Zeitschr.* lii (1959) 362, rightly defends κεινή against Reiske's conjecture κενή, reminding us of Euphorion's liking for the σπονδειάζων). I know of no exact Greek parallel for this expression, although μάταιος is used in a similar way. But it stands to reason that tears may be called empty, as hopes or prayers may, because they can effect nothing; οὐ γάρ τις πρῆξις πέλεται κρυεροῦ γόοιο (Il. xxiv 524; cf. Od. xi 202, 568). Cf. Virgil, *Aen.* iv 449

mens immota manet; lacrimae uoluntur inanes

and also *ibid.*, x 464.

χέω[ν] | καὶ στενάχων. Diels read χέω | καὶ στενάχω, ναί . . . In 1911 Schubart read χέω[ν] | καὶ στενάχων, δι' . . .; but in 1932 he went back to Diels's reading, and Page and Heitsch followed him. Barns for reasons of palaeography and I for reasons of sense independently came to the conclusion that Schubart's reading of 1911 is correct, a conclusion which the new photograph helps to confirm.

This makes the text a great deal easier to understand. First, it eliminates the unspeakably feeble ναί, together with the highly objectionable correction of a monosyllable which its presence involves. Secondly, when taken in conjunction with the emendation I have proposed in l. 18, it removes a grave awkwardness in the sense. Crönert (quoted on p. 75 above) had severely criticised the poet for his rapid transition from the weakness in 19-20 (Diels's reading) to the resolution shown in 24. But we now see that at 20-1, it is not the poet who is lamenting; he is simply contrasting the tribute it would be appropriate to pay to Archilochus with the tribute appropriate to pay to himself.

The tribute proper to Archilochus is one of tears; that is natural, in view both of his early death in battle and of his irascible and jealous character, a commonplace with the epigrammatists of the Anthology, as with other Greek poets. For the notion of a libation of tears made to the dead, see Gow on Theocritus xxiii 38 and add to his examples Meleager, AP vii 476.3-4, with the examples cited by Jacobs on this epigram (no. cix in Brunck's numbering); cf. Peek, *loc. cit.*, 1547.1; 1595.5-6; 2007.1, etc.

With the tribute of tears to Archilochus the poet evidently contrasted the sort of tribute appropriate to his own genial character. What was that tribute? We can hardly hope to know; but I suspect it may have been an offering of wine. For the notion of a libation of wine as an appropriate offering at the tomb of Anacreon, cf. Antipater of Sidon, AP vii 26; anon., *ibid.*, vii 28. Did the δι' function as a preposition or as a preverb standing in *imesi*? And did the poet ask his fellow-citizens to pour wine through his 'kindly mouth', i.e. over the mouth of his statue? We can hardly hope to know. The imperative in l. 19 suggests that somewhere in the text there must have been a vocative. At the end of l. 18,

²⁰ For νᾶμα of a flood of tears, see Sophocles, *Trach.* 919; Euripides, *Heracles* 625. With the whole sentence, cf. W. Peek, *Griechische Versinschriften* 1540 2-3: λυγρόν δ' οἱ μὲν λείβουσι κατ' ὄσσων | δάκρυ, σὸν ὀρφανίης πότμον ὀδυρόμενοι (Smyrna, second century); cf. *id.* 1552.2 λυγρὰ . . . δάκρυα, 1873.13 δάκρυα λυγρὰ, etc.

I cannot extract a suitable vocative from the traces made out by Barns; I therefore suspect that a vocative stood somewhere in the missing portion of the text between *στόμα* and the beginning of *l. 21*. If I had to guess at the nature of the missing part of *ll. 20-1*, I should suggest something like

δι' ἐμὸν δὲ φίλον στόμα [λεῖβε, πολῖτα,
[γῆραιὸν πληρέων ἐκ κυλίκων Βρόμιον]

But I had rather not guess at all.

21. οὖν following *μηδέ* is unusual. But οὖν follows *μή* at Aeschylus, *Suppl.* 392 *μή τί ποτ' οὖν γενοίμαν ὑποχείριος* | *κράτεσιν ἀρσένων*: at Sophocles, *OC* 1135 and the other passages listed by Denniston, *The Greek Particles* 420; and οὔτε and μήτε, of course, frequently. In this text *δ* is four times written for *τ* (see p. 77 above), and this may have happened here also. Such a usage has of course nothing to do with inferential οὖν: the word is here 'ein versicherndes, bekräftigendes, bestätigendes Adverb' (Kühner-Gerth, ii 155).

From the earliest times it was usual to wish that one's death might be lamented; see *Od.* xi 53, Callinus, fr. 1.17, and many other instances. This accords with the primitive belief that the spirits of the dead demand lamentation by their friends and kinsfolk as their due. But we find at work also an opposite superstition, that too much lamentation harms the dead by denying them rest; see E. Rohde, *Psyche* (English edition) 164, 190. Just as we find two opposite primitive beliefs operating in opposed directions, so do we find two opposite rational attitudes; most people wish to be lamented, but a few wish not to be, usually on the ground that they have led a happy life, so that lamentation is inappropriate. The latter attitude is as early as Carphylides (probably a writer of the third century before Christ) *AP* vii 260; Lucianus, *ibid.*, 308; anon., *ibid.*, vii 667; Gregory Nazianzen, *AP* viii 64. Cf. *Epigr.* 151. 13-14 Kaibel; and for further sepulchral examples see R. Lattimore, *Themes in Greek and Latin Epitaphs* (Illinois Studies in Language and Literature, 1942) 217 f. The most famous instance of a man forbidding others to mourn his death is that of Ennius (*Varia* 17 Vahlen):

*nemo me lacrumis decoret nec funera fletu
faxit. cur? uolito uiuu' per ora uirum.*

Trypanis' supposition that Ennius was imitating our poet is exceedingly unsafe. Ennius was by no means the first to say this.

αὐτὰρ ἐγὼ . . . Kranz²¹ has observed that αὐτὰρ ἐγὼ, like ἀλλ' ἐγὼ and ἐγὼ δέ, commonly introduces the last sentence of a poem. The Hellenistic poets echoed the ancient rhapsodic formula

αὐτὰρ ἐγὼ καὶ σείω καὶ ἄλλης μνήσομ' αἰοδῆς.

They first adapted such a formula to look forward to a task, often a poetic task, awaiting them in the future. In our poem it is used to introduce a final prayer; for a poem to end with a prayer is another ancient poetic practice. It is common in the Homeric hymns (2, 6, 8, 10, 11, 13, 15, 16, 19, 20, 21, 24, 26, 30, 31; nine of Pindar's surviving epinicians end in this fashion). Compare the conclusions of Theocritus 15, 16, 17, 18, 20 and 22, and see Gow on that of 24. Callimachus in his hymns took over the practice, consciously echoing the Homeric hymns. Compare also the concluding lines of the *Aitia* (fr. 112.7-9). The first prayer is addressed to an unknown god or goddess; the second to Zeus; then the poet employs the rhapsodic αὐτὰρ ἐγὼ to look forward to his next poetic task:

χαῖρε, σὺν εὐεστοῖ δ' ἔρχεο λωϊτέρῃ.
χαῖρε, Ζεῦ, μέγα καὶ σύ, σάω δ' [ὄλο]ν οἶκον ἀνάκτων.
αὐτὰρ ἐγὼ Μουσέων πεζὸν [ἔ]πειμι νομόν.

²¹ *Loc. cit.* on p. 83, p. 121; cf. pp. 11, 16.

Our poet adapts these two ancient poetic practices, the concluding *αὐτὰρ ἐγὼ* and the final prayer, to introduce not merely the prayer for prosperity of the rhapsodic convention, but a prayer for his own last days and final end. The closest parallel known to me is the prayer to Apollo in which the thirty-first ode of Horace's first book reaches its climax:

*frui paratis et ualido mihi,
Latœ, dones, et, precor, integra
cum mente, nec turpem senectam
degere nec cithara carentem.*

In that ode we may recognise a prayer of the ageing poet to his patron god that employs a motive found in Euripides (*Heracles* 673 f.) and Callimachus (fr. 1); note especially the last two lines. I shall return presently to the thoughts set in motion by this final prayer; but first the commentary on the words of the text must be completed.

22. γήραι. κτλ. γήραι must be trisyllabic; see O. Schneider on Callimachus, *Hy.* 2.40. Is the meaning, 'May I come . . . to Rhadamanthys in my old age' (i.e., 'when I am old')? This would not go easily with l. 5, where the poet has called upon the Muses to help him sing of old age. True, the poet might conceivably call the Muse to sing of an old age which has not yet come upon him; but such a belief is not easily reconciled with the tone of the poem, and especially of the final prayer. It is therefore likely that the meaning is 'May I come . . . to Rhadamanthys through old age' (i.e. not through illness or through violence). For νόσος, βία, γήρας as the three main causes of death, see, e.g., Peek, *op. cit.*, 1816 (Mysia, first century B.C.), 1817 (Smyrna, first century B.C.). For this use of γήραι, cf. Meleager, *AP* vii 470 γήραι δ' ἡ νόσῳ βίον ἔλλιπες; Peek, *op. cit.*, 1449 (Chalcis, third century, B.C.) γήραι δὴ . . . λιπὼν βίον.

μυστικὸν οἶμον. οἶμον should probably have a rough breathing; see Callimachus, fr. 1.27 with Pfeiffer's note. The word is often used of the road to the next world; e.g. Diodorus, *AP* vii 627.2; Peek, *op. cit.*, 1330.5 (Teos, first century B.C.?); 1472.2 (Bithynia, first century B.C.); 1539.2 (Smyrna, second century B.C.); 1826.2 (Thessaly, second or third century A.D.); 1895.13 (Megara, second century A.D.).

What is the 'mystic path' along which the poet aspires to travel? The mention of Rhadamanthys shows that his destination is Elysium. Rhadamanthys was its first inhabitant, a position for which he was specially fitted by being the pattern of σωφροσύνη. Already in the *Odyssey* (iv 564) Elysium is the place ὅθι ξανθὸς Ῥαδάμανθς; and in Pindar's Second Olympian he presides over the blessed place attained by those who have thrice lived out a blameless life and have come by the road of Zeus to the tower of Kronos, where they live in eternal felicity in the company of Peleus, Cadmus and Achilles.²² Pindar's Islands of the Blest were clearly intended from the start only for great heroes born of gods and specially favoured by Zeus; but by his time a stage had been reached at which admission could be won by those who had lived out three lifetimes without crime. After the fifth century, eternal happiness came to be thought of more and more as the privilege of all εὐσεβεῖς as opposed to ἀσεβεῖς, of all ἀγαθοί as opposed to κακοί.²³ For example, an epigram of Hegesippus, a writer of about the same date as Posidippus, seems to present Rhadamanthys as presiding over the good in general (*AP* vii 545):

*τὴν ἐπὶ πυρκαϊῆς ἐνδέξιά φασι κέλων
Ἐρμῆν τοὺς ἀγαθοὺς εἰς Ῥαδάμανθον ἄγειν.*

When a dead man is made to say *μύσταις ἄμμιγα ναιετάω* (Peek, *op. cit.*, 1822.6; Thyrrheion

²² On Rhadamanthys, see Jessen in Roscher's *Lexikon*, s.v. 75; E. Rohde, *Psyche* (English edition) 55 f., 247; E. R. Dodds, *Plato, Gorgias* 374 f.

²³ See Rohde, *op. cit.*, 536 f.; cf. R. Lattimore, *Themes in Greek and Latin Epitaphs* (Illinois Studies in Language and Literature, xxviii, 1942) 40 n. 154.

in Acarnania, second century B.C.), he probably means by *μύσταις* not 'initiates' in the strict sense, but all good men among the dead.

Does Posidippus think of an Elysium populated by the good in general? The context and tone of his poem make it far likelier that he conceived himself as belonging to a select company of exceptional people. Of course it is possible that he had actually been initiated, as Euphronion seems to have been (see Theodoridas, *AP* vii 406). But his request to Apollo for special honours amounts to a claim to heroisation; and it is natural that one who has made such a claim should aspire to eternal happiness in the company of the great heroes of the past. The word *μυστικόν* may mean no more than that the poet thinks himself one of the elect. But Hellenistic poets and their Roman imitators often conceive the poet as an initiate into the 'mysteries' (*ὄργια*, *τελευταί*) of the Muses;²⁴ and it is not unlikely that this is the sense in which Posidippus thought of himself as treading the mystic path.

ἰκοίμην simply means 'may I come'; yet it is worth remarking that this verb is often used of attaining to a longed-for place, or one's last resting-place. The *locus classicus* for this usage is Plato, *Crito* 44b 2, where Socrates describes how a beautiful woman in white appeared to him in a dream and said *ἡματί κεν τριτάτῳ Φθίην ἐρίβωλον ἰκοιο* (an adaptation of Homer, *Iliad* ix 363). 'Achilles', writes Burnet *ad loc.* (p. 177 of his *Euthyphro, Apology und Crito*) '... means that he can get home in three days, and that is what Socrates understands the dream to mean.' Cf. Euripides, *Bacch.* 403 *ἰκοίμαν ποτὶ Κύπρον*: the phalaecean verse from a sepulchral epigram quoted by Wilamowitz, *Hellenistische Dichtung* i 176 *χαῖρε καὶ εὐσεβέων ἰκοιο χώρον*: Peck, *op. cit.*, 1487.16 (Phrygia, third century B.C.) *εἰς ὄρ[φ]νῃν ἰκοίμην πᾶσιν ὀφειλομένην*: 1508.16 (Gaza, about 201 B.C.) *ἴκειο τὴν κοινὴν ἀτραπὸν εἰς Ἀἶδεω*.

23. *δῆμῳ καὶ λαῷ παντὶ ποθεινός ἐών*. The *πόθος* of the whole community for the valiant warrior is a commonplace of early elegy and the early epigram. Diels compared Callinus, fr. 1.16 f. (*ὁ μὲν* refers to the man who stays at home instead of fighting):

ἀλλ' ὁ μὲν οὐκ ἔμπηξ δῆμῳ φίλος οὐδὲ ποθεινός,
τὸν δ' ὀλίγος στενάχει καὶ μέγας, ἣν τι πάθῃ.
λαῷ γὰρ σύμπαντι πόθος κρατερόφρονος ἀνδρός
θνήσκοντος, ζῶων δ' ἄξιος ἡμυθέων.

By adapting such language to his own praises, the poet is staking his claim to be regarded, no less than if he were a great warrior, as a benefactor of the community.²⁵ If a distinction can be drawn between *δῆμος* and *λαός*, *δῆμος* is 'community' and *λαός* is 'host', as in the poem of Callinus; but the difference is hardly to be insisted on.

24. *ἀσκήπων*. The *σκήπτρον* is normally the *γεροντικὸν ὄπλον* (Callimachus, *Epigr.* 1.7) in classical poetry and later; cf. Aeschylus, *Agam.* 75, Euripides, *Heracles* 107-8, etc. The ability to do without it appears as a characteristic of vigorous old age in an epigram by Theodoridas, *AP* vii 732:

ᾧχε' ἔτ' ἀσκήπων, Κινησία, Ἑρμόλα νιέ,
ἐκτίσων Ἀἰδῇ χρεῖος ὀφειλόμενον,
γῆραι ἔτ' ἄρτια πάντα φέρων. . .

Theodoridas flourished about 225 B.C., and may have known this poem. Cf. *AP* vii 224,

²⁴ Aristophanes, *Ran.* 356-7 is an early instance of a way of speaking that in Hellenistic poetry became a commonplace. See the passages cited by Shackleton Bailey, *op. cit.*, 141 (on Propertius iii 3.29), and add Meleager, *AP* iv 1.57 (poets as *μύσται*); Himerius 16.6, p. 489 (*τελευταί* of the Muses); Christodorus, *AP* ii 133 (*ὄργια* of the Muses).

²⁵ Cf. *IG* i² 54 (= M. N. Tod, *Greek Historical*

Inscriptions i² 59), l. 1: *ἀνδρας μὲν πόλις ἦδε ποθεῖ καὶ δῆμος Ἑρεχθέως*: cf. Simonides, fr. 137.2 Diehl, Anacreon fr. 100.1-2 Diehl, etc. Such language is used of a poet (Phaenocritus) by Dionysius of Rhodes, *AP* vii 716 (*πρώτος, ἀλλὰ ποθεινός* . . .); cf. the epitaph on the comedian Euthias, Peck, 1495: *ζηλοῖ σε Ἑλλὰς πᾶσα, ποθεῖ θ' ἱεροῖς ἐν ἀγῶσιν*.

an epigram of uncertain authorship, where Callicrateia is said to have attained the age of 105 σκίπωνι τρομεράν οὐκ ἐπιθείσα χέρα.

ὀρθοεπής ἀν' ὄμιλον. ὀρθοεπεία and ὀρθοεπέω are used to denote correctness of diction, the former first by Plato, *Phaedrus* 267 C. The sense here is that the poet is no τραυλὸς γέρων, but retains in old age the gift of words. ὄμιλος is not common in Hellenistic poetry, but is used by Theocritus at 22.7 and (in a less elevated context) at 15.65. ἀν' ὄμιλον here strikes an epic note; cf. Homer, *Il.* iii 449; v 528; vii 183; xi 247, 259, 324; xii 49; xvi 365. The poet probably has in mind the most celebrated epic description of the eloquent speaker (*Od.* viii 169 f.):

ἄλλος μὲν γὰρ εἶδος ἀκιδνότερος πέλει ἀνὴρ,
ἀλλὰ θεὸς μορφήν ἔπεισι στέφει · οἱ δὲ τ' ἐς αὐτὸν
τερπόμενοι λεύσσουσιν, ὃ δ' ἀσφαλῆως ἀγορεύει
αἰδοῖ μειλιχίῃ, μετὰ δὲ πρέπει ἀγρομένοισιν,
ἐρχόμενον δ' ἀνὰ ἄστνυ θεὸν ὥς εἰσορώσιν.

Compare Hesiod, *Theog.* 81 f., a passage which is usually held to have influenced or been influenced by the passage just quoted. Hesiod too describes the effect of the speaker's words upon his audience (84-5 οἱ δὲ τε λαοὶ | πάντες ἐς αὐτὸν ὀρώσι. . .), and speaks of him as ἀσφαλῆως ἀγορεύων (86, cf. ὀρθοεπής in Posidippus).

25. The natural wish to leave one's property to the heirs of one's body (for which see Homer, *Il.* v 154, 481; Pindar, *Ol.* x 86 f.; Euripides, *Ion* 475 f.; Menander, fr. 594, etc.) is allied to more than one common theme of the sepulchral epitaph, e.g. to the wish that one's children may survive one; to take one of countless instances, cf. Peek, *op. cit.*, 1388.2 (Smyrna, third century B.C.) τοιγάρτοι παίδων σε φίλαι χέρες, ὡς θέμις ἐστί, | κρύψαν, ἐπεὶ γήρως ὄλβιον ἦλθε τέλος.

ὄλβος for wealth is an epic and tragic usage, employed by Callimachus in the hymns (1.84, 95, 96), by Theocritus in the encomium of Ptolemy (17.95) and by the author of the *Ἡρακλῆς Λεοντοφόνος* (= Theocritus 25.24, 111).

The detailed examination of the poem has made it abundantly clear that no adequate reason for disbelieving the author's claim that he is Posidippus of Pella exists. The apparent anomalies of language and metre that have caused editors to assign it to a poet of the first century A.D. have been shown to be due to the imperfect preservation of the text. Wherever the text can be made out with any degree of certainty, we recognise it as an elegiac poem by a writer of high competence, in all probability a contemporary of Callimachus. The style and vocabulary do not resemble those of the epigrams of Posidippus; they have a marked epic flavour. But it is not surprising that an elegy of this length should have little in common with epigrams most of which are of a sympotic or amatory nature; it is decidedly more like the epigrams on the Pharos²⁶ and the temple of Arsinoe Zephyritis preserved in the Didot papyrus frs. 1.2 (Page, *Greek Literary Papyri*, no. 104 a and b, p. 444 f.).

²⁶ *Il.* 1-4 of the first of these epigrams appear in our texts as follows:

Ἑλλήνων σωτήρα, Φάρον σκοπόν, ὃ ἄνα Πρωτεύ,
Σώστρατος ἔστησεν Δεξιφάνου[ς] Κνίδιος.
οὐ γὰρ ἐν Αἰγύπτῳ σκοπαὶ οὐρεά θ' οἷ' ἐπὶ νήσῳ,
ἀλλὰ χαμαὶ χηλὴ ναύλοχος ἐκτέταται.

L. 3 corr. Blass: σκοπαιονρησσειει Π.

L. 4 is rendered by Page as follows: 'but low lies the breakwater where ships may harbour'. The use of χαμαί, properly 'on the ground', to mean 'low' is

unusual; and we expect not to be told that the breakwater is low, but that the harbour as a whole is. Mr E. Lobel has pointed out to me that χαμαὶ χηλὴ is a misreading of the papyrus, which in fact has χαμαιζηλη: the squashed appearance of the ζ makes it look like χ: see the photograph published by H. Weil in the *editio princeps*. Apart from being in the manuscript, this gives far better sense. For χαμαιζηλος in verse, cf. Nicander, *Ther.* 70; usually it has two terminations, but cf. Hippocrates, *Art.* 13.

For what purpose, and for what kind of publication, was this poem designed? Dr Barns's examination of the tablets and mine of the text show that there is no reason to think that any part of it is missing, apart from those gaps which I have indicated by rows of dots; but how are we to explain the curious brevity of the main portion of the poem in relation to the elaborate invocation of the Muses and Apollo at the beginning? The discussion of 21 f. has already furnished an indication of the direction in which a solution to this problem is to be found. There it was observed (p. 92 above) that a final sentence introduced by αὐτὰρ ἐγὼ and also a concluding prayer, are often features of a σφραγίς. Several other features of the σφραγίς are present in this poem. The poet's appeal to the Muses, as Kranz has observed, is often bound up with his introduction of himself. He twice mentions his own name (5, 9), ceremoniously referring to himself in the third person. Leo in a famous article²⁷ observed that the σφραγίς regularly gives us the poet's γένος and describes his τρόπος. This poet uses the device of describing the statue he would like to see erected to give his γένος (16) and, by way of a contrast with the bitter and unfortunate Archilochus, to describe his τρόπος (18 f.). This seems to me abundant evidence for supposing that our poem was the signature poem of a collection. Such a poem might stand either at the beginning of a book or at the end; in this case the opening invocation strongly suggests that the poem stood at the beginning.

Can we guess what book the poem may have appeared in? One possible clue is furnished by a passage in the Homeric scholia (Σ A on *Il.* xi 101, ed. Dindorf, i, p. 376). This line appears in the manuscripts thus:

αὐτὰρ ὁ βῆ ρ' Ἰσὸν τε καὶ Ἀντίφον ἐξεναρίξων:

the commentator is discussing a variant βήρισον, which he assigns to the authority of Posidippus. 'μὴ ἐμφέρεσθαι δέ φησιν', he writes, 'ὁ Ἀρίσταρχος νῦν ἐν τοῖς ἐπιγράμμασιν τοῦ Ποσειδίππου, ἀλλ' ἐν τῷ λεγομένῳ σωρῶι εὑρεῖν. εὐλογον δέ φησιν ἐξελεγχόμενον αὐτὸν ἀναλεῖψαι (cf. Σ T ad loc., ed. Maass, i, p. 384). It has naturally been inferred from this that Posidippus first published some of his epigrams in a collection called the Σωρός, and that they were later republished in a collected edition.²⁸ The reputed connexion of Posidippus with Asclepiades and the fact that in the Anthology there is often doubt as to whether a poem belongs to one or the other of these poets or to Hedylus led Reitzenstein²⁹ to guess that there may have been a volume of collected poems published by these three poets together. This may be right; but since we know of no other such collection of poems put out jointly by several authors—an anthology of course is quite another matter—Reitzenstein's conjecture should be looked upon with caution. This poem may have been the σφραγίς of the book called the Σωρός. If it was, it seems unlikely that Asclepiades and Hedylus also contributed to the volume; for a joint volume with contributions from three poets would hardly bear so personal a signature. But as there is nothing but a possibility that this poem belonged to the collection in question, this argument does not help us much. For all we know, it may have been the signature poem of the later, complete collection of epigrams mentioned in the scholion.

But there is another possibility. In l. 5 both συναείσατε and συναείρατε are possible readings, but the former is likelier to be correct (above, p. 84). If it is, we could most easily dispose of the objection that Posidippus does not in fact sing of old age in this poem by supposing either that the collection of which this was the seal-poem was called Γῆρας—a perfectly possible name for such a collection—or that the poems it contained related to this theme. An explanation on these lines seems likelier than not to be the right one.

If so, the poem was probably written when its author was already old. Can we make

²⁷ F. Leo, *G.G.N.* (1898) 469 f. = *Ausgewählte Kleine Schriften* ii 169 f.; cf. Wilamowitz, *Sappho und Simonides* 296 f.; Fraenkel, *Horace* 305 f.

²⁸ See Schott, *op. cit.*, p. 105 f; Peek, p. 431.

²⁹ R. Reitzenstein, *Epigram und Skolion* (1893) 100-2.

any guess about its date? Peek (p. 429) has rightly said that we know less of Posidippus' chronology than was once supposed. It was confidently believed³⁰ that useful evidence for the date of his birth could be extracted from *AP* v 134 = fr. 8 Schott = Geffcken, *Griechische Epigramme* no. 257):

Κεκροπὶ ῥαῖνε λάγυνε πολὺδροσον ἱκμάδα Βάκχου,
ῥαῖνε, δροσιζέσθω συμβολικὴ πρόποισις.
σιγάσθω Ζήνων ὁ σοφὸς κύκνος ἃ τε Κλεανθοῦς
μοῦσα · μέλοι δ' ἡμῖν ὁ γλυκύπικρος Ἔρως.

This was thought even by Wilamowitz in 1924 to show that the poet had studied in Athens under Zeno and Cleanthes and had therefore reached student age before Cleanthes died in about 275. But for me to say 'Let Heidegger be silent' it is not necessary for me to have been Heidegger's pupil; this was pointed out by Pohlenz as early as 1911.³¹ At *AP* xii 8 = fr. 15 Schott = Geffcken, *loc. cit.*, no. 260, Posidippus says that his *ψυχή* has been exercised by reading;³² at *AP* xii 120 = fr. 16 Schott he says that *λογισμός* stands by his side to help him against his enemy Eros.³³ Both epigrams are consistent with his having had some philosophic training; but they no more prove him to have been a Stoic than *AP* ix 359 = fr. 21 Schott proves him to have been a Cynic (see Peek, p. 430). It is true that the resigned close of our poem strikes a note that would not surprise us in a poet under Stoic influence; and we may remember that for much of Posidippus' lifetime Macedonia was ruled by Antigonos Gonatas, a Stoic and a patron of poetry.³⁴ Phoenix of Colophon addressed a poem (fr. 3 Knox = fr. 6 Powell) to one Posidippus; but Gerhard's grounds for identifying him with the epigrammatist are insufficient.

The epigrams on the Pharos and the Arsinoe temple mentioned above (p. 95) show that Posidippus was active in Egypt between about 284 and about 270; and the inscription from Thermum (p. 76) shows that he was honoured by the Aetolians in about 264/3. If we could safely follow Trypanis in taking *l.* 9 to allude to the latter event, we could safely date our poem after it; but I have given reasons for thinking his argument unsafe (above, p. 87). Still, if Posidippus was an established poet about 284 and was honoured by the Aetolians about 264/3, he can hardly have been born much after 310, and very likely he was born earlier.³⁵

One would give much to know the exact chronological relation between Posidippus and Callimachus; for the main interest of the poem under discussion seems to me to consist

³⁰ E.g. by G. Knaack ap. F. Susemihl, *Geschichte der griechischen Literatur in der Alexandrinerzeit* ii 531 n. 66; cf. Wilamowitz, *Hellenistische Dichtung* (1924) i 148.

³¹ M. Pohlenz, *Χάρτες Fr. Leo zum sechzigsten Geburtstag dargebracht* (1911) 93; cf. G. Pasquali, *Hermes* xlviii (1913) 207 n. 6; Peek, p. 429 f.; A. S. F. Gow, *CQ* n. s. 4 (1954) 200.

Schott (p. 45) finds it odd that Posidippus should describe Zeno and Cleanthes in terms properly applicable only to poets, since though Cleanthes wrote verse, Zeno is not known to have done so. The use of *Μοῦσα* need not imply poetry, but that of *κύκνος*; seems to; cf. Leonidas fr. 25 Geffcken = *AP* vii 19 (of Alcman); Antipater of Sidon, *AP* vii 30.1 (of Anacreon); Horace, *Od.* iv 2.25 (of Pindar); Christodorus, *AP* ii 382 (of Pindar); *ibid.*, 414 (of Virgil); for the swan's song, see Wilamowitz on Euripides, *Heracles* 100 and Fraenkel on Aeschylus, *Agam.* 1444. But in applying to the prosy philoso-

phers language appropriate to poets, Posidippus was surely aiming at a humorous effect.

³² *Τὸν Μουσέων τέττιγα Πόθος δήσας ἐπ' ἀκάνθαις
κοιμίζειν ἐθέλει πῦρ ἐπὶ πλευρὰ βαλόν.
ἢ δὲ πρὶν ἐν βύβλοις πεπονημένη ἄμμι' ἀθερίζει
ψυχὴ ἀνηροῖ δαίμονι μεμφομένη.*

In *l.* 3, the manuscript has *ἄλλα θερίζει: ἄμμι' ἀθερίζει* is the emendation of C. A. Trypanis, who also pointed out the relevance of the proverb *ἀκάνθιος τέττις* (*Κρητικά Χρονικά*, 1953 296-7).

³³ *Ἐὐπολῶ καὶ πρὸς σε μαχίσσομαι, οὐδ' ἀπεροῦμαι
θηγνός εἶν' σὺ δ', Ἔρως, μηκέτι μοι πρόσσας.
ἦν με λιβήης μεθύοντ', ἅπαρ' ἐκδοτον ἄχρη
δὲ νήφω,
τὸν παραταζόμενον πρὸς σε λογισμὸν ἔχω.*

The manuscript has *παραταζάμενον* in *l.* 4; but surely a future is required.

³⁴ See Wilamowitz, *Hellenistische Dichtung* i 36 f.; W. W. Tarn, *Antigonos Gonatas* (1913) ch. viii 'Antigonos and his Circle', 223 f.

³⁵ See Peek, *loc. cit.*

in the light it throws upon the literary controversies of which the greater poet was the centre. The Florentine scholia to the *Aitia* (Pfeiffer, i 3) contain a list of names which some think to be the names of writers who were concerned with the quarrel between Callimachus and those enemies whom he called 'Telchines' after the malignant demons of ancient Rhodes, but which most take to be a list of the 'Telchines' themselves.³⁶ This list includes Posidippus, together with his friend Asclepiades. Why should these poets have been enemies of Callimachus? Some of his rivals reproached him with having written no long poem; but these two wrote only short poems (with the possible exception of the work or works of Posidippus discussed on p. 86), nor do their epigrams differ markedly in tone and subject-matter from Callimachus' own. Still, we possess certain indications that they were his enemies. Both wrote epigrams in praise of the Lyde of Antimachus, which Callimachus (fr. 398) derided;³⁷ and an indecent and amusing epigram by one or the other of the pair (*AP* v 202 = Posidippus fr. 25 Schott) contains (l. 4) an obvious parody of the second line of Callimachus' *Bath of Pallas* (*Hy.* 5.2).

Does the seal-poem help us any better than the epigrams to understand the differences of poetical principle between its author and Callimachus? Its style and manner are as different from those of the epigrams ascribed to Posidippus as the difference of subject-matter would lead us to expect; only the epigrams on the Didot papyrus are somewhat similar (see the resemblances to other poems of Posidippus collected by Schubart and set out on p. 76 above). The seal-poem is nothing if not in the grand manner. Style and vocabulary both strike an epic note, as the commentary should have made clear. The ancient form of the ὕμνος κλητικός is employed to address the Muses; they are to leave their usual haunts on Helicon and come to where the poet is. Apollo, if my guess is right (p. 87) is asked by means of an equally ancient formula to befriend the poet now, if he has done so in time past. Every regular feature of the σφραγίς is found here (see p. 96). The use of these traditional formulas does not by itself mark out the writer as a traditionalist; Callimachus himself used all of them. What makes this writer's use of them appear so different is the conventional manner in which they are applied. This poet could scarcely have claimed to have avoided the main roads of poetry or the beaten tracks (see Callimachus, fr. 1.25 f.), nor could he have echoed Callimachus' prayer ἐγὼ δ' εἴην οὐλαχὺς, ὁ περὶοίς (*ibid.*, 32). The second prologue to the *Aitia* (Callimachus, fr. 1) resembles this poem in being written late in its author's career and in its concern with old age and with the Muses; and certain contrasts between the two poems are instructive. The faded imagery and stilted technique of the lesser writer contrast unfavourably with the freshness, lightness and playfulness of the great poet; so does the pompous and theatrical attitude struck by Posidippus with the gay, ironical and unsentimental tone of Callimachus. While Posidippus ceremoniously calls upon the Muses to join him in his song of old age, Callimachus briefly remarks that the Muses do not abandon when their hair is white those on whose youth they have cast a kindly eye (fr. 1.37-8; cf. *Epigr.* 21.5-6). In contrast with the self-conscious stoicism (in the modern sense, at least) of Posidippus, Callimachus is not ashamed to lament for lost youth (33 f.), like Euripides and Mimnermus before him. One would give a great deal to hear Callimachus' comment on the other poet's monstrous impertinence in claiming to deserve the same heroic honours accorded to Archilochus.

That this poem is by the famous Posidippus, and that it is the seal-poem of a collection seems to me certain; possibly the collection was called *Γῆρας*. But though it is the work of a competent writer of the third century B.C., it is no masterpiece. Its main value lies not in its intrinsic merit, but in the interest it has as providing an example—and in all

³⁶ For a bibliography of the discussion, see H. Herter, *Bursians Jahresbericht* 255 (1937) 109 f.

³⁷ Asclepiades fr. 30 Knauer = *AP* ix 63; Posi-

dippus fr. 18 Schott = *AP* xii 168. Both are reprinted by B. Wyss, *Antimachi Colophonii reliquiae* lxvii.

probability, an example from the hand of a poet older than Apollonius—of the kind of contemporary poetry against which Callimachus and his followers reacted with so much sharpness.³⁸

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³⁸ When the first draft of this article was complete, Mr E. A. Barber told me that in 1951 he had been prompted by the observation of Professor Trypanis mentioned on p. 76 above to make a study of this poem; and with great kindness he and Professor D. L. Page allowed me to read a correspondence on the subject which at that time passed between them. To my great satisfaction, I learned that Mr Barber had anticipated some of my own remarks. In *l.* 3 he preceded me in conjecturing 'Ὀλέμπου and in referring it to the aulete of that

name. He also divined the reference to Archilochus in *l.* 11; but as Kondoleon's important article had not yet appeared, he was unable to give a satisfactory explanation of Posidippus' claim to be honoured as Archilochus was honoured.

My great obligation to Dr J. W. B. Barns has already been mentioned (p. 77). I would also like to thank Sir Maurice Bowra, Professor R. Kassel and Professor G. W. Williams for valuable comments on the first draft of my paper, and Sir John Beazley and Mr D. M. Lewis for helpful information.

THE DATING OF DOCUMENTS TO THE MID-FIFTH CENTURY—II

IN this Journal last year, in the first part of this paper, we began our discussion of Mattingly's proposal to lower the dates of several fifth-century documents: we began with the two from which he started, namely D7 and D14.¹ These two he proposed to put in the late twenties. We and Malcolm McGregor had put them in the early forties, and after discussing what we took to be Mattingly's most formidable arguments we found no cogent reason to change these dates. We therefore did not go again in detail into their historical contexts, into the numismatic consequences of D14 or into the traces which D7 may have left in the long appendix to quota-list 8.²

In this second part of our paper we shall discuss the other documents to which Mattingly has assigned more or less plausible new contexts later than those assigned hitherto. In our judgment, the early date of most is indicated by their script, particularly by the fact that most of them write *sigma* with three bars (ς) instead of four, so that it requires arguments of some cogency to move them much below the mid century.

For three documents, Mattingly has alleged reasons of such cogency: for D11 (Miletos); for Tod 40 = SEG x 30 (Nike priestess); for SEG x 24 (epistatai for Eleusis). All these use the three-barred *sigma*, and if the positive reasons which he alleges for their late date are valid, then he is entitled to refuse our *sigma* criterion. We therefore draw the reader's special attention to these three. There are three other documents which seem to us vital: the Peace of Kallias (see Hill's *Sources for Greek History*,³ 344); D12 (the 'Congress Decree'); D13 (the 'Papyrus Decree'). We know of these by report only, and no inscribed copy of any has survived: no physical criteria can be used. We have dated all three to 450/49 B.C.; Mattingly puts them respectively in 424/3,⁴ 438/7 or later, and 431/0. These dates are indeed incidental to his main argument: D12 depends on his doctrine about heralds' journeys,⁴ and all three are to some extent involved in his beliefs about Perikles' building programme. We discuss these dates below, pp. 106 ff.

I. ATHENS AND HER ALLIES

Miletos: D11

Our task in maintaining the date 450/49 for D11 has been greatly eased by J. P. Barron's article in *JHS* lxxxii (1962) 1 ff. Barron sets the document in its Milesian context, in the light of Milesian prosopography and Milesian reaction to the claims of Athens to be the metropolis of Ionia; he provides incidentally some valuable support for our tentative suggestions about how Athens' use of that claim, around the middle of the century, was

¹ Decrees of Klearchos on coinage and of Kleinias on tribute collection. In this second part of our paper, as in the first (*JHS* lxxxii [1962] 67 ff.) we refer to the documents concerned by the numbers given to them in *ATL* ii 46-78, or (if not there numbered) by reference to Tod's *Selection* or (most often) to *SEG*.

² The former question we leave to numismatists. For the latter, we prefer to leave still fairly open the question of how far the appendix to list 8 records transactions incidental to fighting at Eion and at the Hellespont in 448 (some of which we believed we could trace: *ATL* iii 59-61), how far it records the kind of exaction which Kleinias desired (e.g. the

payments from Kos: 8 i 92, 8 ii 102). Whether the defection of the Neleids in Miletos, c. 446, may have been partly provoked by D7, see Barron in *JHS* lxxxii (1962) 1-6.

³ He believes in the peace of 423: *Historia* x (1961) 161 n. 61, 175 n. 123. Here, too, in fact, he finds a document of the later twenties falsely ascribed to the mid century: but in this case the error is older than any modern views about the shape of *sigma*.

⁴ *Historia* x (1961) 159: *JHS* lxxxii (1962) 72 ff. On the rearrangement of tribute-periods which his doctrine requires, Mattingly writes again in *CQ* NS xi (1961) 158-60: but we are not persuaded.

received by her allies.⁵ This is a case, indeed, in which Mattingly's new context has little intrinsic plausibility, since the quota-lists give clear evidence of Milesian defection before 450, whereas Thucydides in his narrative of the Archidamian War gives none at all. Mattingly's case here rests rather on what he says about the *epimeletai*:⁶ these officers, who are mentioned in D11, were (so he maintains) first created in 426. This is one of those positive reasons of which we spoke above, and we return to it below. Meanwhile, the new context.

Mattingly assumes, as we have all done, that D11 deals with some phase of those troubles at Miletos of which ps. Xenophon speaks, iii 11. Ps. Xenophon gives no indication of date: in three places where civil war had started, Athens supported the upper class and had reason to regret it. At Miletos 'Athens chose the *beltistoi*, and within a short time these revolted and massacred the democrats'. We may distinguish three stages: (1) civil strife; (2) Athens co-operates with oligarchs; (3) oligarchs massacre democrats and leave the alliance: and since Miletos did not remain outside the alliance, we must add (4), a fourth stage when Athens recovers Miletos: we may assume she suppressed the unsatisfactory oligarchs and restored democracy. Mattingly identifies D11 with stage 4,⁷ and puts it (because of the archon's name)⁸ in 426/5. Of stages 1-3 Thucydides in his account of these years says nothing. Mattingly fills this void by importing into 426/5 an event which Diodorus puts in 405. This is no doubt the least plausible part of his case.⁹

Supporting evidence. He quotes two passages of Aristophanes' *Knights* (of 424 B.C.) where Miletos is mentioned in some connexion with Kleon:¹⁰ there was, surely, some basis for these jests, but we see no reason whatever to associate D11 with them. Besides that, he notes that D11 provides (probably: lines 10-19) for Milesian hoplites, and that Thucydides speaks of Milesian hoplites in 425 and 424: iv 42.1; 53.1; 54.1. Mattingly suggests that this is a new thing, due to D11: 'this is the first time in the war that he specifies any such contingent from the Empire'.¹¹ We note: (a) that Thucydides at ii 9.5 speaks of Athens' resources for this war and says that while some few allies provided ships, 'the others provided infantry (*παιζόν*) and money';¹² (b) that allied hoplites had served at Potidaia in 432 and at Tanagra in 458;¹³ (c) that in 425 Thucydides specifies others besides Milesians.¹⁴

⁵ Barron (note 2 above) proposes that the second Milesian revolt (of which ps. Xenophon speaks) was between 446 and 443 (p. 2) and was the work of those Neleids who earlier had supported Athens and the notion of kinship (p. 4: cf. p. 6). This may well provide a background of fact for what we wrote about cow and panoply, pp. 69-71.

⁶ *Historia* x (1961) 177.

⁷ The traces of Milesian oligarchy in D11 (lines 5-6, 67, and perhaps the word *σποπποῖσι* in 82?) suggest that this is stage 2 rather than stage 4. Mattingly recognises the problem (n. 132 and especially n. 144), and suggests that the officers named (probably) in 5-6 are 'hieratic' and concerned (merely?) with reconciliation.

⁸ The archon's name in D11 is Euthynos (or Euthynous): Diodoros knows no archon of this name, but three named Euthydemos, in 450/49, 431/0, 426/5. The facts have often been assembled: e.g. Hill's *Sources*² 398-9 (under the three years in question), and recently *Hesp.* xxvi (1957) 183.—We note that Mattingly implies that there is no reason to question Diodoros' Euthydemos in 450/49, and this blunts one of the weapons most used against our view of D13.

⁹ Plutarch (*Lys.* 8.1-3) and Polyaeus (i 45.1) associate the massacre, as Diodoros does, with

Lysander. Mattingly's pretext for moving it from this context is that Xenophon says nothing of it, and we must not (apparently) foist on Xenophon events of which he says nothing. So we foist them on Thucydides instead?—Barron (see note 5 above) gives a more felicitous explanation of ps. Xenophon: the oligarchs are the Neleids.

¹⁰ *Historia* x (1961) 176: *Knights* 361 and 927 ff. For the second passage, see Wade-Gery, *EGH* 234 n. 1.

¹¹ *Historia*, *ibid.*

¹² Mattingly wishes to discount this (176 n. 126), but does not explain how. It was, we imagine, a standing obligation and D11 is probably requiring acquiescence thereto.

¹³ Potidaia, Thuc. i 61.4. At Tanagra, Thucydides (i 107.5) names the Argives first and then τῶν ἄλλων ξυμμάχων ὡς ἕκαστοι: Pausanias saw at Olympia (v 10.4) the Spartan dedication for victory over Argives, Athenians, and Ionians (cf. *ATL* iii 249 n. 17). Mattingly understands these 'Ionians' as Euboians: but if (as we believe) the cities of Euboea, except Karystos which was not Ionian, were still furnishing ships in 458, we should not expect them to furnish infantry, cf. Thuc. ii 9.5.

¹⁴ Andrians and Karystians in the same force as the Milesians in 425, Thuc. iv 42.1: nothing suggests that these are consequent on any special recent

But D11 has a further provision which, in our opinion, necessitates a date before the peace with Persia in 450/49. Line 77 mentions *φρουροί*, the visible evidence of Athenian military occupation, which we believe not to have existed in any allied city on the coast of Asia Minor after the peace. Mattingly does not discuss this difficulty, but the absence of garrisons in the Asiatic cities of the Empire after 450/49 marks one of the significant developments of Athenian foreign policy.¹⁵

So we come to Mattingly's contention about the *epimeletai*: that they were first created in D8, in 426/5, so that D11 where we find them in existence is later. This will be cogent if (but only if) D8 really records their first creation.

Their occurrence in D11 line 42 is beyond question, and Mattingly has a very attractive restoration for lines 51-2, which suggests how they functioned:

- - γραφαὶ [δ]ντον κατ' αὐτὸ π[ροδοσίας πρὸς τοὺς ἐπιμελετάς · οἱ δ' ἐπιμελε]
ταὶ ἐσαγόντων - -

This keeps remarkably close to lines 45-8 of D8, and makes it probable that the *epimeletai* of 450 and 426 had substantially the same functions.¹⁶ But do lines 38 ff. of D8 contain the first creation of these officers? It can certainly not be proved that they do, and almost certainly they do not. They call for present appointment (in 426/5) according to the terms of an earlier decree (line 40). This earlier decree (how much earlier we do not know) was, conceivably, the original authorisation, and if so it will no doubt have been many years earlier. More likely, it was an intervening enabling act: comparable to the decree, with preceding probouleuma, which was necessary each year before the election of the strategoi,¹⁷ and to the decree which had to be voted to decide whether or not there should be an ostrakophoria.¹⁸ These enabling acts did not create the institutions of strategoi and ostracism; they merely set in motion the immediate application of a known and long-established practice. So here in D8 it is resolved to follow the dictate of the enabling decree and appoint *epimeletai*; they were a known institution, and their number, method of election, etc., which are conspicuously not mentioned in D8, were a matter of record. They seem to take care of punishable offences (*graphai* rather than *diadikasiai*) in connexion with public matters such as tribute.

The new feature in D8 is the obligation that one of the strategoi must always sit with this board. Furthermore, the implications of the present tense in *αἰρεῖσθαι* (D8 line 39) are that the *epimeletai* were not a standing board, but one that was appointed from time to time. It was one of the purposes of D8, and of its antecedent decree, to make sure that they were available in 426/5. As it turned out, there was probably more than the usual need for them, and for the strategoi to sit with them, following the establishment of the local boards of tribute-collectors.¹⁹

Kolophon: D15

Though not specifically ranged in the category of decrees with three-bar *sigma* that Mattingly uses as examples to show that a late date is possible and desirable, the decree concerning Kolophon now published as D15,²⁰ with its three-bar *sigma*, has been associated agreement. (The Imbrians and Lemnians of iv 28.4 may be cleruchs; the peltasts and archers, *ibid.*, are allies but not hoplites.)

¹⁵ Cf. *ATL* iii 142-4, and 284 n. 40.

¹⁶ *Historia* x (1961) 177. Note also the verb *κακοτεχνεῖ* in D8 line 43 and D11 line 50, and compare D8, 50-2 with D11, 53.

¹⁷ Aristotle, *Ἀθ.π.* 44.4. See also Wade-Gery, *BSA* 33 (1932/3) 121, lines 42-3.

¹⁸ Ar. *Ἀθ.π.* 43.5.

¹⁹ In the text of D11 Mattingly quite rightly questions the restoration of *Κεκροπὶς* in line 2, where (as he says) *Ἀντιωχίς* and *Ἐρεχθεῖς* are equally possible. Further, the name restored as *[Ὀρέτ]ορ* in line 3 admits of many other restorations.

²⁰ *ATL* ii 68-9 (D15).

by him with events in the early years of the Peloponnesian War rather than with the reduction of tribute in 447/6.²¹ Yet the oath (D15, lines 42–55) sworn by the Kolophonians, though longer, is much like that sworn by the Erythraian councillors in 453/2 (D10, lines 21–29) and that sworn by the Samians in 439/8 (D18, lines 15–21). And though Mattingly finds it 'the natural interpretation' of our documents to associate D15 with the settlement described in Thucydides iii 34, it is remarkable that despite the synoecism at Notion and the establishment there of the Athenian colony no mention of Notion appears either in the text of the inscription or in the oath which the Kolophonians were to swear. There is, however, mention in the inscription of the Diosiritai (line 26) and probably of the Lebedioi (restored in lines 25–6) and of colonists ([οἰκέτ]ρες, line 22) as well as of colonisers (οἰκισταί, lines [19], 41). The sending of Athenian colonists was in keeping with contemporary Athenian practice and was in this instance the natural excuse for the reduction in tribute of Kolophon, Lebedos, and Dios Hieron in 447/6 B.C., when the inscription is given its traditional date.²² We still hold, therefore, to the early date for D15.

*Hermione: SEG x 15*²³

Apart from title and prescript, only four words of this document are preserved. The physical criteria (particularly the three-bar *sigma*) appear to us to demand a date before 445: the argument for a later date depends on the absence of the name Hermione from Thucydides' list of cities which the Athenians ravaged from their base at Methana in 425 B.C. (Thuc. iv 45.2). In 430 an Athenian fleet had attacked Epidauros, and ravaged the territory of Troizen, Halieis, and Hermione (Thuc. ii 56.5): the raids of 425 B.C. were only against Epidauros, Troizen and Halieis. Why was Hermione spared, Mattingly asks, though it was more exposed to attack than Halieis? Since Athens and Halieis made treaty in 424/3 (*SEG* x 80) and some agreement was made with Troizen before 423 (Thuc. iv 118.4), Mattingly concludes (p. 173) that Hermione too had come to terms with Athens and that the text of *SEG* x 15 is that agreement.²⁴

There is nothing cogent here. The Athenians simply may not have attacked Hermione in 425 although they did attack Halieis. These raids from Methana were not on the quite unusual scale of the expedition of 430.²⁵ Nor was the coastal plain of Hermione so easy to attack as that of Halieis. It was narrow (relatively speaking) and could be defended from the city itself, which the broad and open coastline of Halieis could not.²⁶

It is thus likely enough that small raiding parties avoided Hermione, and we do not have to posit a separate agreement with her, c. 425, of which Thucydides says nothing. But Athens' interest in this piece of coast, so long as she is opposing Sparta, is evident (it was not unlike Persia's interest in Kolophon): if then we were to suppose that Athens twice made treaty with Hermione, in c. 450 and again in c. 425, there are other criteria than the three-bar *sigma* which indicate the earlier date for this inscription. One such is the use of three-point punctuation in the heading: [χσ]υνθέκαι : 'Ερμιονέον : καὶ 'Αθηναίον. This is not conclusive, for such punctuation occurs late in the century (rarely, as in *IG* i² 108 II of 407/6 B.C.),²⁷ as well as early (frequently, as in *IG* i² 1, 3, 4, 5, 42, 44, 50, etc.).

²¹ *Historia* x (1961) 175–6.

²² *ATL* iii 282–4.

²³ First published by J. H. Oliver, *Hesp.* ii (1933) 494–7 (no. 12) with a photograph on p. 494. A text is given in Hill's *Sources*², B29.

²⁴ *Historia* x (1961) 173.

²⁵ Thucydides gives the details, ii 56.1–2: 100 ships plus 50 more from Chios and Lesbos, 4000 hoplites, 300 cavalry on transports. How exceptional a scale it was, Thuc. vi 31.2.

²⁶ We have had the benefit of discussion of this part of the Argolid with Michael H. Jameson, who has explored the entire area in great detail; and Meritt has himself visited the sites of both Halieis and Hermione.

²⁷ See Meritt and Andrewes, *BSA* xlv (1951) 200–9; cf. *SEG* xii 37.

More telling is the moulding between the heading and the first line of the treaty. Lucy Shoe, whom we have consulted as an expert on the moulding, affirms that as between 450 and 425 (or thereabouts) she inclines to the earlier date. Although the proportion of depth to height may vary according to the position occupied by a moulding, in general the slenderness of the profile here is more closely paralleled by other mouldings of the middle of the century than by those of the last quarter. The lower edge of the upper inscribed surface is worn where it joins the moulding, but it must have been vertical originally, perhaps projecting ever so slightly in front of the top of the moulding below.²⁸ It cannot have affected the profile of the 'straight-sided' ovolo, which both in proportion and in character strongly suggests the mid-fifth century.

The five documents we have been examining are concerned with what may roughly be called foreign policy: D7 (tribute collection), D14 (uniformity of coinage), D11 (settlement of Miletos), D15 (colony to Kolophon), SEG x 15 (treaty with Hermione). For each of these, which we and others have dated in the mid-century, Mattingly proposes a new context in the twenties. We may perhaps add the treaty with the Persian king, which Mattingly holds to have been first made in 423.²⁹ Now, for the twenties we have what (relatively to what we have for the mid-century) may be called a fairly full background: we have Thucydides' main narrative, five comedies of Aristophanes, and other contemporary material. This fact cuts both ways: circumstances *not unlike* the circumstances of the document become rather easier to find, but the *argumentum a silentio* becomes more telling.

Between 450 and 420, the political map and the consequent problems of foreign policy are fairly constant: the consequences of the conversion of a league into an empire, so urgent in the early forties, were sharpened again when the Thirty-year truce ran into trouble. There was a second coinage decree:³⁰ tribute was stiffened and its collection for a war against Sparta needed further measures (D8). With regard to Kolophon (D15) and Hermione (SEG x 15), we note that the former, exceptionally far from the sea and near to the satrap, was exposed to recurrent pressures; and that Hermione and other cities of the Argive peninsula were likely objectives for Athens when fighting Sparta. For Miletos, on the other hand, the *silentium* weighs heavily; and now that Mattingly has sought to bring D16 and D17 (decrees for Eretria and Chalkis) down to the twenties,³¹ we would say the same about Euboea. The great importance for Athens of Euboea and Miletos is clear from the opening chapters of Thucydides' eighth book: if there had been really serious trouble in either, we are likely to have heard.³²

A rather different consequence of the ending (in 450 B.C.) of the Persian war, was the Periklean building programme. Problems of foreign policy are liable to recur: but the Periklean buildings, once built, stood for centuries. There ought to be less room for ambiguity about the dates of the building decrees.

²⁸ The profile was measured by John Travlos, who kindly forwarded a drawing of it for our study. A frontal view is shown in Oliver's photograph (above, n. 23) but we can now add that the height of the moulding is 0.015 m. and its depth 0.011 m.

²⁹ Above, note 3.

³⁰ SEG x 87: *Hesp.* xiv (1945) 119 ff., no. 11. Cf. Gomme, *Commentary on Thuc.* vol. iii 621 f., 626 f.: Mattingly, *Historia* x (1961) 168, 181.

³¹ 424/3: *JHS* lxxxi (1961) 124 ff.

³² We have not thought it part of our task to re-examine D16 or D17 (neither uses the three-bar *sigma*): but we note that when Aristophanes produced the *Clouds* in 423 (or perhaps when he revised it later: *JHS* lxxxii (1962) 70 n. 10), Perikles' campaign of 446 was still topical (line 213): it was what had brought Euboea to her present shape.

II. PERIKLEAN BUILDINGS

The Akropolis project

The Akropolis project, which raised the storm of controversy soon after Kimon's death, comprised three main undertakings: the *Parthenon*, Athena's temple on the south part of the Akropolis; the *Parthenos*, the gold and ivory statue which was to stand in the Parthenon; the *Propylaia*, the ceremonial gateway by which the Panathenaic procession entered the Akropolis on its way to Parthenon and Parthenos. All three constructions can be fairly exactly dated: the Parthenon, begun in 447/6, was finished in 433/2; the Parthenos, begun in 447/6, finished in 438/7; the Propylaia, begun in 437/6, finished in 433/2. Though the Propylaia does not cohere so closely as Parthenon and Parthenos, it was regarded as part of one project, as is seen e.g. in the words of Demosthenes (xxii 13) which gave rise to the comment in the Strasbourg papyrus.

This building project is not Mattingly's main concern. His doctrine on this is largely a by-product of his date for D12, and it is argued incidentally, and sometimes almost casually, in connexion with two more central doctrines, about heralds' journeys and about the use of 4. We believe all the three doctrines are false, but of the three the building project is historically the most significant topic, and we must now close with it.

We begin with D19, the so-called 'Springhouse' decree.³³ Date and interpretation of this document are difficult and perhaps not vital either to Mattingly's case or ours, but Mattingly has brought it into the discussion (pp. 164-5) and raised some interesting points. It does not use the three-barred *sigma*. It is remarkable (as Wilhelm has said³⁴) for the beauty of its lettering: there is a group of some half-dozen documents in this hand, which have been very roughly dated to the earlier and later thirties;³⁵ this one cannot be later than 430, since Perikles' sons are still alive, and it might be some years earlier. We have dated it to '437/6?' (*ATL* ii 74) and believe it to be two or three years before D1 of 434. Mattingly, in his note 73, suggests that it 'could come as low as 432/1'.

Of the main decree almost nothing survives. Of a first amendment, there is seldom more than a third of each line: the shorter second amendment has survived rather better. We print here a text of the two amendments, making partial use of Wilhelm's suggestions³⁶ for the first:

- 5 [- - Νικόμαχος εἶπε · τ]
 [α] μὲν ἄλλα καθ[άπερ τῇ βολῇ · ἐπισκευάζεν δὲ καὶ τὰς κρένας τὰς ἐν ἄστ]
 εἰ, ὁπὸς ἂν ῥέουσ[ι πᾶσαι ὁσὶ κάλλιστα καὶ καθαρότατα · ὁπὸς δὲ ἂν ἀπὸ ὁ]
 λιγίστον χρεμάτο[ν ἐπισκευασθῶσι καὶ κρῆναι τὸς πρυτάνες οἱ ἂν λάχ]
 οσι πρῶτοι πρυτανεύ[εν δόναι περὶ τοῦτο τὸν φεῖλον ἐς τὸν πρότερον τῶν]
 10 κυρίον ἐκκλησίον πρῶτ[ον μετὰ τὰ ἱερὰ · μισθῶσαι δὲ καθὰ ἂν δοκῇ ἄγα]
 θὸν εἶναι τοῖς δέμοις τοῖς Ἀθηναίοις καὶ ὁπὸς ἂν ἐς τὰ ἔργα πολυτέλεια με]
 δεμία γίγνεται καὶ ἔχει Ἀθηναίοις ὁσὶ ἄριστα καὶ κάλλιστα ε]

³³ *IG* i² 54 = Hill's *Sources*² B69. We do not now think that the decree was concerned mainly with a Springhouse: see below, p. 109.

³⁴ *JHS* lxxviii (1948) 129.

³⁵ Besides *SEG* x 44 which (as Mattingly has observed, n. 73) is by the same hand, there are further the four documents in the 'three-chisel' hand assembled by Wade-Gery, *BSA* xxxiii (1932/3) 122 ff.: viz. *IG* i² 77; 160; 61 + 169 + 179; 185 + two further fragments (= *SEG* x 60). None of these admits of exact dating. Wade-Gery (*ibid.*, 101, 134) supposed they were of early and late thirties: it is likely that they cover several years. A close

descendant of the same hand is seen as late as 415 B.C., *ATL* ii List 39 (see figs. 1 and 2 on p. 37); another, perhaps, as late as 408/7, in *IG* i² 118 (phot. Kern, *IG* 18). This last is in the Ionic alphabet, and the letters are also smaller than in the earlier examples, and neither of these later pieces keeps exactly to the cutter's strict practice.

One error in Wade-Gery's tabulation of that practice (*loc. cit.*, p. 122) needs correcting. *Chi* is made with two strokes of the 0·011 m. (not the 0·009 m.) chisel.

³⁶ *Loc. cit.* (see note 34).

- ἴπε· τὰ μὲν ἄλλα καθάπερ Νικόμα[χος· ἐπαινέσαι δὲ καὶ Περικλεῖ καὶ Παρ]
 ἄλοι καὶ Χσανθίπποι καὶ τοῖς ὑέ[σιν· ἀπαναλίσκεν δὲ ἀπὸ τῶν χρημάτων]
 15 ἡόσα ἐς τὸν φόρον τὸν Ἀθηναίων τελ[έται, ἐπειδὴν ἡε θεὸς ἔχσ αὐτῶν λαμ]
 βάνει τὰ νομιζόμενα.

The gaps are formidable. Even the second amendment, with which we are most concerned, is far from certain, but it seems probable that its purpose is (a) to express thanks to Perikles and his heirs for their offer to bear the cost of some public work, and (b) to make other provision for that cost—that is to be met out of tribute revenue after priority has been given (each year?) to Athena's claims.³⁷ These claims are called τὰ νομιζόμενα,³⁸ and we have understood this as a reference to those payments whose completion is recorded in D1 (first decree of Kallias) lines 3–4. There it is resolved 'to pay to the Gods the money due to them, now that the 3000 T. which had been voted have been conveyed to the Akropolis for Athena'. This was in autumn or winter of 434/3. It is certain, we believe, that no single sum of 3000 T. was so conveyed: what had been voted was a series of payments which by 434 totalled 3000 T. It is because this series of payments, as we conceive, was safeguarded in the last lines of D19, that we date D19 to c. 437/6—when there were still two or three payments to safeguard. This date is not acceptable to Mattingly, who disbelieves in the series of payments.³⁹

He suggests, p. 164, that this document is the basis for the story in Plutarch, *Per.* 14, where Perikles offers to pay for some project which his critics have called extravagant: as in D19, the offer is declined but the project approved. It is certainly an interesting parallel, but Plutarch dates his episode firmly in the middle forties,⁴⁰ a long way from Mattingly's 432/1. Is Plutarch's date wrong? or should D19 be put in the middle forties? or were there in fact two episodes of this kind? Mattingly infers that Plutarch has misdated the episode, which is no doubt possible: more surprisingly, he claims that Plutarch has misdated not only this episode but the whole controversy, in *Per.* 12–14, about the Periklean buildings. This must all come down to the thirties, and apparently to the later thirties.⁴¹

Our own belief is that this controversy was an early consequence of the peace made with Persia in 450/49. In our view the peace was made after Kimon was dead and before the temple building started.⁴² It called for a reappraisal of Athens' relations with her allies, since the tribute contract must now be revised: we recognise two attempts at that reappraisal in two Periklean decrees, D12 (the Congress decree) and D13 (the Papyrus

³⁷ The tense of τελ[έται] in 15 shows that revenue is spoken of, not reserve: the tense of [λαμ]βάνει in 15–16 shows that Athena's claims are recurrent (annual?).

³⁸ The phrase τὰ νομιζόμενα, as in Thuc. i 25.4 or D11 line 4, means 'what is recognised as the current practice'. The tense is such as to say nothing of how that practice was established: no doubt it will often have been established by public resolutions. Athena's recognised current claims on tribute money (safeguarded by this clause) will no doubt include the *aparchē*, but the *aparchē* by itself is nowhere else safeguarded in this manner, and this clause seems to us more reasonable if it safeguards something else as well, like the payments spoken of at the beginning of D1.

³⁹ Mattingly rejects our view that Athena received substantial payments of tribute money (that is, the Tamiai received them from the Hellenotamiai) in the years preceding 434: he writes (n. 75) 'basically this all rests on their interpretation of Thuc. ii 13.3 and the mutilated Anonymus Argentinensis papyrus'.

We believe indeed that the order for these payments was recorded in line 8 of the papyrus (see below, p. 108), and that Thuc. *loc. cit.* (if he wrote αἰεὶ ποτε as we believe) refers to the way these payments kept the reserve on an even keel; and we believe, further, that these payments are safeguarded in the last lines of D19 (see the previous note). But the firm foundation of our belief is of course D1. That rather difficult text demands to be read patiently, but (given that) we believe there can be no doubt what lines 3–4 mean.

⁴⁰ The episode is one of the things which culminate in Thucydides' ostracism.

⁴¹ In note 75 he suggests that it belongs to the time after Thucydides' return from ostracism, c. 433: perhaps this applies only to the single episode of ch. 14? P. 166 top, he suggests that the protests reported in *Per.* 12.2 were made 'on Perikles' final rendering of accounts'.

⁴² Besides the Parthenon, the temples named in n. 46 below.

decree). As we see it, the peace and the two decrees all belong to the year 450/49, the year of Kimon's death;⁴³ one consequence of D12 is that tribute was not collected in 449/8, while one consequence of D13 is that collection was then resumed, for 448/7 and thenceforward; so that in 447/6 work began on the Parthenon. Both decrees were essentially financial, and both alike posed the two questions: how was the fleet, and how were certain temples, to be paid for?

This, we believe, is what happened on Kimon's death and preceded the start of work on the Parthenon. Mattingly has a quite different date for D12, and feels himself therefore obliged to assault our whole position. He denies (a) that peace was made with Persia on Kimon's death; (b) that those 'Hellenic *hiera* burnt by the Persians' which were proposed for discussion in D12 included the Parthenon; (c) that D13 has any connexion with the start of work on the Parthenon.

Mattingly does not reargue the case about (a): he cites the recent articles of Stockton and Sealey, and where these disagree he follows Sealey in supposing that the 'peace of Epilykos' of 424/3 is authentic. We do not think that the case for the authenticity of the peace of Kallias needs restating.⁴⁴

On (b), that is, on D12 or the Congress decree, we wrote last year, and spoke of Mattingly's doctrine of 'heralds' journeys'.⁴⁵ We find that doctrine implausible, and not capable of carrying Mattingly's date (438 or later) for D12. The Congress was to have three items on its agenda. Items 2 and 3 imply that peace has been recently concluded: item 2 refers to the war in the imperfect (*ὅτε πρὸς τοὺς βαρβάρους ἐμάχοντο*), item 3 refers to the peace in the present tense (*ὅπως . . . τὴν εἰρήνην ἄγωσιν*). Item 1 is, we believe, even clearer: it proposes for discussion the problem of 'the Hellenic *hiera* which the barbarians burnt'. In a piece of special pleading Mattingly contends (p. 163) that 'the Parthenon can and should be separated from' these *hiera*. Possibly, he pleads, building did not start on the Parthenon site before 480: we need not go into this hypothesis, since even if it was true it would not stop the site being a part of the damaged *hiera*. His alternative plea, that it was 'incidentally' destroyed, so that 'the Persians could hardly be accused of wantonly burning it', is (surely?) not seriously relevant. He says nothing about the fairly numerous other temples in Attica known to have been started, like the Parthenon, in the early forties:⁴⁶ they are not likely to have been all completely new foundations. By 438 decisive action had been taken about all these temples, as well as about tribute.

That this abortive Congress was planned long before 438 is in our judgment virtually certain. We do not claim the same kind of certainty for (c), for our interpretation of D13: here we depend on the fragmentary Strasbourg papyrus in which more than half of each line has to be restored. Yet we believe that enough is extant to show the probable connexion of thought, and in particular that Perikles' motion is connected with the start of work on the Parthenon. The motion appears to be quoted, in the papyrus, primarily for the purpose of establishing a date after which building began (*ἤρξαντο οἰκοδομεῖν*), Demosthenes having implied that this was soon after Salamis. We have restored our

⁴³ We believe D13 to be the last of the three, and to be dated (in the Strasbourg papyrus) to this year.

⁴⁴ We now think Kyaneai, in the reported terms, to be at the northern mouth of the Bosphoros, and that therefore the demilitarised zone which Wade-Gery supposed between Kyaneai and Phaselis is non-existent. But we still hold that Asia Minor west of the Halys was barred to the King's own troops, as Isokrates states explicitly at vii 80 and xii 59 (cf. Herodotos v 52.2, and Xenophon, *Hellenica* i. 4.3). Isokrates' statement should not be regarded as a grotesque exaggeration of the 'autonomy' zone. A. Andrewes has an illuminating discussion of the

terms of the Peace as they dealt with the restrictions on movements of Persian troops (of the King and his satraps) in western Asia Minor (*Historia* x [1961] 15-18).

⁴⁵ *JHS* lxxxii (1962) 72-4. Mattingly's phrase in his note 90 (*op. cit.*, p. 168) 'studiously avoid' seems to us so exaggerated as to amount to a *reductio ad absurdum*.

⁴⁶ The Hephaisteion: the Ares temple, moved to the Agora in Roman times, perhaps from Acharnai: the temple of Poseidon at Sounion: of Nemesis at Rhamnous.

author's report of Perikles' motion as follows (we repeat what we wrote in *Hesp.* xxvi (1957) 187 f., indicating roughly the lines of the papyrus):

- 6: [to carry up] at [the Panathenaia for Athena] the money lying in the public treasury
- 7: [which had been collected from the cities], a sum of 5000 talents, according to Aristeides'
- 8: [assessment, and to carry up] to the Akropolis after that [a further 3000] during the period of
- 9: [construction: and in order to] maintain [control of the sea] the Council to [care for the] old triremes
- 10: [so as to] hand them over [sound], and to build new ones in addition each
- 11: [year, besides those already in hand, to the number of] ten.

This is Perikles' motion, 'of Euthydemos' year', cited in connexion with the start of work on Parthenon and Propylaia. We think the motion covers all the lines just quoted: besides the difficulty of finding any new lemma, the words *μετ' ἐκεῖνο* in line 8 seem to us (for lines 3-8 at least) almost decisive. In earlier studies of this papyrus (we include our own studies, in *ATL* i and ii and elsewhere) it has been assumed that these words start a new clause and are preceded by a heavy stop. On this assumption, no credible restoration has yet been found: the difficulties disappear when *μετ' ἐκεῖνο* is taken to qualify the preceding verb: *[ἀναφέρ]ειν εἰς τὴν πόλιν μετ' ἐκεῖνο*. This, we believe, restores *μετ' ἐκεῖνο* to a natural place in its own sentence and gets rid of a long-standing obstacle: and if we are right, this ties line 8 firmly to what precedes.⁴⁷ As for what follows, in lines 9-11, the connexion is less clear and the connecting phrase, such as it is, comes in the restored half of line 9. Even so⁴⁸ we have not found it practicable to detach these lines and conclude, provisionally, that they are part of Perikles' motion.

Though both D12 and D13 are concerned with the financial consequences of the peace, they no doubt differed very much in their tone. In D12 Perikles proposes a discussion on the most general lines and 'studiously avoids'⁴⁹ mention of any specific project. D13 seems to have focused, more narrowly, on the Akropolis project and (apparently) the maintenance of the Athenian fleet. As we conceive it D13 was concerned with tribute money, and only on these was tribute money to be used.⁵⁰ The Parthenon frieze depicts the Panathenaic procession, which was to be a festival for the tribute-payers as well as for Athens. The other Attic temples (Eleusis, as well as those mentioned in n. 46) had no doubt been understood to be included, as well as many outside Attica, among the 'Hellenic

⁴⁷ We hoped we had made our position clear on this, in *Hesp.* xxvi (1957) 187, as well as in the translation which we have repeated above. Yet Sealey in his criticism of that article (*Hermes* lxxxvi (1958) 442) still assumes that we put a stop before *μετ' ἐκεῖνο* and construct that phrase with what follows: 'Die Worte *μετ' ἐκεῖνο* *γινόμενον τῶν ἔργων* sind sehr unklar.'

⁴⁸ Our misgivings are as stated, *Hesp.* xxvi (1957) 183: a certain reluctance 'to separate these provisions from those shipbuilding provisions on which Demosthenes keeps insisting in xxii 8-20'.—But Sealey's criticism (*Hermes* loc. cit.: 'in der ganzen athenischen Gesetzgebung findet sich kaum eine derartige "lex satuta"') seems to us grotesque. The total of Athenian decrees or laws, which are anywhere near complete, is very small; financial decrees frequently deal with both defence and cult (e.g. D1, though that is no doubt far from complete); and the combination of these topics is specially

Periklean. See *Hesp.* xxvi (1957) 184 with n. 54 and p. 197.

⁴⁹ Mattingly's phrase in another context: see n. 45 above.

⁵⁰ On p. 164, with n. 71, Mattingly supposes that tribute money spent on the Akropolis works would appear in the accounts as 'received (by the epistatai) from the Hellenotamiai'. If this were so, then probably little more than the *Aparchē* came from tribute, and the violent language which Plutarch reports, *Per.* 12.2, is surprising. Of the possibility that much of what was received from the *Tamiai* came from tribute, Mattingly says nothing.

The *Tamiai* were astonishingly rich in the second half of the century, until the collapse of Athens. Apart from the building accounts this is clear from the series of documents of which Tod 50, 55, 64, 75, 81, 83 are examples. In our belief it was D13 which made them rich, and the source of this wealth was tribute.

hiera of D12, but we conceive that D13 did not provide for them. Those which were built were not built out of tribute money.

This brings us back to D19, an exceptional case where tribute money *was* used. We know almost nothing of what this project was: some watercourse was perhaps mentioned in the last sentence of the main decree (*τῆς ἀγογῆς* in line 5), and the first amendment appears to deal with watercourses (*ὁπὸς ἀν ῥέουσ[ι]* in line 7), but these are probably not the works mentioned in the main body of the decree.⁵¹ We have suggested in our supplements, as printed above, that this amendment dealt with improvement and repair of the city's water supply generally. This was to be undertaken, on motion of Nikomachos, according to plans still to be adopted (line 11), not yet ready for discussion (lines 9–10, the *prytaneis* are to have a motion ready for the next *ἐκκλησία κυρία*).

Excavations have brought to light a well-constructed stone aqueduct which runs from east to west across the full width of the Agora to supply the South-west Fountain House.⁵² The construction of the Fountain House and of the aqueduct, apparently all one project, has been dated tentatively to the last quarter of the century, but all the evidence from the pottery has not yet been assembled, and we are told by Thompson that the date may be within the limits, perhaps, of the last third of the century.⁵³ There is no evidence of anything but the most careful workmanship throughout. If the aqueduct and the South-west Fountain House are in fact to be associated with this decree, then it must be supposed that the work was postponed or for some reason interrupted and only later resumed. There may be any one of many explanations for the lapse of time between the decree and the completion of the work envisaged in the amendment.

In this respect the so-called Springhouse Decree (D19) resembles the decree for the temple of Athena Nike (*IG* i² 24 = *SEG* x 30). Nothing is preserved of the main body of that decree that can give any clue to its content. We have to infer what the decree was about from the nature of its amendments, and these deal with the appointment of a priestess of Athena Nike and the building of a temple and a stone altar. This decree is now once again brought to our attention by Mattingly,⁵⁴ who begins his argument for a late date by claiming that in it 'the Assembly voted for the appointment of a priestess for Nike's cult and the building of a temple'. It is necessary to bear in mind that this vote was in an amendment, that it was not part of the decree proper. Whatever the decree was about, it must have been something of such importance that the reorganisation of the cult of Athena Nike and the construction of a new temple and altar could be added merely as an appendage to some larger plan.

The actual construction of the temple of Athena Nike is now almost universally dated in the mid 420's, and the date of the inscription has been generally accepted, until Mattingly's challenge, as c. 448. The delay, of course, between the plan and its execution was due to a conflict of interest involving the south-west wing of the Propylaia and the precinct of Athena Nike. This was finally resolved, as the preserved monuments testify, by a very considerable alteration in the original plan of the Propylaia. But when the Propylaia and the temple were first authorised the conflict was not foreseen. The decree, therefore, belongs earlier than 437 at the latest (the date at which the Propylaia were begun) and may belong much earlier. Its main subject-matter was almost surely the architectural reorganisation of the western approach to the Akropolis. Nothing less than

⁵¹ The main business was some major construction for which Perikles had offered to pay, and which could go forward at once. We suggest, very tentatively, that this project was concerned with the Panathenaic approach to the Akropolis—and was thus (perhaps) involved marginally in the Propylaia project.

⁵² H. A. Thompson, *Hesp.* xxv (1956) 52–3. In

his report published in *AJA* lx (1956) 135, Thompson suggested a connexion between this aqueduct and the ἀγογή of *IG* i² 54 (= D19, line 5).

⁵³ His latest published report (*Hesp.* xxix [1960] 347–9) suggests a date too late for association with D19.

⁵⁴ *Historia* x (1961) 169–71.

this, which included the planning for and authorisation of the Propylaia, will have been of sufficient moment to carry the temple of Athena Nike merely as a rider.

So the decree IG i² 24, judged by its subject-matter alone, which is not preserved but which can be inferred, belongs to the 440's rather than to the 420's, and the delay in construction was caused by delay in the start of the Propylaia and by a local topographical quarrel for which we still have the architectural evidence.

This need not have prevented the reorganisation of the cult of Athena Nike and the appointment of her priestess, which we assume took place in the early 440's, as authorised by the amendment of Glaukos. We now possess the metrical epitaph of the first priestess,⁵⁵ who has been identified with the Myrrhine who figures with Lysistrata in Aristophanes' comedy of that name. She may have died about 405 B.C.,⁵⁶ but this hardly 'creates serious difficulty' for the appointment forty-two years earlier. Myrrhine was appointed for life, and there is no indication in the epitaph that her tenure was of short duration.⁵⁷

In the text of IG i² 24, Mattingly refers to the reading and restoration given by Meritt (SEG x 30). The reasons for preferring (lines 6-7) $\eta\epsilon\ \alpha[\gamma\ \kappa\omicron\iota\nu\epsilon\iota\ \eta\alpha\iota\rho\epsilon\theta\epsilon\iota]_i$ to $\eta\epsilon\ \alpha[\nu\ \delta\iota\alpha\ \beta\iota\omicron\ \eta\iota\epsilon\rho\alpha\tau\alpha]_i$ were given in *Hesp.* x (1941) 309-10. The stone is so preserved that the letter following the alpha cannot have been nu.⁵⁸ One cannot restore $\eta\epsilon\ \alpha[\nu\ \delta\iota\alpha\ \beta\iota\omicron\ \eta\iota\epsilon\rho\alpha\tau\alpha]_i$, but we now suggest $\eta\epsilon\ \alpha\gamma\ [\kappa\lambda\epsilon\rho\omicron\mu\epsilon\kappa\epsilon\ \lambda\acute{\alpha}\chi\epsilon]_i$,⁵⁹ which squares with the words of the epitaph $\epsilon\kappa\ \pi\acute{\alpha}\nu\tau\omega\nu\ \kappa\lambda\acute{\eta}\rho\omega\iota$ and is supported by a passage in Plato's *Politicus* (298e) which describes the election of archons: $\kappa\alpha\tau'\ \acute{\epsilon}\nu\iota\alpha\upsilon\tau\omicron\nu\ \delta\acute{\epsilon}\ \gamma\epsilon\ \acute{\alpha}\rho\chi\omicron\nu\tau\alpha\varsigma\ \kappa\alpha\theta\acute{\iota}\sigma\tau\alpha\sigma\theta\alpha\iota\ \tau\omicron\upsilon\ \pi\lambda\acute{\eta}\theta\omicron\upsilon\varsigma$, $\epsilon\acute{\iota}\tau\epsilon\ \epsilon\kappa\ \tau\omicron\omega\nu\ \pi\lambda\omicron\upsilon\sigma\acute{\iota}\omega\nu\ \epsilon\acute{\iota}\tau\epsilon\ \epsilon\kappa\ \tau\omicron\upsilon\ \delta\acute{\eta}\mu\omicron\upsilon\ \pi\alpha\nu\tau\acute{\omicron}\varsigma$, $\delta\varsigma\ \acute{\alpha}\nu\ \kappa\lambda\eta\rho\acute{\omicron}\mu\epsilon\nu\omicron\varsigma\ \lambda\alpha\gamma\chi\acute{\alpha}\nu\eta$ - - -. It follows also that the verb in line 8 was $[\kappa\alpha\theta\acute{\iota}\sigma\tau\alpha]_i$, and not $[\kappa\lambda\epsilon\rho\acute{\omicron}\sigma\alpha]_i$, as Papademetriou would have had it.⁶⁰ The text follows:

IG i² 24 = SEG x 30

[*Ἐδοχσεν τῇ βολῇ καὶ τοῖ δέμοι · Λεο*]
[*ντὶς ἐπρυτάνευε,*]

lacuna

[*. τὸς δὲ πρυτάνες χρειμ*]
[*ατίσαι περὶ τοῦτον ἐν τῷ] [δέ]μο[ι ἐν τ]*
5 [ἔϊ πρότεϊ ἡδῶραι · ν. Γλ]αῦκος εἶπε · [τῇ]
[Ἀθηναίαι τῇ Νί]κει ἡέρεαν ἡ ἐ ἄγ [κλ]
[ερομένη λᾶχε] ἐχς Ἀθηναίων ἡπα[σῶ]
[ν καθίστα]σθαι - - - - - κτλ. - - - - -

On the back of the lower part of the original stele was cut the later decree IG i² 25 (= SEG x 85).⁶¹ This is dated in 424/3 B.C., but we are ignorant of what the decree is

⁵⁵ SEG xii 80 = Papademetriou, *Arch. Ep.* 1948/9, 146-53.

⁵⁶ *Historia* x (1961) 169.

⁵⁷ Mattingly, *Historia* x (1961) 170 n. 103. When the Peloponnesian War broke out Chrysis had been priestess of Hera at Argos for 48 years (Thuc. ii 2.1): she continued as priestess for eight and a half years more (Thuc. iv 133), making fifty-six and a half years in all, and even then fled in the full vigour of life to avoid punishment for carelessly setting the temple on fire.

⁵⁸ In suggesting a change Mattingly is right, in a formal sense, in saying that the epigraphical evidence for reading $\alpha\gamma$ is not conclusive (*Historia* x [1961] 169 n. 95). But the evidence against reading $\alpha\gamma$ is conclusive, and this is important.

⁵⁹ The sloping stroke of *gamma* is visible on the squeeze in Princeton. Epigraphically, of course, it could be part of *alpha* or *delta*.

⁶⁰ *Arch. Ep.*, 1948/9, p. 150. Cf. Mattingly, *Historia* x (1961) 169 n. 98.

⁶¹ The stele was made of two pieces of Pentelic marble, one doweled above the other with a scarf-joint (cf. *Hesp.* x [1941] 311). It must have already been in position when the decree of 424/3 was inscribed on the reverse. The mason chose to inscribe this later decree on the lower of the two fragments of the stele, possibly in order not to loosen the joint by the hammering of the letters on the upper fragment.

about, except that in some way it was concerned with the cult or the sanctuary of Athena Nike. It has been Mattingly's belief that the decree stipulated when and by whom Nike's priestess should be paid her annual salary of fifty drachmai, and he finds it a 'rather odd fact' that this was authorised only some twenty-five years after the authorisation for the appointment. But the later decree does not authorise, belatedly, the payment of the priestess' salary. Presumably Myrrhine had been paid regularly from the beginning. The new decree plans something new about the cult (we can only guess what), and it begins with a recapitulation about something that will not be altered by the innovation. The opening lines of *IG* i² 25 merely acknowledge an obligation, and the new business of the decree begins where the stone is lost.⁶² This kind of chancery style in the writing of decrees finds a good example in the opening stipulation of *D2*.

The Epistatai at Eleusis: SEG x 24

This text was first published by Kourouniotis in 1932.⁶³ Meritt was able to examine the stone several times in May 1961. The amendment of Thespiesus, lines 6–34, is the only part which yields a continuous sense:⁶⁴ we print it here, with new readings in 31–2:

- - Θεσπιεύς [εἶπε · τὰ μὲν] ἄλλα καθάπερ
 ρ τῆ[ι β]ο[λ]ῆι, ἃ[νδρας δὲ] ἡλέσθ[αι] 'Α'θENAIO
 ν πέ[ν]τε, τούτ[ος δὲ] φέρειν τέττ[αρας] ὀβολοῖ
 [ς] ἡέκαστ[ον] π[α]ρὰ τὸν κολ[ακρ]ετῶν, ἡένα δ[έ]
 10 [τ]ούτον [γρ]αμματε[ύ]ει[ν] κατὰ] φσε[φ]ον · τούτο
 [ς] δὲ ἐπιστ[ῆ]ναι [τ]οῖς χρέμασι τοῖς τοῖν θ
 [ε]οῖν καθάπερ ἦοι ἐπὶ τοῖς ἐμ πό[λ]ει ἔργ[ο]
 [ι]ς ἐπεστ[ῆ]το[ν] τοῖς νεοῖ καὶ τοῖς ἀ[γ]άλματι ·
 [ἐ]χ[ο]σομοσίαν δὲ μὲ ἐν[α]ι · [τὸς δὲ] ἡειρεμένο
 15 [ς] προσιόντας πρὸς τὴν βολέν, ἐάν τι ὀφελ
 ὄ[μ]ενον ἔ[ι] τοῖν θεοῖν, φρά[ζ]εν καὶ ἀ[ν]απρά
 [τ]τεν · ἄρχεν δὲ ἐπ' ἐνιαυτῶ[ν] ὁμόσαντας με
 ταχρὸν τοῖν βομοῖν 'Ελευσῖνι καὶ τὸ λοιπ
 ὄν κατὰ ταῦτὰ ἡαιρεῖσθαι κ[α]τ' ἐνιαυτὸν τ
 20 ὅς ἀνδ[ρ]ας · ἐπιμέλεσθαι δὲ καὶ τὸν ἐπετε
 ῖον ἡὰ λαμβάνεται τοῖ[ν] θ[ε]οῖν καὶ ἐάν τι
 [ἀ]πολολὸς πυνθάνονται [ἀ]νασσί[ζε]ν · τὸς δ
 ἐ λογιστὰς λογιζεσθαι 'Ελευσῖνι μὲν τὰ
 'Ελευσῖνι ἀνελομένα, ἐν ἄστει δὲ τὰ ἐν ἄσ
 25 τει ἀνελομένα ἀνακαλὸντας τὸν ἀρχιτέ
 κτονα Κόροιβον καὶ Λυσανίαν ἐν τοῖς 'Ελε
 υσινίοις, Φαλερῶι δὲ ἐν τοῖς ἡιερωῖ ἡὰ Φαλ
 ερόνδε ἀνέλοται · ἀναλίσκεν δὲ ὅ τι ἂν [μά]
 λιστα δέει μετὰ τὸν ἡιερόν καὶ τῆς β[ο]λ[ῆ]
 30 ἐς βολενομένος τὸ λοιπὸν · ἀνα[κ]αλῆν δ[έ] ἃ
 πὸ τῆς ἀρχῆς ἀρχ[ο]σ[α]μένος ἡὲ Κτε[σ]ί[αι] παρ
 ἐ[δ]οκε[ν] τὰ χρέματα · γράφσαι δ[έ] τὸ φσεφίσ
 μα ἐν στέλει 'Ελευσῖνι κα[ὶ] ἐν ἄστει καὶ Φ
 αλ[ε]ρῶι ἐν τοῖς 'Ελευσιν[ίοι] - -]

⁶² It is possible, of course, that there had been irregularities in the payment, that Myrrhine had complained, and that the opening clause was her reassurance. But this can hardly have been the main business of the decree.

⁶³ 'Ελευσινιακά i (1932) 173 f. Besides *SEG* x 24,

a convenient text is available in Hill's *Sources*² B41.

⁶⁴ In line 37 Meritt finds the σ of παρέδοσ[αν] impossible: the letter has a hasta and may perhaps be κ. This excludes the present restoration of the second amendment.

First, the new readings in 31-2. It has always looked likely that *hekte* in 31 should be written *hè kte* -, the relative pronoun referring back to *archēs*, plus a new word beginning with *kte*. What came between *hé* in 31 and *τὰ χρέματα* in 32? At the beginning of 32 Kourouniotis read *O4OKE*[.]: when Meritt examined the stone, assisted by John Threpsiadis and Ronald Stroud, he was able to confirm *OKE*; but he did not believe in the supposed traces of *4*, and read the first letter as *E* not *O*. In 31 he was able to confirm the *I* which he had previously read from the squeeze in Princeton. The reading, then, is *κτε ε . οκε .*; [*παρ*]ε[δ]οκε[ν] seems virtually certain, and seems to require a dative for which we suggest *Kte[σ]l[ai]*.—Who was this Ktesias?

It was the purpose of the *logistai* to give to the new board of *epistatai* a completely audited portfolio. Their plan was to carry the audit back, on each item or on each sum of money, until they came to the board (*archē*) which transferred the money to Ktesias. He was probably the first *tamias* to receive the money of the Goddesses on the Akropolis. The money of the Eleusinian Goddesses had been kept on the Akropolis, at the pleasure of the Demos, since about 460 B.C. The authorisation is in *SEG* x 6 (Face C), lines 115-21:

- 115 τὸ δὲ *hierō* ἀργυρί[ο τῆς φυλα]
 [κ]ῆς ἐχ[σε]ῖναι Ἀθεν[αίοις μὲλ]
 [ε]σθαι, *h[ε]os* ἂν βόλο[νται, καθά]
 περ τὸ τῆς ἈθENAΙΑ[ς ἀργυρίο]
 τὸ ἐμ πόλει · τὸ δὲ ἀρ[γύριον τὸ]
 120 *s hieropoiois* τ[ὸ] το[ῦ θεοῦ ἐ]
 [μ] πόλει ταμιεύεσθαι - -]

Now, in 449 or 448 B.C., as we believe, it was to be stewarded by the new board of *epistatai*. Although the *hieropoioi* had been in control of it (*ταμιεύεσθαι*) since about 460, the money was actually, no doubt, held for safe-keeping by the *tamiai*, of whose first custodial board Ktesias may have been one.

This amendment orders the creation of *epistatai* at Eleusis, on the model of certain *epistatai* at Athens, and it names the architect Koroibos who is otherwise known only from Plutarch, *Per.* 13.7.

Plutarch, in a list of the famous Periklean buildings and their architects, says that Koroibos began to build the Telesterion at Eleusis, and erected the lower columns with their architrave; when he died, Metagenes placed on these the *diazosma* (whatever that may be⁶⁵) and the upper columns and Xenokles added the roof-lantern on top. In our inscription 'Koroibos the architect' is to be called by the auditors in connexion with what has been spent at the Eleusinion in Athens (lines 24-7); since his presence is not required in connexion with what has been spent at Eleusis (lines 23-4) it seems likely that he has not yet started his work at Eleusis.⁶⁶

Vital for the question of date is the model which Thespies names for his new *epistatai*. They are to 'take charge of' (*ἐπιστῆναι*, aorist) the two Goddesses' property in the same way as those in charge of the Akropolis programme *had charge of*⁶⁷ (*ἐπεσάτων*, imperfect) the temple and the statue' (lines 11-13). Here Mattingly finds his positive grounds for a late

⁶⁵ Whether *gallery* (Noack) or *frieze* (Mylonas: cf. Athenaeus 205c).

⁶⁶ Mattingly's inference (p. 171 with n. 108) is that 'at the time actual building was . . . in progress only . . . in Athens'; and in n. 112 he has Koroibos working at Eleusis from c. 435 to c. 432. But the audit will not be concerned mainly (if indeed at all) with work actually in progress: rather, with

the work of the past year or more: cf. the tense of *τὰ ἀνελομένα, ἃ ἀνέλοται*, in lines 23-8. We understand the audit as part of an immediate stock-taking, and it is to reach back over several years (lines 30-2).

⁶⁷ We thus translate *ἐπεσάτων*, quite literally, and without prejudice to its possible idiomatic sense, which we discuss below, p. 113.

date: the new board is modelled, he says, on 'some previous Commissioners who had now gone out of office. . . . The tense of ἐπεστάτων is decisive'.⁶⁸ The previous Commissioners, he says (rightly, we think⁶⁹) are the *epistatai* of Parthenon and Parthenos: the latter went out of office in 437/6, the former not till 432/1. He concludes that the inscription cannot be earlier than 432/1. He suggests the following tentative chronology:⁷⁰

Koroibos begins work at Eleusis	c. 435
he is moved to Athens	c. 432
this inscription	c. 432/1
work resumed at Eleusis	c. 430
Koroibos dies (of plague?)	c. 427
his successors finish the work by	c. 423

It should be noted that Noack, writing shortly before our inscription was found, suggested dates not unlike these. Iktinos⁷¹ surveys the site and projects the new building c. 440: then, with war imminent, he abandons the work and Koroibos and his successors proceed, during the war, with a modified plan. Noack was seeking to harmonise ancient statements and actual remains. The problem is (in part) that Strabo and Vitruvius name Iktinos as architect of the Telesterion,⁷² whereas Plutarch (who has spoken of Iktinos immediately before, in another connexion) names Koroibos and his two successors. All three writers mean, evidently, to speak of the same building, that is, the building of their own day, which stood from the fifth century B.C. until the catastrophe of A.D. 170. The actual remains, and especially the beddings prepared for the interior columns, suggest that more than one plan was mooted.

Noack's solution⁷³ was to associate Iktinos' name with one particular plan (an abortive plan for 20 interior columns) and to dissociate him completely from the 42-column plan which was eventually built. This he ascribed to Koroibos: and for this change of plan and change of architect he imagined some exterior cause, such as the imminence of war or the fall of Perikles.—SEG x 24 was published five years after Noack wrote. It does not, perhaps, actually disprove any of this, yet the naming of Koroibos in an Eleusinian document, which seemed to be of the early forties, suggested a rather different story. We believe Kourouniotis was right to suppose that the main part of the decree (now lost) had dealt with the construction of a new Telesterion, to replace the interim structure which had served since the Persian invasion. If the inscription is (as on the orthodox epigraphic view) of the early forties, then the new Telesterion will probably have been taken in hand in the early (or middle) forties; and it seems likely that Koroibos was from the start in some kind of charge.—This brings us to the question of the tense of ἐπεστάτων in line 13.

It is, we suggest, the idiomatic usage which Gildersleeve has defined under the name 'Imperfect of Points Assumed'.⁷⁴ He quotes an example from Plato, *Laws* 867 D: ὁ δὲ θυμῷ

⁶⁸ *Historia* x (1961) 171.

⁶⁹ As against Picard in *CRAI* 1933, 10 ff., and Vallois in *REA* 35 (1933) 196 ff., who suggested the *epistatai* of the Polias temple and the Promachos statue; they put the inscription, accordingly, c. 450.

⁷⁰ *Historia* x (1961) 172 with n. 112.

⁷¹ Iktinos, whom Plutarch does not name in this connexion, is named as builder of the Telesterion by Vitruvius and Strabo (see next note), and plays a very important part in Noack's story. References to Noack's book are given in n. 73.

⁷² Vitruvius vii pr. 16; Strabo ix 1.12 (395). Neither Vitruvius nor Strabo speaks of an abortive project, nor suggests that there was any change of

architect; but what they do say (κατεσκεύασεν Ἰκτίνος, Strabo; *pertexit*, Vitruvius) is more than what e.g. Mylonas reports (*Eleusis* [1961] 113), 'we learn from Vitruvius and Strabo that Iktinos . . . was commissioned to draw the designs of a new building'.

⁷³ Noack, *Eleusis* (1927) 143-4, 198-9. Noack's main conclusions were briefly resumed by D. S. Robertson, *Gk. and Rom. Archit.* (1929) 171-4, who writes (p. 172) 'on the fall of Pericles the work was apparently handed over to three new architects, mentioned by Plutarch'.

⁷⁴ B. L. Gildersleeve, *Syntax of Classical Greek* (1900) 96, §218.2.

μέν, μετ' ἐπιβουλῆς δὲ κτείνας, τὰ μὲν ἄλλα κατὰ τὸν πρόσθεν αὖ, τρία δὲ ἔτη, καθάπερ ἄτερος ἔφευγε τὰ δύο, φευγέτω. Plato legislates here for two kinds of homicide: the penalty for both shall be the same except that 'as the former's exile was to be (as we have seen) for two years, the latter's shall be for three'. Similarly Thespies: 'these men shall take charge of the Goddesses' property, as those at Athens were to have charge (as we have seen) of temple and statue'. If the usage is the same in the two passages, we infer that the Akropolis *epistatai* came in an earlier piece of the same body of legislation—namely, the Periklean building programme. This programme unfolded systematically. There was as yet no plan for the *epistatai* of the Nike temple: these must have come later.⁷⁵ There was no board yet of *epistatai* of the Propylaia: these began to function in 437/6. The plan for the western approach to the Akropolis may well have been made about 446 B.C., with work actually begun on the Propylaia when the Parthenon had been dedicated; and work on the Nike temple done in the 420's when there was some relief from the war.

The date, therefore, of SEG x 24 will be either 449/8 or 448/7, and its three-bar *sigma* will be in keeping with the traditional epigraphic view.

This idiomatic use of the imperfect is rare and probably colloquial: it is perhaps one mark among others of the informal drafting of this amendment. Another mark, no doubt, is the way the subject of the main infinitives in lines 23–30 is changed twice without warning. The logistai are subject of λογίζεσθαι in 23 and of ἀνα[κ]αλῆν in 30, but in 28 the subject of ἀναλίσκειν cannot be other than the *epistatai*. We may note, further, that the new *epistatai* were not in fact particularly to resemble their model. They are to take charge of χρήματα whereas the model had charge of ἔργα; and consequently the new *epistatai* prove to be in some ways more like the *tamiai*. They publish inventories rather than accounts; the office proves in fact to be permanent and not to finish when some job finishes.⁷⁶

An amendment, such as we have here, has not been before the Council but is drafted in the full Assembly. We may expect the drafting to be sometimes rather rough.

The decree to which Thespies made his amendment was, we conceive, of about 449/8 or 448/7: it was a portion of the building programme and was concerned with the new Telesterion. This need not mean there were no delays after this, whether of the sort imagined by Noack, or others;⁷⁷ and even without delays the large building will have required some years to finish. Professor Martin Robertson has called our attention to one concrete indication of date, namely the fairly numerous fragments of moulding which appear to come from the top course of the cella wall: Noack, *Eleusis*, p. 182, fig. 74; Lucy Shoe, *Gk. Mouldings*, p. 128, pl. 61.3.—We do not attempt either to date these mouldings closely or to determine exactly what stage the work had reached when they were put in place.⁷⁸

III. SOME EXCEPTIONS?

We have been concerned so far with the 'chancery style' of inscribed decrees: it is likely that the three-bar *sigma* survived occasionally in inscriptions of other kinds. It occurs, for example, in the names Thoukydides and Perikles, on a few ostraka which are

⁷⁵ Assuming that there ultimately were such. Above, p. 110. for the date of IG i² 24, and the delay in the construction of the temple.

⁷⁶ Thespies intends they shall be annual (line 17) with an annual secretary (9–10); yet in IG i² 311 we find Philostratos as secretary certainly for two, and probably for all four, of the years 422/1–419/8. In 409/8, 408/7, 407/6, we find that they and their secretary change annually, IG i² 313, lines 1–3; 314, lines 1–2. Strabo (as in n. 72) seems to say that

Perikles was *epistates* for Iktinos both for Parthenon and Telesterion.

⁷⁷ Cf., e.g., Dinsmoor, *Archit. of Ancient Greece* (1950) 195 f.

⁷⁸ We know of no reason to assign these mouldings to an 'Iktinos phase' (supposing that Iktinos and Koroibos phases are to be distinguished). They may indicate that the building was at least well advanced before the Spartan invasions began in 431. Eleusis lay, of course, right in the invaders' path.

most likely from the ostrakophoria of 443.⁷⁹ These are highly individual graffiti: there is a more important survival on the choregic dedication of Aristokrates son of Skelias: *IG* i² 772. This inscription unfortunately is lost, but there is no reason to doubt the report of its three-bar *sigma*. It was plainly archaising in style: it has one archaic *alpha*, and it was cut on a fluted column.⁸⁰ Aristokrates' career is best known from 421 B.C., when he swore to the peace with Sparta (Thuc. v 19), to 406 B.C. when he met his death as one of the strategoi condemned after Arginousai (*PA* 1904). If his age was, perhaps, sixty in 406/5, he could have been born about 466/5 and could have dedicated a choregic monument in the thirties. He would then have been about forty-five years of age when he swore to the Peace of Nikias and to the separate treaty with Sparta soon thereafter (Thuc. v 19; 24.1). But this is about the earliest possible dating: Kirchner gives his *floruit* as about 428 B.C.:⁸¹ the inscription may well belong to the Archidamian war, or even later.⁸²

Mattingly has also adduced the *horoi* (boundary markers), in Attic script, in Samos and Aigina.⁸³ The date of these is not indeed finally established, but it has often been supposed that those in Samos are not before the Samian surrender in 439, and those in Aigina not before the Athenian colony of 431. Their use of mid-century script has been ascribed to the conservatism to which *horoi* are liable⁸⁴ and (perhaps) the fact that they are outside Attica may be relevant.⁸⁵ But we understand that J. P. Barron is proposing (see n. 83) to call these dates in question.

The Messene Decree

There is still one document (*IG* i² 37), not discussed by Mattingly, which must be considered for the light it throws on the date of three-bar *sigma*. The stone carries a relief, probably a personification of the city-state of Messene,⁸⁶ and across the top, on a taenia, a proper name in which occurs one three-bar *sigma* [- -]οκλῆς Φι[- -]. The best interpretation of the monument is that it comes from a stele on which was inscribed an Athenian decree concerning Messene (or the Messenians in Naupaktos) and that the name at the top was that of the secretary in whose term of office the decree was passed. Meritt had no hesitation in accepting Hiller's date (*ante a.* 446/5) and argued vigorously against the later date advocated by those who based their opinions on the style of the sculpture.⁸⁷

The sculpture has been dated variously. Percy Gardner gave its date as the middle of the fifth century.⁸⁸ Rosemarie Binneboessel dated the relief about 410/09⁸⁹ and Hermine

⁷⁹ *IG* i² 911 (from A. Brueckner, *Ath. Mitt.* xl (1915) 8-12; Peek, *Kerameikos* iii 84-5, no. 160. These all have the name Thoukydides. In *Hesp.* x (1941) 3, fig. 2, we have the name Perikles, with old-fashioned forms of *chi* and *theta* in the patronymic: might this be from the ostrakophoria of 461?

⁸⁰ For inscriptions of fluted columns, see A. E. Raubitschek, *Dedications from the Athenian Akropolis* (1949) 5-28.

⁸¹ *PA* i 329, under the *stemma* of *Ἐπιχάρης Μικίωνος Χολλεῖδης* (*PA* 5003).

⁸² Mattingly, p. 149, reasonably suggests the Archidamian War. It was surely this Aristokrates who made the dedication, and not his grandfather as E. R. Dodds has suggested in his edition (1959) of Plato's *Gorgias* (p. 244, note on 472 a5-b3); he follows a suggestion of D. M. Lewis, but see now Andrewes and Lewis, *JHS* lxxvii (1957) 179-80. Plato lets Sokrates name Perikles ('and his whole house'), Nikias ('and his brothers'), and Aristokrates: there seems to be some oblique (dramatically ironic?) reference to their catastrophic ends.

⁸³ Mattingly, p. 149 notes 7 and 9; Hill's *Sources*,

B96. We understand that J. P. Barron will shortly publish a full account of the *horoi* from Samos, with photographs of those extant. He informs us that there are five in the Heraion Museum and two at Vathy: two others (*IGA* 8, and one similar to *SEG* i 376) he believes to be lost. We think that conclusions about the date and occasion of these *horoi* (and of similar ones in Aigina and Kos) should await his publication and discussion.

⁸⁴ The letter H (*heta*) is used for the aspirate in the word *horos* on many of the fourth-century *horoi* found in Attica.

⁸⁵ It is true that the copy of D14 which has the 4 was found outside Attica, at Kos: but it is fairly certain it was inscribed in an Attic workshop.

⁸⁶ The letters *Μεσση*[- -] are cut on the background beside the figure, with four-bar *sigma*.

⁸⁷ *Hesperia* xiii (1944) 223-9.

⁸⁸ *New Chapters in Greek Art* (Oxford, 1926) 235-6. He was influenced, partly, by the inscription.

⁸⁹ *Studien zu den attischen Urkundenreliefs des 5. und 4. Jahrhunderts* (1932) 6 (no. 17) and 40-2.

Speier fixed definitely on 425/4, restoring the archon's name Stratokles in the superscription.⁹⁰ We have, during recent years, discussed this relief with a number of experts in the field of sculpture, and have found a kind of *communis opinio* as of today that the figure is standing (not seated), that she displays a *déhanchement* characteristic of Attic sculpture later than the Parthenon frieze, and that a suitable date may well be somewhere in the 420's. But we have not, as a rule, found any great willingness to fix definitely upon a date; this is partly due to the poor preservation of the relief and partly, no doubt, to the subjective nature of many of the criteria. There has been some inclination to see in the headdress an influence of the Caryatids of the Erechtheion, but this has been merely a suggestion and not a firm argument for date. The drapery and the pose of the figure have been the significant items.

Through the kindness of H. Brunsting, of the Museum of Antiquities in Leiden, an excellent cast of this inscription, with its sculpture, has been forwarded to the Institute for Advanced Study in Princeton. This shows, more clearly than any photograph, the details of the sculptured figure. There can be no doubt that Messene was standing. She seems to have held a *patera* in her right hand, now largely broken away. One is reminded of the personification of Aphytis in the sculpture which adorned the Athenian treaty of 428 (D21).⁹¹ This confirms the impression that the Messene relief comes from a public monument, presumably a treaty, but the pose is different and the comparison with D21 offers no clue to the date. The experts in sculpture who have studied the cast prefer a date after the Parthenon frieze, basing their belief largely on the stance and the appearance of the drapery.

We differ from these experts with some misgiving, but we do wish to record our belief that the style of the hair and the frontality of the breasts may well be earlier. A date later than the frieze of the Parthenon would bring the relief down into the mid-thirties. But the standing pose (*déhanchement*) is like that of the female figure best preserved on the pedestal of the Pergamene copy of the Athena Parthenos.⁹² The copy is in Pentelic marble, and presumably, even if influenced by Pergamene taste,⁹³ it followed the original, which must have been completed at least before 438 and which was probably executed at some time in the 440's. How early, then, could a similar pose have been employed? If earlier than 440, we venture to believe a date as early as 446/5 not impossible. More significant, perhaps, because the sculpture is original and not a copy, is the similarity of attitude and of heaviness of drapery between the figure in our relief and the standing figure (though male) at the north end of the east frieze of the Hephaisteion.⁹⁴ Here the weight is carried on the left leg, the right knee is bent, there is a noticeable *déhanchement* of the left hip (covered by the drapery), and the right arm is thrown across the body, giving a pose in many respects comparable to that which appears in the relief. Dinsmoor's date for the Hephaisteion (449-444 B.C.),⁹⁵ argued largely on the basis of its architecture, reminds us that conclusions drawn from a study of the sculptural style may be subjective, and permits a reasonable belief that our relief is not out of place in the early 440's.—While, therefore we feel bound to record this *communis opinio*, we would wish to reserve our own judgment.

It is the opinion of Lucy Shoe, whom we have consulted, that the moulding on the relief may well belong to the mid-fifth century. It has an affinity with the ovolo of the abacus of

⁹⁰ *Röm. Mitt.* xlvii (1932) 24.

⁹¹ *ATL* ii (1949) 75 and pl. 12. A photograph is also available in Süsserott, *Griechische Plastik* (1938), pl. 2, no. 4.

⁹² Heinz Kähler, *Pergamon* (Berlin, 1949), pl. 4.

⁹³ Arnold Schober speaks of the departure from Pheidias severity to a freer and more mobile Pergamene style (*Die Kunst von Pergamon* [Berlin 1951] 113-14).

⁹⁴ Herbert Koch, 'Studien zum Theseustempel in Athen', *Abh. der sächs. Akademie d. Wissenschaften zu Leipzig* (phil.-hist. Klasse), xlvii (1955) Heft 2, pl. 34, fig. 28.

⁹⁵ *Hesp.*, Suppl. v (1941) 150-60. The date of the sculpture is still debated: C. H. Morgan, *Hesp.* xxxi (1962) 221-35, and xxxii (1963) 104-8, argues a date after the Peace of Nikias for the frieze.

the Ionic capitals of the temple of Athena at Sounion and with the ovolo of the Ionic capital from Kynosarges, as well as with that of the Ionic capital in Hadrian's Library, which latter Walther Wrede dates in the fifties of the fifth century.⁹⁶ The ovolos on all these capitals are, indeed, remarkably alike, and remarkably, also, like that on the Messene relief.

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⁹⁶ *Ath. Mitt.* lv (1930) 200. The temple of Athena at Sounion is dated in the second quarter of the fifth century (A. Orlandos, 'Αρχ. Έφ., 1917 183-4). The Ionic capital from Kynosarges was dated by Pieter Rodeck earlier than 471 B.C. (*BSA* iii [1896/7] 103), but a much later date, even in the fourth

century, has been suggested by R. Martin (*BCH* lxxviii-lxix [1944/5] 369) though he groups it in his table (*op. cit.*, 364) with the Sounion capitals. Lucy Shoe holds its moulding too early for the later date and favours still the second quarter of the century.

THE BEACON-SITES IN THE *AGAMEMNON*

Lines 283-5

"Ἴδη μὲν πρὸς Ἑρμαῖον λέπας
 Λήμνου· μέγαν δὲ πανόν ἐκ νήσου τρίτον
 Ἀθῶν αἶπος Ζηνὸς ἐξεδέξατο,

Beazley, quoted by Fraenkel *ad loc.*, has suggested that τρίτον Ἀθῶν αἶπος Ζηνὸς alludes to τρίτος (Σωτήρ) Ζεύς. The role played by Ζεὺς Σωτήρ in the religious thought of Aeschylus and in his treatment of the *Oresteia* myth is important (*cf.* Fraenkel on 1387), but this is not by itself sufficient to prove the allusion here. The route Ida-Lemnos-Athos shows no signs of having been artificially contrived in order to bring Athos into the third place, and while Aeschylus evidently intends to give the signal a divine send-off, so to speak, with the triad Hephaestus, Hermes and Zeus, the importation of a particular Zeus into the context makes no useful contribution to this end. τρίτον may undoubtedly evoke thoughts of Σωτήρ, but Aeschylean evocations of thought usually have point; hence, perhaps, the caution detectable in Fraenkel's approach to Beazley's suggestion. Our doubts may be resolved by the fact that even if we restrict τρίτον to the literal sense, Σωτήρ is already present in the context by implication, in the word Ἀθῶν. No audience could distinguish between Ἀθῶν and ἄθων, and three of the manuscripts have not succeeded in doing so either (ἄθων MFT^r). Aeschylus frequently embarks on word-play, particularly with proper names (see, e.g., *Suppl.* 45-7, 315; *PV* 732-4, 848-52; *Ag.* 681 ff.; *Aitv.* fr. 27 Mette), and in some of the cases is probably exercising the poet's prerogative to coin his own etymologies, but this may be an instance drawn from popular usage. Athos, rising to a height of 2033 m.¹ at the end of a promontory, must have been a familiar landfall to sailors in the northern Aegæan, and Ζεὺς Ἀθῶς, by analogy with such titles as Ζεὺς Ἰκέτης, may have been regarded amongst them as the patron deity of the unscathed, with apotropaic undertones, perhaps, if Mardonius' experiences in the area (*Hdt.* vi 44) were not unique. If so, the allusion to Σωτήρ in τρίτον may be more confidently accepted, as being prompted not by a whim in which Aeschylus indulged without regard to the sense, but by popular etymologising on the word Ἀθῶν which follows it immediately in the text.

Lines 286-93

ὑπερτελὴς δὲ πόντον ὥστε † νωτίσαι †
 ἰσχύς πορευτοῦ λαμπάδος † πρὸς ἡδονὴν †
 πεύκη τὸ χρυσοφεγγὲς ὥς τις ἥλιος
 σέλας παραγγέλλασα Μακίστου σκοπαῖς·
 ὁ δ' οὔτι μέλλων οὐδ' ἀφρασμόνως ὕπνῳ
 νικώμενος παρήκεν ἀγγέλου μέρος,
 ἔκτας δὲ φρυκτοῦ φῶς ἐπ' Εὐρίπου ῥοὰς
 Μεσσαπίου φύλαξι σημαίνει μολόν·

286 τε *codd.*: δὲ *scripsi* νωτίσαι *codd.* vix sanum 287 πρὸς ἡδονὴν *obelis notaverunt Denn.-Page*
post hunc v. lacunam statuit Paley 289 σκοπὰς *codd.*: corr. *Turnebus* 292 ἐκὰς *codd.*: *correx*

Fraenkel supports the narrative τε in 286 with the parallel in *Pers.* 413-18, ὥς δὲ πλῆθος ἐν στενῷ νεῶν | ἤθροιστ', ἀρωγὴ δ' οὔτις ἀλλήλοις παρήν, | αὐτοὶ δ' ὑπ' αὐτῶν ἐμβόλοις χαλκοστόμοις|

¹ The geographical data in this article are drawn mainly from *M. D. R.* 676, Ἑλλάς, 1 : 100,000. Heights are given in metres and distances in statute miles.

παίοντ', ἔθραυον πάντα κωπήρη στόλον, | 'Ελληνικαί τε νῆες οὐκ ἀφρασμόνως, | κύκλω πέριξ ἔθεινον . . . ; Broadhead conversely defends τε there against emendation to δέ by referring to Ag. 286. The interpretation of the *Persae* passage has been hampered by misunderstanding of ἀρωγή . . . παρῆν, which has left the editors uncertain where the apodosis starts and even led Koechly to transpose these words with ἔθραυον . . . στόλον, 416. ἀρωγή . . . παρῆν means not 'they could not help one-another' but 'they could not fend one-another off'.² The structure of the sentence now becomes clear: ὡς . . . παρῆν deals with the Persians' lack of sea-room and its causes, and the apodosis, which commences with αὐτοί, describes, first, the damage which it compelled them to inflict on themselves (αὐτοί . . . στόλον) and, second, the damage it enabled the Greeks to inflict on them ('Ελληνικαί . . . ἔθεινον). This leaves us with two anomalies in the same sentence, δέ in apodosis and narrative τε, which could be economically eliminated with Butler's emendation of δ' to θ'; this yields a simple 'both . . . and' construction to which the position of αὐτοί and 'Ελληνικαί is well suited. Unless, therefore, a further Aeschylean parallel for τε in Ag. 286 is forthcoming, the correction to δέ is justified.

The reasons generally adduced for accepting Paley's lacuna after 287 seem to me to be cogent, though I must dissociate myself from arguments appealing to the location of Macistus in Euboea.³ Further arguments for the existence of the lacuna will be provided below in the note on 292-3. Whether the lacuna is accepted or not, there seem to be only two ways of interpreting νωτίσαι: either 'the sea turned its back and fled' (so A. J. Beattie, *CR* lxviii (1954), 77-81) or, more popularly, the signal 'traversed the back of the sea' (schol. ὑπερβῆναι). Neither of these alternatives will square with the imagery of the beacon passage. Although the system is presented as a human arrangement for human purposes, conforming presumably to the practical requirements of visibility and geography, its more prosaic aspects have been kept well in the background. In the earlier stages, Ida-Lemnos and Lemnos-Athos, the human element yields place to the divine triad Hephaestus, Hermes and Zeus, and in the later stages, Messapion-Cithaeron and Cithaeron-αἰγίπλαγκτον ὄρος, where the activities of the φύλακες in keeping watch and lighting the fires receive notice, Aeschylus stresses not human vision discerning a static glare on the horizon but light (φάος, 292, 300, 302, 311; σέλας, 281, 289) endowed with independent life, passing from peak to peak with scarcely a pause at the link-stations. Hence, while he does not quite say that the light which came to the palace of the Atreidae was identical with the light that left Ida, he does remind the audience with the remarkable phrase φάος τόδ' οὐκ ἀπαππον Ἰδαίου πυρός, 311, that it bore the family resemblance, a τέκνον γονεῦσιν εἰκός. Probably also the article in τὸ χρυσοφεγγές . . . σέλας, 288-9, which has been treated as expendable by the critics who would emend πεύκη (ἐπεῦκτο Lawson, ἐπέσυντο Koch, ἐπᾶλτο Sidgwick), is not to be interpreted with Wilamowitz possessively of the light belonging to the πεύκη but as the light which went from Ida to Argos. Since the independent movement of the signal is stressed at the expense of human instrumentality, the light is not represented as moving on the horizontal line of sight but in a series of arcs, following the apparent curvature of the sky. So in 302, 308 and 310 the verb σκίπτειν, correctly explained by Beattie, describes its descent from the zenith as it prepares to come to earth.

This is the predominant image, but Calder is wrong to maintain that it is the only one, for Aeschylus, with characteristic fondness for variety, has introduced language suggested by the Greek λαμπαδηδρομία⁴ in 284-5, μέγαν . . . ἐδέξατο, 287, ἰσχύς πορευτοῦ λαμπάδος, 296, σθένουσα λαμπάς and 312-14, τοιοῦδε . . . δραμῶν. The image in 288-9

² See my article, 'Notes on the *Persae*', forthcoming in *CQ* lvi (1962).

³ See Calder, *CR* xxxvi (1922), 155 ff., and Fraenkel.

⁴ The further image of the Persian ἀγγραήιον,

282, compared to the λαμπαδηφορία by Hdt. viii 98.2, is not developed; the recurring παραγγέλλειν, 289, 294 and 316, applies literally to the transmission of the signal.

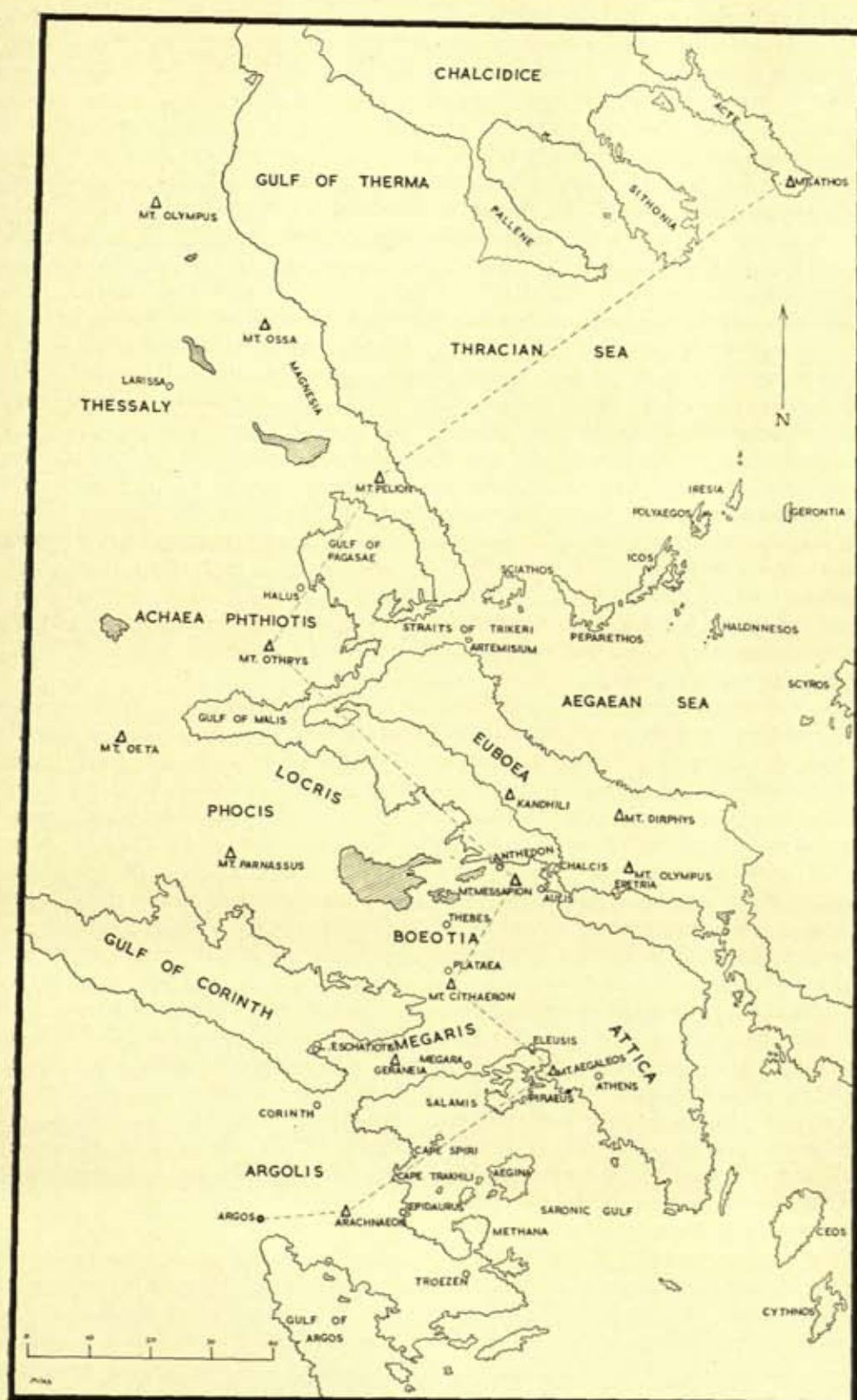
should not be included in this category. At first glance *πέυκη* combined with *ὥς τις ἥλιος* would naturally suggest a torch that is moving (cf. *ὑπερβοροῦσα . . . δίκην φαίδρας σελήνης*, 297-8), but *τὸ χρυσοφεγγές σέλας παραγγέλλασα* shows that it is light which is being transmitted here. *πέυκη* is, therefore, not a travelling torch but a fire blazing on a mountain peak ('pine-tree blaze', Fraenkel) which gleams like the sun at dawning (cf. *χρυσοφεγγές*), and *τις*, if it is discharging any special function, may be intended to apologise for a sun which is not rising in the east (the ancient world was rather sensitive about such matters).

These arguments lead to a conclusion which is of some importance for the textual criticism of 286-9: continuous and consistent sense cannot be made of 287 and 288 simply by piecemeal emendation of *ἰσχύς* or *πορευτοῦ* or *πέυκη*. As the text stands, whether *πορευτοῦ* is emended or not, the signal which leaves Athos is a torch which is being transferred, and the signal which arrives at Macistus is a beam of light which is being transmitted. Since a torch is transferred entire from one lap to the next and a beacon-fire does not move, 288 can only be made to follow 287 at the risk of conflating two incompatible images and producing the sort of confused picture to which Aeschylus, though he may be obscure at times, is not likely to have committed himself. In the rest of the beacon passage the two images have been carefully kept distinct, and where a transition from one to the other has been made for the sake of variety, it has been made at the link-stations and not in the middle of a stage. The only apparent exception to this rule occurs at 300-1, where, as we shall see, there are independent grounds for believing that the anomaly is due to an interpolator. The textual problem in these lines, cannot, therefore, be solved by the sort of half-measures adopted by Murray in the Oxford text and by Beattie. Editors who still maintain that the signal passes directly from Athos to Macistus must embark on some fairly ruthless emendation of words which have a *prima facie* claim to be genuine in a context such as this.

Although the torch-imagery diverges in detail from the light-imagery, it has not altered Aeschylus' general conception of the signal's progress. None of the torches mentioned is conceived to be transferred on the horizontal line of sight. The torch arriving at Athos has no trajectory specified, but the torch passing from Messapion to Cithaeron like the moon evidently moves in an arc and the torch which leaves Athos is *ὑπερτελής*, notwithstanding the transference of the epithet to *ἰσχύς*. In rendering *ὑπερτελής* 'paying more than was due (?)' Fraenkel is wrong to take a stand, though doubtful, on the formal derivation from *τελεῖν* (cf. Denniston-Page), for Soph., *Trach.* 36 and Eur., *Ion* 1549 demonstrate that in usage it was associated with *τέλλειν*. In both of the instances cited, however, the sense is completed with an objective genitive, and the Aeschylean *ὑπερτελής* is best paralleled with *ὑπερτείλας* used absolutely of the sun rising in the heavens by Hdt. iii 104.2.

Since, then, the signal *qua* light or torch moves in a series of arcs, the *ἰσχύς πορευτοῦ λαμπάδος* cannot be going 'over the back of the sea'; *ὑπερβῆναι*, the scholiast's attempt at a synonym, would suit its movement well enough, but *νωτίσαι* will not. And Beattie's alternative (which is linked with the theory of no lacuna and emendation of *πέυκη* to *λεύσει*) is likewise not permissible. Aeschylean imagery is profuse and varied, but it does not sanction the ruin of one image by the superaddition of a second. If the torch rises as it sets out from Athos, the sea cannot turn its back in flight; it must stand still. *νωτίσαι* is therefore corrupt, and what the torch did to the sea or the sea to the torch is open to speculation; 'mirrored' is a possibility, but *ὥστ' ἐνοπτρίσαι*, for example, does not carry much conviction for Aeschylus. The pattern of corruption at this point in the text, two corrupt line-endings and a lacuna following, suggests the mutilation of a papyrus sheet at the bottom and right-hand edge.

The above remarks on imagery may serve to clear up a latent difficulty in *ἐπ' Εὐρίπου ρόας*, 292. For *ἐπὶ* Denniston-Page give 'to' with 'over' as alternative, while Fraenkel decides for 'over', appealing to *ἐπ' εὐρέα νῶτα θαλάσσης* and similar expressions. But while



THE BEACON-SYSTEM FROM MT.ATHOS TO ARGOS

ἐπί = 'over' suits a ship travelling on and over the broad back of the sea, it does not suit a light travelling in an arc at a considerable distance from ground-level; even if the light were travelling along the line of vision, ἐπί would still be unsuitable. The natural preposition for this spatial relationship is ὑπέρ, and ὑπέρ is duly used in 297, ὑπερθορούσα, 302, λίμνην ὑπὲρ γοργῶπιν and 307, πρῶν ὑπερβάλλει. The editors' readiness to entertain ἐπί = 'over' probably springs from recognition of the difficulty that, if ἐπί means 'to', here, and here only, Aeschylus has used two phrases for the signal's immediate objective, ἐπ' Εὐρίπου ῥόας and Μεσσαπίου φύλαξι, and the two phrases are, strangely, not geographically coincidental. A simpler solution of this difficulty is available. Messapion was evidently not selected as a beacon-site for any distinction that it would boast in myth or history; the town of Anthedon to the north supplied Aeschylus with the local legend used in the *Γλαῦκος Πόντιος* (Paus. ix 22.5), but that is a far cry from τὰ Τρωικά, while in recent history the Boeotians in whose territory Messapion stood⁵ had submitted to Persia. What did recommend it was its proximity to Aulis. Aeschylus was bent on bringing the signal which heralded the fall of Troy to the very point from which the Greek fleet had set out across the Aegean ten years earlier (cf. the preceding parodos, 184 ff.), and then passing it on to Cithaeron as a compliment to the Plataeans who had staunchly supported Athens and the Hellenes in the Persian Wars. Messapion is the only high point on the mainland side which would serve his purpose. To keep matters in their proper perspective, therefore, the Euripus is brought forward in the sentence as the real objective of the signal and the mountain relegated to syntactical dependence on φύλαξι; this device is reinforced if, as I argue below, the first word should be emended to ἔκτας. These are small points, but they serve as a warning in the case of the beacons not yet identified that Aeschylus' choice of sites may reflect no humdrum routine with ruler and map but certain niceties of history and myth which he did not need to make explicit.

If ἐκάς, 292, is what Aeschylus wrote, its sense and application seem scarcely disputable. Since it qualifies a verbal combination expressing motion, σημαίνει μολόν (= 'signifies its arrival'), it means 'to afar' rather than 'afar'. This sense, which is not paralleled in the other two Aeschylean instances, *Ag.* 1104 and 1650, and is surprisingly rare, for a word of this sort, in the other fifth-century authors cited by *LSJ*⁹, is attested by Eur., *Phoen.* 907, at least. Why Denniston-Page give the unparalleled 'from afar' is not clear—it may be a slip—but if it is their intention thereby to extend the application of ἐκάς to the whole distance from Ida to Messapion, despite the fact that 292–3 are restricted to the stage Macistus–Messapion, it reads into the Greek more than is actually there. While an epithet such as *τηλέπομπον*, 300, may look beyond the limits of its clause, an adverb cannot do so without explicit qualification. ἐκάς simply means that Messapion is a long way from Macistus. Over one hundred years ago Thiersch⁶ argued that this indicates a site for Macistus in the far north of Euboea. Fraenkel believes that the theory 'does not compel assent'; it does not, but the difficulty it is designed to solve must surely compel us to find a Macistus which does not lie close to Messapion or, failing that, to emend ἐκάς. Fraenkel himself accepts the identification of Macistus with Kandhili, but the distance Kandhili–Messapion, a mere 16 miles across the channel, makes arrant nonsense of ἐκάς. Alternative Euboean sites are available; Dirphys in the centre of the island confers no advantage in terms of extra distance, but the heights in the north, from Xirón (991 m.) to Elliniká (395 m.) would give us a beacon-stage of 30–40 miles. If we extend the search to the mainland we can do better; the northern limit seems to be set by Othrys (1792 m.) in Achaea Phthiotis (see below), and Othrys–Messapion is about 57 miles. A stage of this length can scarcely be described as 'long'. It does not exceed the upper limit of approximately 60 miles set by the beacon-system which operated across Asia Minor in Byzantine times (cf. Calder, 157)

⁵ Calder's statement that all the beacons lie in the Athenian sphere of influence is unwarranted.

⁶ *Abh. Münch. Akad. Philol.-philos. Cl.* v. 2 (1849) 39; I owe this reference to Fraenkel.

and in Aeschylus' own system falls far short of two other stages, Ida-Lemnos (approximately 95 miles) and Athos-site X (precise distance unknown, but whether site X is in Thessaly or on any island of the northern Sporades group which is substantial enough to act as a beacon-station, it cannot be less than 70 miles). Nor has Aeschylus elsewhere in the beacon speech directed attention to the length of the stages. When the signal travels from Messapion to Cithaeron and from the αἰγίπλαγκτον ὄρος to Arachnaeon, it travels πρόσω (294, 307), but that means no more than 'onwards'. The light which arrives at Cithaeron is said to be τηλέπομπον, but not, surely, on the strength of the 23 miles or so which separate Messapion from Cithaeron, but because it is coming from Ida and going to Argos. The Ida-Lemnos stage would strain human vision to the limit, and yet it is not described as 'long'. The emphasis throughout the beacon speech falls upon the rapidity and continuity of the signal's progress and not upon the distances traversed. ἐκάς therefore arouses suspicion for two distinct reasons: it ineptly calls attention, all the more loudly for its emphatic position, to the unusual length of a stage which is decisively eclipsed by that of two stages which have preceded it, and it jars with the overall silence deliberately maintained by Aeschylus on the length of the other stages. Despite its pretensions to innocence we should be justified in emending it. The slight change to ἔκτας will restore to the text a satisfactorily idiomatic piece of Greek. Precisely as in τρίτον Ἀθῶν αἶπος Ζηνός, the ordinal numeral with adverbial force naturally attaches to the objective of the beacon-signal, ῥόας, and not to φῶς or φύλαξι (= 'to the streams of Euripus, which are the sixth stopping-place'), and, by normal hyperbaton, occupies the emphatic position in the sentence (cf. ἕκτον λέγοιμι ἄν ἄνδρα σωφρονέστατον, Sept. 568). A simple count shows that the traditional text gives only four beacons preceding Messapion, and so the lacuna after 287 and the omission of a beacon between Athos and Macistus are confirmed. There were nine beacons in all, and at regular intervals, the third and the sixth, Aeschylus kept the tally with two phrases identical in form, τρίτον Ἀθῶν αἶπος Ζηνός and ἔκτας ἐπ' Εὐρίπου ῥόας.

Macistus is the name of a mountain; this fact is obvious, so obvious that it has obscured the further fact that Aeschylus speaks of it as if it is not. The signal is received and passed on to Messapion not by φύλακες but by a Macistus who is considered, albeit by way of denial, to be subject to the human failings of tardiness, sleepiness (οὔτι . . . νικώμενος) and negligence (παρήκεν). In contrast with the other beacon-sites, which are not personified to any extent, Macistus' vigilance is stressed in oddly elaborate terms, which can only be accounted for by the poet's desire to play on the etymological connexion between Μάκιστος and μακιστήρ, μακίνειν. Here, as with αἰγίπλαγκτον (see below), the play is not effected directly but through an intermediate term roughly synonymous, μέλλων, which hints that Macistus' promptness in passing the signal on belied the ἔτνμον of his name. Μακίστου σκοπαί are, therefore, not simply the peaks of a mountain but the look-out posts of a personified mountain, who is himself conceived to be watching for the signal. The closest available parallel for such a concept is Soph., fr. 216 N., Θρηῆσαν σκοπιάν Ζηνός Ἀθῶν (cited by the commentators on 285, but more appropriate here), where, rather than render σκοπιάν as 'peak' (LSJ⁹), we should credit Sophocles with the intention of representing Athos as the look-out of Zeus, because there was a statue of Zeus on the peak.⁷

Μάκιστος is masculine for Aeschylus but neuter in the schol., Μάκιστον ὄρος Εὐβοίας. Variation in the gender of place-names is not unusual—the town Macistus in Triphyly was found in both the masculine and the neuter (Hecat. fr. 122 Jacoby, Strabo viii 3.16–25, Pliny *NH* iv 20)—but if both genders were established in ordinary usage, the scholiast's preference for the neuter variant against Aeschylus' masculine looks a little capricious.

⁷ Hesych. s.v. Ἀθῶς. For other look-outs see Eur., *Hel.* 1323–4 and *El.* 447 (text uncertain); in Simon. 130, Κιθαιρώνας οἰονόμοι σκοπαί, Cithaeron

is the mountain and not the 'king' of Paus. ix 3. 1–2, since it is bracketed with Pelion and Ossa.

One cannot avoid the suspicion that he was not familiar with a mountain of this name and, *faute de mieux*, identified it with the superlative *μήκιστος*, being aware, perhaps, that Aeschylus elsewhere affects the 'Doric' forms of *μήκος* derivatives in plain dialogue (*μακιστήρα* Pers. 689, in trochaic tetrameters, *μάκιστον* fr. 491 Mette, in trimeters), and hence assimilated the 'adjective' to the gender of *ὄρος*. In connecting Macistus with *μήκιστος* he is doubtless correct, but the etymology contributes nothing to the identification of the mountain; on the contrary, if this was the scholiast's procedure, we must be more critical of his location of Macistus in Euboea, which, as the commentators observe, could easily have been inferred from the general line of advance from Athos to Messapion and may even so be a mistake. The possibility that he had access to geographical knowledge which is not available to us and knew of a neuter *Μάκιστον* in Euboea cannot be refuted, but his location of the *ὄρος αἰγίπλαγκτον*, 303, in the Megarid will be demonstrated below to be erroneous, and the value of the scholium on 289 is proportionately diminished.

For a Euboean site the choice would appear to be restricted to Kandhili, Dirphys and the heights overlooking Artemisium; other mountains are either too insignificant or too far south, although Aeschylus may have permitted himself some deviation from the general line of advance from Athos for special reasons unknown to us. Kandhili has been the popular choice, so popular that the identification has established itself on some classical maps, but while the elimination of *ἐκάς* removes one objection to the identification, the text in its emended form raises another which is equally fatal. If the objective of the signal on the journey from Macistus to Messapion was the Euripus, we can be reasonably sure that Macistus was not itself on the Euripus; no poet would describe a signal reaching Sestos from Abydos as 'arriving at the streams of the Hellespont' unless he had taken leave of his senses. The coastal range of Kandhili is substantially no further from the Euripus Channel than Messapion; therefore it cannot be Macistus. The other two candidates are not subject to this disability. The hills of northern Euboea rise to a maximum height of 991 m., high enough to intercept a signal from Athos through an intermediate link. Some unidentified hills in this area, perhaps Elliniká (395 m.), were manned with look-outs from the Greek navy during Xerxes' invasion (Hdt. vii 183.1); it is possible that the fire-signals on Sciathos mentioned by Herodotus were actually observed from this area, but Sciathos lies close enough to Artemisium for the look-outs on the ships to have seen them for themselves. Nevertheless, neither in history nor in height are these hills distinguished beyond the ordinary, and despite their convenient position they must lose by comparison with Dirphys in central Euboea, which, with a maximum height of 1745 m., is more suited to long-range signalling. Literary evidence in support of the identification has recently been adduced by Beattie from Strabo x 1.10, 'Ερετρίαν δ' οἱ μὲν ἀπὸ Μακίστου τῆς Τριφυλίας ἀποικισθῆναι φασιν ὑπ' Ἐρετρίως κ.τ.λ. Beattie argues from this that a tradition ascribed the foundation of Eretria to a Macistus from Triphylia, that the *οἰκιστής* may have given his name to the sole peak of the Dirphys range which lies in Eretrian territory, Olympus, and that *Μακίστου σκοπαί* means Dirphys, the part being put for the whole. These arguments are far-fetched. Strabo evidently attests a tradition that Eretria was founded from the Triphylian town Macistus, not by an *οἰκιστής* Macistus, and even if the *οἰκιστής* be supplied, as he may be, from Steph. Byz. s.v. *Μάκιστος*, it is a tortuous road which winds from Triphylian Macistus via an Eretrian Olympus to the summit of Chalcidian Dirphys. The evidence would more easily support Olympus itself as the beacon-site, but Olympus (1171 m.) is too low to sight Athos directly over the intervening mass of Dirphys (Beattie believes there is no intermediate link), and even if an intermediate stage is admitted, Olympus is too close to Messapion to serve any useful purpose. On the other hand, the claims of Dirphys, the highest point of Euboea, must be allowed on grounds of geographical feasibility.

Against these considerations must be set the objection raised by Ahrens, that the form

Μάκιστος instead of the Attic-Ionic *Μήκιστος* is improbable for a mountain in Euboea. Fraenkel's reply, that the mountain may have been named from across the straits, would be convincing enough if Euboea were uninhabited, but, as it is, Euboean mountains are no more likely to have been named from Boeotia, for example, than Boeotian mountains from Euboea. More credible would be the theory that a Euboean form *Μήκιστος* was altered by Aeschylus himself for the same reasons, obscure as they are, which induced the Attic tragedians to use *μάκιστος* and not *μήκιστος* as the superlative of *μακρός*. Stephanus' entry *Μήκιστον, πόλις Τριφυλίας · Ἐκαταῖος Εὐρώπῃ*, contrasting with a separate entry for *Μάκιστος*, establishes that Hecataeus (fr. 122) accommodated the form of Triphylian *Μάκιστον* to his own dialect, and so we must evidently concede the paradox that since an Ionian writer converted a 'Doric' form into Attic-Ionic, an Attic writer may have converted an Attic-Ionic form into 'Doric'. Nevertheless, the existence of a Euboean *Μήκιστος* is not borne out by the geographical distribution of the place-name. As a designation for the highest mountain of the neighbourhood the name is so commonplace that we should expect other *Μήκιστοι* in the Attic-Ionic region, but no other mountain or town of this name is known. On the contrary, what does occur elsewhere is *Μάκιστος* or *Μάκιστον* for a mountain in Lesbos (Pliny *NH* v 140), *Μάκιστον/Μάκιστος* for the town and eponymous hero in Triphylia, probably associated with a local mountain (Hdt. iv 148.4, Strabo x 1.10, Steph. Byz. s.v. 'Μάκιστος'), and another town *Μάκιστον* further north in Elis.⁸ The idiom of nomenclature is not Attic-Ionic but Aeolic-Achaean.

A clue to the locality of origin is provided by one of the passages of Stephanus already cited, *Μάκιστος, πόλις τῆς Τριφυλίας, ἣν ὤκησαν Καύκωνες, ἀπὸ Μακίστου τοῦ ἀδελφοῦ Φρίξου, ἀφ' οὗ καὶ ἡ Φρίξα πόλις, ἐπ' ὄρους ὑψηλοῦ κειμένη πρὸς ἑω τῆς Λεπρεατικῆς κ.τ.λ.* Since there was a well-established tradition that Triphylia was settled by Minyae from northern Greece (Hdt. iv 145-8), the eponymous founder of Phrixia is evidently to be identified with the son of Athamas by his first marriage to Nephele. The complexities of the Athamas myth,⁹ reflect an early migration; Athamas, the eponymous hero of the Athamanians, married Nephele in southern Thessaly and after migrating or extending his influence into Boeotia, contracted a further marriage with Ino, which brought catastrophe upon him and his children. The myth in the form in which it appears in our sources is compounded of Thessalian and Boeotian elements which can scarcely be disentangled,¹⁰ but one thing, at least, is clear: neither the Thessalian nor the Boeotian tradition knew of any children of Athamas and Nephele other than Phrixus and Helle. The relationship of the hero Macistus to Phrixus is therefore a local Triphylian fiction symbolising the common origin and, perhaps, the friendship of the two towns, and Macistus, when stripped of his mythical connexions, looks like an eponymous invention. But the name Macistus or Macistum, for town and, perhaps, associated mountain, is likely to have been imported into Triphylia by the settlers along with Phrixus, thus deriving from the same area of Greece, southern Thessaly. Any attempt to locate the original Macistus more precisely than this through the association with Phrixus is hazardous, but it is worth noticing that the Thessalian tradition for Athamas and Phrixus, which was still very much alive in the time of Xerxes' invasion,¹¹ centred on the town of Halus in Achaia Phthiotis, founded by Athamas in the vicinity of Mt Othrys (Strabo ix 5.8, Steph. Byz. s.v. 'Ἄλος').

To revert to the geography, if Euboea is excluded, the only alternative is to look for a suitable site on the mainland in the area extending from Achaia Phthiotis in the north to Locris and Phocis in the south; the northern limit of the area of search seems fixed by

⁸ Steph. Byz. s.v. 'Μήκιστον'; the Ionic form derives from Hecataeus (cf. *supra*). The Elean Macistum is expressly distinguished from the Triphylian by Stephanus, but not by Bölte, *RE* s.v.

⁹ Apoll. i 7.3, 9.1, iii 4.3; Paus. ix 34.4; Hyg. i-v.

¹⁰ See A. B. Cook, *Zeus* i 414 ff., ii 899¹.

¹¹ Hdt. vii 197. The associated human sacrifices seem to have persisted into the fourth century (Plat., *Min.* 315c).

Othrys (1792 m.) and the southern limit is prescribed by the distance Athos–Messapion, which will scarcely allow another Boeotian site to precede Messapion. This area contains such striking ranges as Parnassus (2457 m.) and Oeta (2152 m.). If any peak in these mountain masses is Macistus, beacon-site X intermediate to Athos and Macistus must be identified with Pelion, for Othrys cannot at a distance of 115–20 miles receive the signal direct from Athos, and any other route, e.g. Ossa–Oeta or Ossa–Parnassus, creates similar or worse difficulties of distance. Even so the system has moved beyond the bounds of practical signalling; Parnassus–Messapion is inconsistent with Messapion–Cithaeron to follow, Pelion–Parnassus is much too long for a stage over land (Aeschylus, apparently, permits himself the longer stages, Ida–Lemnos and Athos–site X, where the signal has an uninterrupted passage over the sea) and the stage Pelion–Oeta is farcical, because the intervening heights of Othrys make the passage to Oeta unnecessary. Parnassus and Oeta, and lesser peaks in the area, must therefore be discarded as lying too far to the west or south, and the probability begins to emerge that Othrys is Macistus. Its height and its position make it an excellent look-out post. To the north and north-east it commands the plain of Thessaly as far as the coastal range of Magnesia, to the east the Aegean and northern Sporades through the Straits of Trikeri and to the south the Euripus along its length at least as far as Messapion 57 miles away. For site X we now have the choice of Olympus (2911 m.), Ossa (1978 m.) and Pelion (1548 m.) to receive the signal as it comes in from Athos. Olympus, pre-eminent for its height and association with Zeus, lies a tolerable 75 miles from Othrys, but suffers from its excessive distance from Athos, about 105 miles in contrast with the previous maximum of 95 miles (Ida–Lemnos). There is not much to choose between the other two, but since the route Athos–Ossa–Othrys gives approximate stages of 90 and 50 miles, and the route Athos–Pelion–Othrys stages of 85 and 32 miles, Pelion is to be given the preference for directness. Although the Greeks were familiar with the idea of long-range signalling by means of beacons (Hdt. ix 3.1), we cannot say whether they had ever used a system of the sort described by Aeschylus, but the route Pelion–Othrys–Messapion has one great virtue that would have recommended it strongly as a practical proposition in this area: it lies over the Gulf of Pagasae and along the Euripus, and thus ensures maximum visibility in poor weather conditions. In Greece, even when the weather is good, clouds tend to develop quickly and cling to the land masses.

The route via Thessaly conforms not only with the general direction (west-north-west) taken by the signal from Ida to Athos but also with contemporary trends in Athenian politics. Three or four years before the *Oresteia* was produced Athens, in a reversal of foreign policy, had embarked on an alliance with Argos and Thessaly. The stress laid upon the new link with Argos in *Eum.* 287–91, 667–73 and 762–74 shows that Aeschylus recognised its significance for Athens and also gave it his approval.¹² In 458, when the tripartite alliance was still young and untried, as far as we know from Thucydides' excursus on the Pentekontaetia, the Athenians must have had high hopes of the new connexions and the poet's oblique compliment would have been well received in the theatre; a little later, after the treachery of the Thessalian cavalry at the battle of Tanagra in 457,¹³ the reception would have been frosty. There is nothing inherently improbable in the idea that Aeschylus' distribution of the beacons was at some points conditioned by the international affiliations of Athens. He was not, as far as we know, circumscribed by tradition, and, as will appear from the following notes, he permits himself some deviation from the direct line of advance in order to include Cithaeron as a compliment to the Plataeans, and duly reserves for Athens herself the

¹² K. J. Dover, 'The Political Aspects of Aeschylus' *Eumenides*', *JHS* lxxvii (1957) 235, is more cautious, but I hope to show in a later article that the correct interpretation of Orestes' parting speech, *Eum.*

762–74, puts Aeschylus' sympathies beyond doubt.

¹³ Thuc. i 107; for the date see Gomme, i 411–12, on Diodorus and Theopompus fr. 88.

right to transfer the signal over the Saronic Gulf to the Argolid as a symbol of cordiality towards an ally.

Lines 294-301

οἱ δ' ἀντέλαμψαν καὶ παρήγγειλαν πρόσσω
 γραίας ἐρείκης θαμὸν ἄφαντες πυρί.
 σθένουσα λαμπάς δ' οὐδέπω μαυρουμένη
 ὑπερβοροῦσα πεδίον Ἀσωποῦ δίκην
 φαυδρᾶς σελήνης πρὸς Κιθαιρώνος λέπας,
 ἤγειρεν ἄλλην ἐκδοχὴν πομποῦ πυρός.
 [φᾶος δὲ τηλέπομπον οὐκ ἠναίνετο
 φρουρά, πλεόν καίουσα τῶν εἰρημένων.]

296 οὐδέπω *suspectum*: an οὐδ' ἀπημαυρωμένη? 300-1 *seclusi*

The traditional text of these lines presents us with a confused picture which is un-Aeschylean. Within the limits of a single beacon-stage the signal is represented both as a λαμπάς and as φᾶος, combining elements from two images which the speech elsewhere keeps distinct, the torch transferred and the light transmitted. It may be pleaded in extenuation that since λαμπάς and φᾶος stand in separate sentences which are consistent in themselves, there is nothing worse than a rather abrupt transition from the one image to the other, and that while λαμπάς is limited in its scope to the journey from Messapion to Cithaeron, the presence of τηλέπομπον, which cannot be restricted to the 23 miles separating Messapion from Cithaeron, proves that φᾶος means the light which travelled the whole distance from Ida to Cithaeron or even, perhaps, from Ida to Argos (see above). Even so φᾶος τηλέπομπον is suspect; nowhere else in the beacon speech does the poet pause for a synoptic view of the whole distance traversed, for the good reason that if he had done so, he would have slackened the relentless speed of the signal's progress. General reflections on the beacon-system would not be in order until the journey is accomplished, and it is at the end of the journey that Clytemestra at last permits herself such a comment, in φᾶος τόδ' οὐκ ἄπαππον Ἰδαίου πυρός.

On closer inspection the defects of 300-1 appear more serious. The torch is said in 296-9 to leap over the plain of the Asopus to the rock of Cithaeron and to arouse another reception, or 'relay' (Fraenkel), for the courier fire. With ἤγειρεν (aorist) one would have expected its mission to be fulfilled; the torch has reached its destination and has been handed over. There is no need for a fire to be kindled here, any more than there was on the summit of Athos, where the transfer of the signal from Ida was completed with the single verb ἐξεδέξατο (cf. ἐκδοχὴν, 299). The next sentence, however, introduces us to an unnecessary restatement of the signal's arrival in terms of light transmitted ('the garrison did not spurn, i.e. it accepted, the far-sent light'), followed by the kindling of a fire. Poets are sometimes repetitious, but the whole beacon speech is remarkable for its word-economy; with the exception of the playful etymologising on Μάκιστος/μακιστήρ in 290-1, there is scarcely a phrase that could be challenged as redundant. These two lines, on the other hand, give us nothing which is indispensable, for the kindling of the fire is necessitated only by the switch from λαμπάς to φᾶος; remove the light, and the need for the fire goes with it. Between the two stages Messapion-Cithaeron and Cithaeron-αἰγίπλαγκτον ὄρος Aeschylus is effecting a change, for the sake of variety, from the torch-imagery to the light-imagery (cf. φᾶος, 302), but φᾶος, 300, cannot be defended as paving the way to this end, for the change must be effected down on the ground, as at the other beacon-sites, and not up in the air. With the text as it stands, the method is to transfer the torch to transmit light to kindle a fire to transmit light; with 300-1 deleted, the method is simply to transfer a torch to transmit light. The garrison, φρουρά, 301, is not an essential part of the picture; there are φύλακες on Messapion and some anonymous watchers on the αἰγίπλαγκτον ὄρος

(cf. πέμπουσι, 305), but the guards manning the other posts are passed over in silence. The expulsion of these lines from the text would also neatly eliminate two stylistic defects, the awkward echo of πομποῦ, 299, in τηλέπομπον, 300,¹⁴ and the duplication of φάος in 300 and 302. A concise description of nine successive beacons necessarily taxes a poet's resources of vocabulary, but nowhere else in the beacon speech are we conscious of any stylistic awkwardness; the dominant words πῦρ, παραγγέλλειν, φάος, etc., are not distributed haphazardly but cleverly arranged as recurring themes. This twofold repetition within the space of four lines would, if genuine, convict Aeschylus of poverty, but as a forgery smacks of bungling improvisation perpetrated without regard for the context.

The question of the motive for the interpolation is bound up with the problem of the disputed words πλέον καίονσα τῶν εἰρημένων. Two meanings have been proposed, 'burning more than those (sc. beacons) aforementioned' (Denniston-Page), which appears too prosaic for Aeschylus¹⁵ but not for an interpolator, and 'burning more than it had been ordered' (Fraenkel), which is less prosaic but not so well supported. In either case the substance, considered within the bounds of its context, seems trivial, if not pointless; it is not apparent why Cithaeron should be singled out for a fire larger than, say, Ida or Athos, or why the zeal of the Plataeans should be illustrated with reference to a fuel prescription, presumably made by Agamemnon, instead of some more dignified idea such as ἀφθόνῳ μένει, 305. If the text of Aeschylus provides no solution to the problem, we may draw on outside help. According to Paus. ix 3 the Plataean festival of the Δαίδαλα, held in honour of Zeus and Hera, was celebrated every fifty-nine years on an extraordinarily elaborate scale with a sacrificial procession to the top of Cithaeron. In this, the Δαίδαλα Μεγάλα as distinct from the Σμικρά, representatives from the other towns of Boeotia participated with the local Plataeans. The offerings were placed on a great wooden structure—it is called by Pausanias a βωμός, but it was evidently conceived as the οἶκημα of the two deities—and consumed together with the altar in a great conflagration visible for miles around (εὐτρέπισται δὲ σφισιν ἐπὶ τῇ κορυφῇ τοῦ ὄρους βωμός, ποιοῦσι δὲ τρόπῳ τοιῷδε τὸν βωμόν· ξύλα τετράγωνα ἀρμόζοντες πρὸς ἀλλήλα συντιθέασι κατὰ ταῦτα καὶ εἰ λίθων ἐποιοῦντο οἰκοδομίαν, ἐξάραντες δὲ ἐς ὕψος φρύγανα ἐπιφέρουσιν . . . σὺν δὲ σφισι (sc. τοῖς ἱερείοις) καὶ αὐτὸν τὸν βωμόν ἐπιλαβὼν τὸ πῦρ ἐξανήλωσε· μερίστην δὲ ταύτην φλόγα καὶ ἐκ μακροτάτου σύνοπτον οἶδα ἀρθεῖσαν). The Plataeans interviewed by Pausanias derived the interval of fifty-eight years between celebrations of the Δαίδαλα Μεγάλα from the period for which the Plataeans of the classical era were in exile (ἐκλιπεῖν γὰρ τοσοῦτον χρόνον τὴν ἐορτὴν φασιν, ἥνικα οἱ Πλαταιεῖς ἔφευγον) i.e. either 427–387 to 373–38, which would make the institution of the Δαίδαλα Μεγάλα later than the *Oresteia*, but this makeshift account will deceive nobody. Neither of the periods of exile will account for the interval of fifty-eight years, and the primitive features recorded by Pausanias must date back further than the fifth century.¹⁶

Although Aeschylus must have known of the Δαίδαλα Μεγάλα and by 458 was old enough to have witnessed one such conflagration, he has not given the least hint of an allusion to it in the genuine lines 296–9; for him, evidently, the location of a beacon on Cithaeron was sufficient compliment to the Plataeans for the services they had rendered to Athens. But the interpolation may conceivably be an attempt to bring the mythical beacon into implicit association with the historical blaze of the Δαίδαλα and so to enhance the compliment paid by Aeschylus. Pausanias' description may, rather oddly, be invoked to support either of the suggested interpretations of τῶν εἰρημένων, since it testifies both to the record proportions of the sacrificial bonfire and to the burning of the altar along with the prescribed

¹⁴ Dindorf's replacement of πλέον . . . εἰρημένων with the anonymous fragment cited by Hesych. s.v. προσαιθρίζουσα πόμπιμον φλόγα evidently makes matters worse; but his hypothesis, that the traditional text is not Aeschylean, is correct.

¹⁵ τοῦ πάρος λελεγμένον, Sept. 424 and 555, is no parallel; in the first passage the sense is 'reckoned' and in the second 'chosen'.

¹⁶ Frazer, *Pausanias, ad loc.* and Müller, *Orcho-menos und die Minyer*² 216 ff.

offerings, but neither interpretation is attractive, even for an interpolation. The first, if we can be sure of the interpolator's mentality, seems too extravagant, because the Aeschylean system includes peaks which may never have seen a beacon-fire in practice, and the second seems too far-fetched and obscure. It would be simpler to refer τῶν εἰρημένων to Μεσσαπίου φύλαξι, in 293, in which case πλέον . . . εἰρημένων is reasonably explained as glorification of the fire-raising efforts of the Plataeans at the expense of the Boeotians on Messapion, and the Plataean φρουρά as a counter to the φύλακες. The allusion to the Δαίδαλα Μεγάλα is not necessarily entailed, but it gives more point to the contrast drawn between the conflagration on Cithaeron and the γραιάς ἐρείκης θωμόν on Messapion. A petty contrast such as this is quite alien to Aeschylus' liberal outlook, but quite in accord with the parochial jealousy and animosity nursed by the Plataeans for Thebes and her allies for much of the fifth century. The likeliest period for the interpolation is the Peloponnesian War, when the presence of Plataean 'evacuees' in Athens and on the Athenian citizen-roll ensured that the allusion did not fall on deaf ears.

Lines 302-11 (Fraenkel's text)

λίμνην δ' ὑπὲρ γοργῶπιν ἔσκηψεν φάος
 ὄρος τ' ἐπ' αἰγίπλαγκτον ἐξικνούμενον
 ὠτρυνε θεσμόν † μὴ χαρίζεσθαι † πυρός.
 πέμπουσι δ' ἀνδαίοντες ἀφθόνῳ μένει
 φλογὸς μέγαν πύργονα, καὶ Σαρωνικοῦ
 πορθμοῦ κάτοπτον πρῶν' ὑπερβάλλει πρόσω
 φλέγουσα · † εἴτ' † ἔσκηψεν † εἴτ' † ἀφίκετο
 'Αραχναῖον αἶπος, ἀστυγείτονας σκοπᾶς ·
 κᾶπειτ' Ἀτρεϊδῶν ἐς τόδε σκίπτει στέγος
 φάος τόδ' οὐκ ἄπαππον Ἰδαίου πυρός.

Textual corruption is evident in 304 and 307-8, but mercifully the topographical clues to the Cithaeron-Arachnaeon route have not been contaminated. The phrases γοργῶπις λίμνη and αἰγίπλαγκτον ὄρος allow a certain amount of latitude: γοργῶπις and αἰγίπλαγκτον may be place-names already familiar to the audience or allusive descriptions, perhaps familiar, perhaps not, and λίμνη may designate either fresh or salt water.¹⁷ Σαρωνικὸς πορθμός is more precise; since πορθμός is found elsewhere in Aeschylus only of the Hellespont,¹⁸ it should mean 'straits' here, and this sense is supported by πόρθμευμα = 'ferry-passage' in *Ag.* 1558 and πορθμεύειν = 'convey' in *Cho.* 685; clear traces of the original sense 'place for crossing'¹⁹ are still perceptible in the Aeschylean use. Since Σαρῶν was a king and a lagoon in a district of Troezen,²⁰ Σαρωνικὸς πορθμός would mean the narrow entrances between Aegina and the coast from Troezen to Epidaurus, across which ferries would have regularly operated from the Piraeus and Aegina to Epidaurus. Beattie seems inclined to restrict the application of the term Σαρωνικός even further, but in the presence of πορθμός it is difficult to see how this can be done or what would be achieved by doing it.

The identification of αἰγίπλαγκτον ὄρος with Geraneia, to which the scholiast apparently points with ὄρος Μεγαρίδος, provides no satisfactory account of Aeschylus' topography; it gives us no explanation of αἰγίπλαγκτον that would single Geraneia out from the generality

¹⁷ *Suppl.* 529 (lyric), where λίμνη means the sea, apparently open (though the chorus may be thinking of the Gulf of Argos), is not really relevant; the important thing is that the water should be enclosed.

¹⁸ *Pers.* 69, 722 and 799. 'Ελλήσποντον of the

Hellespont in *Pers.* 745 is remarkable, but it is restricted by Βόσπορον ῥόον θεοῦ in apposition.

¹⁹ Fraenkel, ii 165 n. 1, referring to J. Holt, *Glott.* xxvii (1939) 192 f.

²⁰ Paus. ii 30.7; see also the discussion of Strabo viii 2.2, 6.4 and 6.22 by Beattie, *op. cit.*, 80.

of Greek mountains, no γοργώπις λίμνη even of insignificant size and, even if the special localisation of Σαρωνικός be waived, no πρῶν or πορθμός. The Gerancia-Arachnaeon line passes over the lower foothills of Gerancia and the western recess of the Saronic Gulf; in this area there is no substantial promontory to justify the term πρῶν, and if ever a ferry service operated here, it operated from one sparsely populated coast to another, over a route which is only slightly shorter than the circuit by land.

More worthy of attention is the theory advanced by Beattie, which has found some favour with Denniston-Page, identifying the αἰγίπλαγκτον ὄρος with Aegina through αἶξ, the λίμνη γοργώπις with the bay opposite Megara and the Σαρωνικοῦ πορθμοῦ κάτοπτρον πρῶνα [*sic*] with the promontory of Methana.²¹ This provides substantial landmarks which were familiar to Aeschylus' audience and could therefore have been treated allusively, but it is open to several objections. Firstly, while it is characteristic of Aeschylus to etymologise a proper name, the -πλαγκτον part of the adjective remains unexplained, and it may be pertinent to ask whether the poet would have considered it good taste to subject a respectable nymph to such a chimerical etymology. Secondly, if λίμνη γοργώπις is the bay opposite Megara, we are left without an explanation of γοργώπις, short of transferring the anecdote of Gorgo's drowning (Hesych. s.v. 'Εσχατιώτις') here from Eschatiotis. Thirdly, an error which is more serious. Aegina lies, Beattie says, in line with Arachnaeon and Argos. This is true but irrelevant. We are concerned with a stage Aegina-Methana-Arachnaeon, not Aegina-Arachnaeon-Argos. The relevant question is whether Methana lies on the Aegina-Arachnaeon line; and it does not. Since Beattie speaks of the signal 'crossing in a southerly direction from Aegina to Arachnaeon', he seems to visualise it travelling approximately south-south-west to the coast near Saron and Troezen, and then turning west-north-west towards Arachnaeon. This is a violation of principle, a principle invoked by Beattie himself against the identification of γοργώπις λίμνη with Eschatiotis, when he says, 'A line Cithaeron-Eschatiotis-Gerancia is absurdly bent'. While the signal travels in an arc, the arc cannot lie in the horizontal plane; if it is said to pass over (ὑπέρ) a landmark, that landmark lies approximately, at least, on the line between two beacons (so Fraenkel). The theory could be rehabilitated in part by adopting Cape Trakhili instead of Methana for the headland, for Trakhili does lie on the Aegina-Arachnaeon line and does overlook the Saronic Straits (to substitute κάτοπτρον for the fantastic imagery of κάτοπτρον). Even so it will not do. The historical context of the *Oresteia* tells decisively against a beacon on Aegina. While the trilogy was being written Athens was preparing to embark on the offensive operations against Aegina which had been postponed by Xerxes' invasion; is it likely that Aeschylus selected an old enemy of Athens to relay the signal to her new friend?

The same historical factor points clearly to Athens herself, and Athens did, in fact, possess an αἰγίπλαγκτον ὄρος rising about 5 miles north-west of the Acropolis, Aegaleos. αἰγίπλαγκτον etymologises Aegaleos from αἶξ and ἀλᾶσθαι. The main peak of Aegaleos, lying south-west of the Sacred Way to Eleusis, is no more than 468 m. high; hence, no doubt, its claims have been neglected by commentators looking for substantial landmarks. But it commands a view to the north-west over the Thriasian Plain as far as Cithaeron 24 miles away and to the south-west over the Saronic Gulf as far as the mountains of the Peloponnese. From an ὄχθος lower down towards Salamis Xerxes had watched the defeat inflicted on his fleet in the bay (*Pers.* 466-7, Hdt. viii 90.4), and although we hear little more of Aegaleos in fifth-century history,²² the Athenians may have found it useful as a look-out post and signal-station which could be manned at short notice—perhaps, even, during the operations against Aegina.²³

The line Cithaeron-Aegaleos runs approximately south-east over the Bay of Eleusis

²¹ On the last point Beattie was anticipated by Ahrens, 499 ff.

²² A bare mention in Thuc. ii 19.2.

²³ The summit is at present occupied by the Greek navy, apparently for its commanding position.

(and, incidentally, over Eleusis, Aeschylus' birthplace). The *λίμνη γοργώπις* is thus satisfactorily identified as a substantial stretch of water which was familiar to all Athenians who had travelled along the road from the city to Eleusis and which, like Aegaleos, was historically associated with the victory over Xerxes' fleet. Since from most points on the mainland the bay appears to be completely shut off from the Saronic Gulf by Salamis, the term *λίμνη* suits it well enough (cf. Soph., *Trach.* 636, where the Gulf of Malis is called a *λίμνη* for similar reasons), but *λίμνη* in this sense is poetical and can scarcely reflect ordinary usage. Whether *γοργώπις* too is to be ascribed to the poet's imagination, as a descriptive or allusive epithet rather than an established name, cannot be settled beyond doubt. We do not know by what name the bay passed in the fifth century; Herodotus, who might have had occasion to designate it precisely and shows his familiarity with the names of the local landmarks, locates the concentration of the Greek ships before the battle of Salamis with the vague *ἐν Σαλαμῖνι* (e.g. viii 56.1, 65.3). In Strabo's time the term *Ἑλευσινιακὸς κόλπος*, interchangeable apparently with *Σαλαμινιακὸς κόλπος*, was applied to the whole of the northern part of the Saronic Gulf from the Megarid and Attica to Aegina (Strabo viii 2.2, 6.22), but this would not preclude the possibility that in the classical period the term *Ἑλευσίνιος κόλπος* denoted only the inlet stretching from the Megarid to Aegaleos, being later extended to embrace the area south of Salamis and the sea-approaches between Aegina and Attica, by a process paralleled in the case of *Σαρωνικός*. Nevertheless, whether we should write *γοργώπιν* or *Γοργώπιν* in Aeschylus' text (and in a context where *αἰγίπλαγκτον* strikes a riddling note the former seems more likely to be correct), the problem is fundamentally the same: what prompted *γοργώπις*? Fraenkel, in discussing the possibility of identifying Aeschylus' lake with Vouliagmeni near Perachora, takes the meaning to be 'Gorgon-eyed', comparing Vouliagmeni to 'a round and shining blue eye somewhat deep-set beneath its rocky brows', while LSJ⁹ gives 'grim-eyed, fierce-eyed' for *γοργωπός* and its cognates. Since the Gorgons were etymologically the Fierce Ones, clear differentiation is impossible in many instances, but in *PV* 356 *γοργωπόν*, of Typhon's glare, seems to be 'fierce-eyed' (cf. *γοργὸν ὄμμα* 'ἔχων, of Parthenopaeus, *Sept.* 537), and in Soph., *Aj.* 450 fr. 760.2 N., Eur., *Ion* 210, Eur., *El.* 1257 the sense 'Gorgon-eyed' is indicated by the association with Athena or her aegis. What connexion either of these two senses bears with the Bay of Eleusis is not self-evident. Etymological play of the same order as *Αἰγάλεως/αἰγίπλαγκτον* seems to be excluded, mythological association between the bay and the Gorgons or Athena's aegis is unknown and simple geographical proximity to Athens is a most inadequate explanation. If we fall back on a visible characteristic, there are two or three possibilities. The eastern end of the bay traversed by the beacon-signal appears land-locked and roughly circular; it could, therefore, have earned the description 'Gorgon-eyed' in the terms indicated by Fraenkel for Vouliagmeni, and perhaps with more justification, since Vouliagmeni is not round but roughly rectangular. Alternatively, as Webster suggests, the allusion may be to the glare of the sun's rays reflected off the bay in the late afternoon, which would give *γοργώπιν* the sense 'fierce-eyed' as in *PV* 356 (Webster calls attention also to the glare in the eyes of Heracles, Eur., *HF* 131, 868 and 990). These explanations seem inadequate, not because they are inapposite but because such features are too general; if roundness, blueness and rocky brows or a dazzling glare are the sole requisites, then most of the enclosed waters of Hellas must have been *γοργώπις* from one aspect or another. The only safe conclusion is that none of the available explanations offers us a sufficiently distinctive connexion between the bay and the epithet (or name). The key to the riddle may lie in the local lore of Eleusis or in the historical associations of the area, but in any case is beyond our reach as the evidence stands.²⁴

²⁴ A further item of interest deserves to be recorded. The shape of the coastline from a point west of Eleusis to the north shore of Salamis resembles the

profile of a human head with a ferocious expression, the Kirá Islands forming the eye and Leros the mouth (the present configuration of the coast should be

Arachnaeon, the objective of the signal as it leaves Aegaleos, has been certainly identified with the ridge Arna which runs east to west immediately north of the road from Argos to Epidaurus (Paus. ii 25.10). The highest peak of the ridge, St Elias (1199 m.), lying to the south of the modern village Arakhnaion (Kheli), commands a wide view over the Saronic Gulf and the Gulf of Argos, which makes it an ideal ἀστυγείτων σκοπή for Argos. The line Aegaleos–St Elias (about 45 miles) passes over Salamis, the highest point of which (365 m.) is too low to intervene, and crosses the coast of the Peloponnese within a mile or so of Cape Spiri, which, after Methana, is the most imposing headland on the coast. This meets the requirements of ὑπερβάλλει satisfactorily, for a discrepancy of a mile is tolerable at a range of nearly 30 miles (Aegaleos–Spiri). The shape of the promontory allows us to interpret κάτοπτον to the best advantage, for it juts out into the gulf in a north-easterly direction and then appears to turn to face the south-east, so that its most easterly point is at right-angles to the mid-point of the Aegina–Saron narrows.²⁵

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corrected in the light of D. Hafemann, *Die Frage des Eustatischen Meeresspiegelanstiegs in historischer Zeit, Deutscher Geographentag in Berlin 20–25 Mai 1959, Tagungsbericht und wissenschaftl. Abhandlungen* (Wiesbaden, 1960) 218–31, to which Mr F. H. Sandbach has drawn my attention). The resemblance is not by itself sufficient to account for γοργῶπις, for it is scarcely perceptible from a ship entering the bay or from any of the heights in the vicinity, but it may be relevant to an ultimate solution.

²⁵ For assistance with geographical data I am much indebted to Mr Markakis, General Director of the Greek Ministry of Public Works, Mr T. Nikolopoulos, an officer of the Ministry, and Mr J. Devery of the Department of Geography, Sydney University; for the map to Mr F. H. Jenkins of the Institute of Classical Studies, London University; and for valuable general criticism to Professor O. Skutsch, Professor T. B. L. Webster and Dr W. Ritchie.

THE DORIC ORDER: HELLENISTIC CRITICS AND CRITICISM

MANY authorities on ancient Greek architecture state that during the fourth century B.C. the Doric order was in a decline, and that by the Hellenistic period it was virtually abandoned for temples.¹ The archaeological evidence, it is argued, seems to bear this out. Doric temples were built during this period, but they can be dismissed, for instance by Dinsmoor (*Architecture of Ancient Greece* 267) as 'for the most part imitations of earlier works, and completion of earlier undertakings, together with a few sporadic but minor structures in which the style was adopted for conservative reasons'. It is clear, however, that the objection was not against the Doric order as such, since it was employed in all Greek areas to the virtual exclusion of the other orders (at any rate externally) in such buildings as stoas, of which large numbers were constructed during this period. We are to suppose, therefore, that it was only for temples that the other orders, Ionic and Corinthian, were considered superior to Doric, and that it was this belief that led to the decline and eventual abandoning of the Doric order. Since Vitruvius (iv 3.1) refers to statements by certain distinguished architects of the period to the effect that Doric was not suitable for temples, it would seem that the case is proved; moreover we are given the causes of this revulsion, *quod mendosae et disconvenientes in his symmetriae conficiebantur*, and more specifically, *quod impedita est distributio et incommoda in opere triglyphorum et lacunariorum*. The problems caused by the corner triglyphs in Doric buildings have been admirably expounded by Professor Robertson,² and it is not my purpose to discuss them further. I intend instead to discuss whether their effects, during the Hellenistic period, were quite as catastrophic as have been thought.³

It is essential first to see what the archaeological evidence is. The following lists of temples of the fourth century and Hellenistic period are compiled from Dinsmoor's *Architecture of Ancient Greece*, and include all buildings mentioned by him. The details of type, and the stylobate dimensions are intended as an approximate guide to the relative importance of the temples. Information in brackets has been supplied from other sources, and is often only approximate. I have divided the lists at 338 B.C., considering that the battle of Chaeronea, by altering the political foundations of mainland Greece, was not without some effect on the history of architecture, the most political of all the arts.

The information provided in this list is, of course, far from complete. Since Dinsmoor wrote his book several important temples have come to light, some of which are of great importance: the Ionic temple of Zeus at Labranda, for instance, and the Doric temple of Apollo at Klaros.⁴ There must be many other temples in places which have not been so thoroughly investigated as Greece, or where the discovery of ancient remains is now impossible.

Even so, it is unlikely that further discoveries of Hellenistic architecture will upset the basic pattern which emerges from these lists, namely that the construction of Doric temples, some of them, at least, major buildings, continued in mainland Greece, to the virtual exclusion of Ionic,⁵ while Ionic was normally employed in Asia Minor, though in those

¹ I am indebted to the University of Birmingham for a research grant which enabled me to inspect during the summer of 1960 the principal Hellenistic temples of mainland Greece.

² *Greek and Roman Architecture* 106 f.

³ Robertson, *op. cit.*, 110: 'It is not surprising that the triglyph problem killed the Doric tradition.'

⁴ For preliminary reports of Klaros, see, e.g., J. M. Cook in *Archaeological Reports for 1959-60* 42.

⁵ Dinsmoor considers that the Ionic capital found at Korone in Messenia belongs to an early votive monument, not the fourth-century temple (*AAG* 121).

DORIC TEMPLES

(i) MAINLAND OF GREECE

400-338 B.C.

<i>Locality</i>	<i>Deity</i>	<i>Date</i>	<i>Type</i> (peripteral front × flank columns, unless otherwise described)	<i>Stylobate dimensions</i> (metres)
Epidaurus . . .	Asklepios	380	6 × 11	11.76 × 23.06
Delphi . . .	Apollo	373 onwards	6 × 15	21.68 × 58.18
Kalydon . . .	Artemis Laphria	c. 360	6 × 13	(14.02 × 31.63)
Thebes . . .	Apollo Ismenios	(Time of Scopas)	6 × 12	(22.83 × 46.25 foundations)
Tegea . . .	Athena Alea	c. 350	6 × 14	19.19 × 47.55
Nemea . . .	Zeus	c. 340	6 × 12	20.09 × 42.555

338 B.C. onwards

Epidaurus . . .	Artemis	(340-330)	6 prostyle	(9.60 × 13.50)
Epidaurus . . .	Aphrodite	?	(2 in antis or 4 prostyle)	(7.50 × 13.65)
Epidaurus . . .	Themis	?	?	(4.87 × 7.26)
Olympia . . .	Metroon	(c. 320)	6 × 11	10.62 × 20.67
Lepreon . . .	?	(c. 320)	(6 × 11)	(c. 11.00 × 19.00)
Delphi . . .	Athena Pronaia	?	6 prostyle	
Molykriion . . .	Poscidon	?	(6 × 13)	(14.37 × 31.45)
Ptous . . .	Apollo	(316)	6 × 13	(11.65 × 24.72 euthynteria)
Stratos . . .	Zeus	(c. 311)	6 × 11	16.57 × 32.42
Lusoi . . .	Artemis Hemera	Hellenistic	4 in antis	(c. 20.00 × 32.00)
Oropos . . .	Amphiaraos	?	6 and 2 ½-cols. in antis	(14.41 × 30.86)
Lebadeia . . .	Zeus Basileus	175	6 × 13	cella 151 ft. long
Lykosoura . . .	Despoina	175-150	6 prostyle	(12.31 × 21.35 euthynteria)
Messene . . .	?	Early Roman	2 in antis	
Kourno . . .	?	Early Roman	2 in antis	(5.20 × 7.16)
Kourno . . .	?	Early Roman	6 × 7?	(7.98 × 9.96)

(ii) ISLANDS

Delos . . .	Apollo	314 onwards	6 × 13	12.47 × 28.53
Delos . . .	Isis	after 166	2 in antis	
Sikinos . . .	Apollo	?	2 in antis	
Kos . . .	Asklepios	200-150	6 × 11	(15.965 × 31.17)
Gortyn . . .	Apollo	?	6 prostyle porch added	
Lesbos . . .	Apollo Bresaïos	first century		

(iii) ASIA MINOR

Ilion . . .	Athena	before 281 (See note 21)	6 × 12	(16.40 × 35.70)
Pergamum . . .	Athena Polias	third century	(6 × 10)	12.27 × 21.77
Pergamum . . .	Hera Basileia	159-138	4 prostyle	(6.765 × 10.135)

(iv) ELSEWHERE

<i>Locality</i>	<i>Deity</i>	<i>Date</i>	<i>Type</i> (peripteral front × flank columns, unless otherwise described)	<i>Stylobate dimensions</i> (metres)
Cyrene	Apollo			
Hermopolis (Egypt) .	Ptolemy III and Berenice		6 × ?	
Selinus	'B'		2 in antis	
Akragas	Asklepios		2 in antis	
Taormina	Serapis		2 in antis	
Crimisa	Apollo Alius		8 × 19 added peristyle	

IONIC TEMPLES

(i) MAINLAND OF GREECE

None

(ii) ISLANDS

Messa (Lesbos) . .	Aphrodite	c. 280?	8 × 14 pseudo-dipteral	22.098 × 39.756
Kos	Asklepios	third century?	2 in antis	(8.50 × 15.07)

(iii) ASIA MINOR

400-338 B.C.

Ephesos	Artemis	356 onwards	8 × 21 dipteral	c. 51.44 × 111.48
Sardis	Artemis	c. 350	8 × 20 pseudo-dipteral	c. 45.73 × 99.16
Priene	Athena	340	6 × 11	19.53 × 37.17

338 B.C. onwards

Priene	Asklepios	late fourth century?	4 prostyle	
Didyma	Apollo	c. 313 onwards	10 × 21 dipteral	51.13 × 109.34
Chryse	Apollo Smintheus	?	8 × 14 pseudo-dipteral	22.578 × 40.436
Halikarnassos . . .	Ares			
Magnesia	Zeus Sosipolis	after 197	4 prostyle	(7.38 × 15.82)
Teos	Dionysos	c. 180?	6 × 11	18.63 × 34.98
Magnesia	Artemis	c. 175	8 × 15 pseudo-dipteral	31.60 × 57.89
	Leukophryene			
Alabanda	Apollo			
Ankyra	Rome and Augustus		8 × 15 pseudo-dipteral	
Mylasa	Rome and Augustus		6 × 7	
Aphrodisias	Aphrodite	c. A.D. 125	8 × 13	19.945 × 32.57
Aezani	Zeus	Roman	8 × 15	21.35 × 36.59
Termessos		Roman	6 ×	
Sagalassos	Apollo Clarius	Roman	6 ×	

regions the intruders—that is, Doric temples—were more numerous and more important. It is quite true that the total number of Doric temples constructed on the Greek mainland is likely to be lower than that of the sixth or fifth centuries. The reasons for this have nothing to do with any dissatisfaction with Doric as an architectural style, since, obviously, there is no evidence for the construction of Ionic temples in their place. The apparent decline of temple construction reflects the general insecurity of fourth-century Greece, the declining economic resources of the mainland, the tendency to devote these more limited resources, quite apart from military expenditure, to the construction of buildings other than temples—stoas, for example, or stone seated theatres. Moreover, the two previous centuries had seen mainland Greece lavishly equipped with magnificent temples, and, except as the result of disasters such as that which destroyed the temple of Apollo at Delphi, there was little incentive for further temple construction.

Equally, it is true that there are probably many more fourth-century and Hellenistic temples in Asia Minor of which we know nothing, and that the majority of these were certainly Ionic. Even if the Asia Minor temples greatly outnumbered those of mainland Greece, we must still bear in mind non-architectural factors governing this: that the Greek cities there were recovering from the stagnation of the fifth century; that many of their earlier temples had been destroyed; that, compared with the mainland, many of the cities were wealthy; and finally, that with the spread of Greek influence, many non-Greek peoples were becoming Hellenised, and, as a result, were equipping themselves with all the paraphernalia of Hellenism, including temples, Maussollos of Caria being an early and striking instance of this. On the other hand, this does not in itself imply any open rejection of Doric as an architectural style. The architects of these temples were simply building in the style which was local and traditional to their own particular regions, or, in the case of the Hellenised barbarians, the nearest Greek region. In no sense are these temples supplanting the Doric order. This all leads to the same point: the archaeological evidence does not show any decline, quantitatively, in the construction of Doric temples as the result of competition from the other orders.

The argument against Doric must therefore shift to the question of quality, that the Doric temples I have listed are, as Dinsmoor says, merely 'imitations of earlier works, and completion of earlier undertakings, together with a few sporadic but minor structures in which the style was adopted for conservative reasons'. One can legitimately claim that fourth-century Doric is not as beautiful or impressive as that of the preceding centuries. However, this is not the reason proposed by the Greek architects quoted by Vitruvius for the unsuitability of the Doric order: *non quod invenusta est species aut genus aut formae dignitas*. The reason for rejection is the corner triglyph, not the more slender columns of the fourth century, or the less beautiful mouldings, or anything of that sort. The corner triglyph has nothing to do with a decline in architectural quality, since the problem was just as real in the fifth century. Secondly, it does not follow that Ionic was in any better condition. It is all very well for Professor Lawrence to tell us that '(Hermogenes) proclaimed that Doric was unsuitable for temples—a reasonable opinion considering the contemporary state of the order'.⁶ We can, I think, quite legitimately doubt whether Ionic was any better off. Certainly the temple of Artemis Leukophryene at Magnesia, Hermogenes' most famous temple, impresses more by its size and position than by the details of the design,⁷ the clumsy frieze being merely the culmination of its crudities, and it is well known that there have been difficulties concerning another of his temples, that of Dionysus at Teos, in distinguishing Hermogenes' work from later Roman repairs.⁸ In some respects, it is

⁶ *Greek Architecture* 216.

⁷ This was not the ancient view, at least as reported by Strabo (xiv 647) who compares the temple favourably with that of Artemis at

Ephesus. I inspected the sorry remains at Magnesia in 1956.

⁸ Pullan, *Antiquities of Ionia* iv 39: 'It is evident from the plan that this temple was not that erected

true, Hermogenes' temples show attempts to return to the purity of design of earlier periods, though I doubt whether they succeed, and this must not be exaggerated; Hermogenes was no archaising purist, or he would not have retained the continuous carved frieze and Attic bases. It is a pity we have no directly comparable Doric temple; but this is the result of chance. If Antiochus Epiphanes' temple at Lebadeia had been completed, and if enough of it had survived for us to form a fair opinion of its qualities, it might well appear to be worthy of comparison with any of Hermogenes' work, with which it would have been contemporary. In other words: we may accept that in the detailed design and execution of individual temples there was a recognisable decline in standards during the Hellenistic period (for indeed the true genius of Hellenistic architecture lies elsewhere) but that this occurs as much in Ionic as in Doric architecture.

The antipathy against the Doric order is therefore by no means universal. It is rather the considered opinion of three particular architects, and bears little relation to actual architectural practice or general architectural ideas. The only reason we are given for the condemnation of the Doric order is the inevitable lack of symmetry in the arrangements of the triglyph and metope frieze, a shortcoming which does not seem seriously to have inconvenienced architects working in the Doric order. Nor do the strictures of these particular opponents of Doric seem to have had any real effect. If Doric was dying, it was for other reasons. It would seem more likely that the opponents objected to the Doric order rather as a matter of principle, and then tried to find a reason for their hostility, a weakness in the Doric order as such which they could use as a justification for their attacks. In order to see how their hostility came about, and to form some idea of its causes and its real extent, we must consider the three antagonists, Arcesius, Pytheos⁹ and Hermogenes.

We know least about Arcesius. In the relevant passage of Vitruvius his name is Rose's correction for the MSS. reading Tarchesius, the 'T' being assimilated from the previous word *negavit*. Rose assumes that the same name is concealed in Vitruvius vii praef. 12, where the MSS. all read Argelius. If we do not accept this identification, we know nothing further about him, other than that he rejected the Doric order for temples. If we accept the identification we can add that he designed and described in a handbook an Ionic temple of Asklepios at Tralles in Caria, and that he wrote a book on the proportions of the Corinthian order. Since in vii praef. 12 Vitruvius gives a list of architects who have written treatises on various architectural topics, and since the present condemnation of Doric is likely to have been made in such a work, it is reasonable to look for the names of these architects in that list. Both Pytheos and Hermogenes are found, while 'Argelius' is the only name that resembles '(T)archesius'. Both emendations are simple and straightforward, particularly as the MSS. of Vitruvius are habitually confused over the precise forms of Greek names and technical terms. The identification of Tarchesius with Argelius seems to be certain. We can deduce his date only from this limited information, if we assume that his work on the Corinthian order refers to its use for external rather than internal colonnades. The earliest major Corinthian temple known is Antiochus Epiphanes' temple of Olympian Zeus at Athens. It is quite possible that the temple of Zeus Olbios at Uzunçaburç belongs to the early third century.¹⁰ If Arcesius' treatise was a pioneering work, we can perhaps date him to the third century B.C.

Of Pytheos we know a little more. Though again we have the same textual complications in the MSS. (here the readings are Pytheus in H and G, Protheus in S), he must be identical with the Phyleos and Phileos of vii praef. 12, and the Pythios, Pithios and Pythius of i 1.12. From these passages we learn that he was the architect of the temple

by Hermogenes and described by Vitruvius as being eustyle. . . . The inferior character of the sculptures of the frieze and the inscription on the architrave prove that it was rebuilt in Roman times.'

⁹ I use the spelling of Krohn's edition of Vitruvius (Teubner).

¹⁰ Lawrence, *op. cit.*, 206.

of Athena Polias at Priene, and, with another architect, Satyrus, of the Mausoleum at Halikarnassos. On both buildings he wrote a handbook. In Pliny's garbled account of the Mausoleum (*NH* 36.30) his name appears as 'Pytis', and he is described as being responsible only for the marble four-horse chariot which crowned its pyramid roof. We are fortunate in having actual remains of his work, most notably, of course, the temple at Priene. His buildings date his floruit securely to the twenty-five years or so, 355 to 330 B.C. If there is anything in Pliny's information, it would seem that he worked on the Mausoleum in a subordinate position, and that he was then a young man. He may well have lived until the end of the fourth century, and his treatises, and so, perhaps, his condemnation of Doric, may belong to any time from 350 to 300 B.C.

Hermogenes is perhaps the most famous of all Hellenistic architects. His writings obviously greatly influenced Vitruvius. He was the architect (*Vitr.* iii 2.6) of the Ionic pseudo-dipteral temple of Artemis Leukophryene at Magnesia, and the 'monopteral' temple of Dionysus at Teos, on both of which he wrote descriptive treatises (*Vitr.* vii praef. 12). He was also responsible (*Vitr.* iii 3.8) for the various categories for describing the design of columns according to the proportions of column height and spacing in relation to the lower column diameter, and he 'discovered' the system of octostyle pseudo-dipteral arrangement, a claim that should probably be taken more seriously than it usually is. He was almost certainly a native of Priene,¹¹ and was responsible for the altar in front of Pytheos' temple there, and the completion of the temple itself. His antipathy to Doric was such that on one occasion he altered the design of a temple intended to be Doric and completed it in the Ionic order. This temple was dedicated to Dionysus; I shall argue below that it was in fact the famous temple at Teos.

It only remains to point out (and this is the most important fact about them), that all three architects were Ionians who worked exclusively, as far as we can tell, in Ionia and the immediately neighbouring districts such as Caria: Arcesius at Tralles, Pytheos at Priene and Halikarnassos, Hermogenes at Magnesia, Teos, and, perhaps, Pergamum. This is not, of course, fortuitous. All three architects come from that part of the world where the Ionic order was the established and traditional style of building. The ultimate reason for their antipathy to Doric is simply this. Not unnaturally, they considered the style to which they were accustomed was superior to all others, and in making the comparison with Doric they were bound to pick on the one element in its composition which could be interpreted as a weakness. What we have to consider now is what led them to exhibit their prejudices, to make their attacks on the Doric order.

In his most interesting book *Paradeigmata*¹² K. Jeppesen seeks to connect the prejudices revealed in this passage of Vitruvius with architectural developments in Attica of the fourth century. He suggests that 'the architects succeeding Ictinus and Mnesicles played a prominent part in the efforts at a renewal of the conventional orders resulting in the creation of a mixed order; Ionic columns combined with a Doric entablature'. He holds that fourth-century architects 'became accustomed to take an aesthetic pleasure in clear proportions, and that ultimately an architectural theory was created embodying a formulation of symmetry on the line of the geometrical definition of this word established by the Greek mathematicians'. So 'as a representative of a new epoch in the history of architecture Pytheus was forced to reject the Doric style, which because of the inevitable angle contraction was not so perfectly compatible with the principles of symmetry as the Ionic style'.

There are numerous objections to this. Despite Jeppesen's arguments it is by no means

¹¹ *Inscriptionen von Priene* no. 207 ἀνέθηκεν Ἑρμογένης Ἀρπάλων τοῦ νεοῦ ὑπογραφῆν, ἣν καὶ ἡργολαβήσεν: the absence of a reference to country or place of origin makes it likely that he came from Priene itself.

¹² K. Jeppesen, *Paradeigmata; three mid-fourth century main works of Hellenic architecture reconsidered* 153 f.

certain that Attic architects of the fourth century B.C. sought for a 'renewal of the conventional orders in the creation of a mixed order'.¹³ The only actual fourth-century examples of a mixed order come from barbaric misinterpretations of Greek architectural forms in the buildings of the Hecatomnid dynasty at Labranda in Caria, which have, perhaps, had too great an influence on Jeppesen's ideas.¹⁴ There, significantly, as in later degenerate Hellenistic mixed orders, Doric entablatures are superimposed on Ionic columns, a development which contributes precisely nothing to the solution of the corner triglyph problem. Undoubtedly Pytheos and his successors did hold that the symmetry of Ionic was preferable to Doric. Even if fourth-century architects were dominated by an all-consuming passion for symmetry,¹⁵ it was hardly this that led them to the mixed orders. Nor, as we have seen, did it lead to the replacement of Doric by Ionic as the natural architectural order for mainland Greece. If we can discount any such serious innovations in mainland Greece, it follows that the antipathy of Pytheos and his successors must be something essentially local, that is, concerned with the architecture of Asia Minor.

If this is so, it seems that the theories of Pytheos, Arcesius and Hermogenes are an example of attack for the sake of defence; that these Ionian architects, rather than seeking to convince their mainland brethren of the error of their ways, are defending their native Ionic against an insidious invasion of Asia Minor itself by the despised Doric order. Whereas from mainland Greece we have, as far as we can tell, no serious intrusion of the Ionic order after the Erechtheum, the intrusion of Doric into Asia Minor is more extensive. In the reconstruction of the Asiatic cities, lavish use was made of the stoa as an architectural unit, for utilitarian and aesthetic purposes. Invariably these stoas, externally, were constructed in the Doric order. At Miletus, during the Hellenistic period a leading Ionian city, the number of Doric columns employed must have been considerably larger than the number of Ionic (even though Ionic was still reserved for temples). We cannot be certain of the reasons for this. It may be that the stoa, as an object of architectural importance rather than a mere utilitarian structure, was developed in mainland Greece, particularly by Athens, who, after the tentative Ionic stoa at Delphi, continued with important Doric structures such as the Stoa of Zeus and the early South Stoa, both in the Athenian Agora. I should prefer to stress more practical considerations. The Doric order was easier and cheaper to produce, with its simpler shafts—no bases, fewer and simpler flutes, often partly reduced to mere facets—simpler capitals (particularly so when one considers the straight-sided echinus of the Hellenistic order), and the relative paucity of complicated mouldings, and the absence of carved decoration on these mouldings. Further, as a matter of convenience it was preferable to have fairly close-spaced exterior columns to support the roof, with wider-spaced, taller columns under the ridge; thus Doric naturally came to be used for external columns, Ionic for internal. Whatever the reason, there can be little doubt that the great Ionian cities were predominantly Doric in appearance during the Hellenistic age.

There was consequently a danger that Doric would completely oust Ionic. Here we see the difficulties that faced Hermogenes. It was manifestly impossible to stop the flood of Doric stoas. Indeed, the setting of Hermogenes' own masterpiece at Magnesia is within a courtyard edged with Doric colonnades and overlooking an agora almost completely Doric. It would not be right to exaggerate the danger, or, at least, this particular cause of danger.

¹³ See appendix.

¹⁴ Hecatomnid architecture is far from being pure Greek, and its peculiarities must not be attributed also to Greek architecture, though they may anticipate late Hellenistic ideas. Another instance is the almost square plan of certain peripteral Ionic temples, Zeus at Labranda, and Augustus at Mylasa.

¹⁵ A certain Silenus wrote *de symmetriis Doricorum*, and Philo, architect of the Piraeus arsenal, *de aedium sacrarum symmetriis* (Vitr. vii praef. 12). Both these books may well have been factual, referring to the actual proportions employed in their time (not, of course, symmetry in our sense).

The stoas were a fairly new development, certainly on this scale, and did not have behind them the architectural traditions which were of such great influence in temple design. The conservative and traditional outlook which seems to be inevitable in religious matters would normally see to it that the old traditional styles were employed for temples. However numerous the Doric stoas may be, they do not necessarily in themselves constitute a threat to the architecture of the temples. Their effect is more insidious; that is, they create an atmosphere favourable to the employment of Doric for temples, if ever a movement strongly urging Doric temples got under way. If this ever took place, it is not difficult to imagine the situation—the traditionalists deploring the innovations, extolling the virtues of the old native tradition against the defects of the upstart outsider, and finally laying it down as a firm law that however suitable Doric might be for the humdrum stoa, it was not suitable at all for temples. In a sense this is not merely blind hostility and prejudice. A temple, an offering to a god, had to be as near perfect as mere human endeavour could make it. I do not doubt for a moment that our Ionian architects sincerely believed that the triglyph problem constituted a flaw in the Doric order, and that therefore, in terms of pure perfection, Ionic was preferable—though Doric architects could legitimately retort that Ionic itself was flawed by the problem of the corner capital. I am sure that something like this lies at the heart of these criticisms of Doric, and that therefore they had no effect outside the immediate context of Asia Minor. The situation is particular, rather than general. These criticisms would not necessarily be accepted by other Greek architects working in other areas. They are totally unrelated to the archaeological evidence, whether this refers to the quantity of later Doric temples, or their individual quality. I feel that they have assumed an exaggerated importance in the works of later authorities, from Vitruvius onwards.

All that remains is to find the actual cause or causes which sparked off this wave of Ionic traditionalism. Basically, the cause must be the spread of mainland Greek influence to Asia Minor and the Ionian regions. The first instance of this was the creation of the Athenian empire in the fifth century, which led to the construction of a Doric temple on Delos.¹⁶ This was not serious. Indeed, it was accompanied by the few certain examples of Ionic influence on the mainland, whether or not these were the results of political considerations. It does not explain the objections of Pytheos and his followers. The threat that influenced Pytheos may have been the second Athenian empire, but this is unlikely, as by the time that Pytheos emerges as an influential architect (admittedly in the pay of Maussollos, an enemy of Athens), the second Athenian empire was moribund. More serious was the threat of Macedon. In 338 B.C. Philip of Macedon became master of the Greek mainland. Up to this point his Hellenic pretensions inevitably concerned mainland Greece. Despite Chaeronea he deliberately chose to remain on good terms with Athens and the other Greek cities. Within Macedon itself the mainland tradition must have been strong. The earlier Hellenising monarchs (Archelaos, for example) were connected chiefly with the mainland Greek cities. The actual Greek towns founded on the coastline of Macedonia, from Dion to Amphipolis, were themselves the colonies of mainland Greek cities. This inevitably means that the chief architectural influence on Macedon was Doric in character.¹⁷ This is, indeed, difficult to prove archaeologically. In general, Macedonian towns seem to have suffered severely from later occupation, invasion, and destruction. Much probably remains for the archaeologist to uncover, as is proved by the splendid results of Petsas' excavations at the Macedonian capital, Pella. The best evidence

¹⁶ It is not unusual for cities building in the great sanctuaries to use their own local style, rather than that of the sanctuary or its surrounding region: e.g. the treasuries of Siphnos, Cnidus and Cyrene at Delphi.

¹⁷ Further east Neapolis (Kavala) was an Ionian foundation, and so comes within the Ionic sphere of influence, as witness the important Ionic temple recently found there.

for Doric temples actually in Macedonia comes from Dion under Mt. Olympus, where there seems to have been at least one major building (*AA* 1933 242). In Thessaly, to all intents and purposes part of Macedon from the time of Philip II, there are Doric temples at Pazaraki (small and undated, *AA* 1931 269), Larissa (major temple of the fourth or third century, *BCH* lxxx (1956) 308), and Pherai (major peripteral temple of the fifth century B.C., with predecessors of the sixth and seventh centuries, *AA* 1927 389). The Doric order was also used for the peristyle and external colonnade of the 'palace' at Palatitza (new excavations, *BCH* lxxix (1955) 279 and lxxxi (1957) 601), for the lion-monument at Amphipolis, and for several Macedonian tombs.

There was not so much Ionic. It was used for the commemorative monument of Philip at Olympia, where its delicacy compared with Doric is perhaps a contributory factor; in some Macedonian tombs (superimposed as an upper storey over a Doric ground floor colonnade in the great tomb at Levkadhia),¹⁸ and as the order for the peristyle of one of the houses recently discovered at Pella (another house, however, uses Doric). The Ionic employed in the Philippeion is hesitant and barbaric, suggesting that at this time the Macedonians had little real acquaintance with it. Some fifty years later they hardly had any better knowledge, for the Ionic order of a Macedonian tomb at Vergina copies faithfully the barbaric details of the Philippeion.¹⁹ The Ionic capitals from Pella, to judge from the photograph in *BCH* lxxxiii (1959) 704 fig. 21, are not in accordance with strict Ionic design, the volutes being linked in a rising 'hump-back' line, which may suggest contacts with mainland Greece—for instance, the internal Ionic capitals of the temple at Bassae.

The Doric, however, is excellent in quality. The proportions are quite in accordance with those of Greece proper, the details, such as the echini of the capitals, being competently designed and worked by contemporary standards. In view of this, the natural position of Macedon, and her relations with Athens, it can safely be assumed that the Doric order had real importance in Macedon.

Following the conquests of Alexander, Macedonian control and influence was spread over a far greater area. It is quite impossible to discern within this extensive area any coherent architectural policy imposed by Alexander. In the Ionian cities, construction of Ionic temples took place. Alexander himself dedicated Pytheos' temple of Athena Polias at Priene. We may, I think, assume that in such circumstances Alexander's policy, if he ever gave it a thought, was to leave well alone. The real difficulty concerns the new Greek and Macedonian colonies, which grew to such importance, Alexandria and Antioch, for instance, and the policies of Alexander and the successor kings in building outside those areas where there was an established Greek architectural tradition. The archaeological evidence is slight in the extreme, but at the very least it does show that the Doric order was not unknown, and that it was used for temples. There are remains of Doric temples from Hermopolis in Egypt and from Seleucia, the harbour town of Antioch.²⁰ We cannot be sure how far this went, but taking into consideration the Macedonian character of many of these foundations it is likely that major 'official' temples sponsored by the Hellenistic kings would be Doric in style, at any rate during the first century of the Hellenistic era. This cannot be asserted as a definite, inflexible rule, but we can be certain that there was some royal prestige attached to the Doric order. It is quite possible that Pytheos lived long enough to protest against this.

An outstanding example of such a 'prestige' temple is that built by Lysimachus at Ilion, and dedicated to Athena.²¹ In this region, a Doric temple is not completely an

¹⁸ Levkadhia: *Archaeological Supplement to JHS* lxxv (1955) 15.

¹⁹ Vergina: Rhomaïos, 'Ο Μακεδονικός Τάφος τῆς Βεργίνης; Lawrence, *Greek Architecture* pl. 103.

²⁰ Hermopolis: Dinsmoor, *AAG* 268; Seleucia: Antioch on the Orontes iii 33.

²¹ Zschietzschmann, *Ber. VI Kong. Arch.* 426; Schleif, *AA* 1935 314. Now published by Goethert

intruder, since there had been a Doric temple at Assos, not many miles to the south, since the archaic period. The same is true of the next Doric temple in Asia Minor, that of Athena Polias at Pergamum,²² which is presumably the work of the independent dynasts of that city, who had usurped for themselves the position (and eventually the title) of kings, and who in many ways sought to stress their (assumed?) Macedonian ancestry by aping their Macedonian 'betters'. As a result of developments in the third century B.C., the Ionian cities of Asia Minor, though usually independent, at least nominally, were actually at the mercy of the political ambitions and activities of three groups—the Seleucids, the Ptolemies and the Attalids. From all these there was likely to be pressure (misguided certainly, and unthinking, rather than a matter of deliberate policy) to build in the Doric style, particularly when the building was put up as a gift of one or other of the kings involved; elsewhere we have a notorious instance of this lack of concern for local traditions in the intrusion of the Pergamene stoa of Attalos II into the purer architecture of the Athenian Agora. It is surely against this form of intrusion that Hermogenes is protesting. The danger was real, as we now know from the Doric temple of Apollo at Klaros, and we also have the awful portent of the supercession of an earlier Ionic temple by a Doric one at the Asklepieion of Kos.

We can, I feel, be more precise on the specific instance of Hermogenes' protest. In 189 B.C., following the treaty of Apamea, Eumenes II gained control of a large part of Asia Minor, including the Ionian city of Teos. For reasons of policy, the Pergamene kings showed great favour to the guild of actors, the *τεχνῖται Διονύσου* who had made Teos their headquarters.²³ It is more than likely that Pergamene favour found its usual expression in the gift of a building, and that Hermogenes' temple was erected under Pergamene auspices.²⁴ I would suggest that it was this temple for which a quantity of marble had been collected, with the intention of building it in the Doric style, the normal Pergamene order, and which Hermogenes *commutavit ex eadem copia et eam Ionicam Libero patri fecit*. The reason for rejecting this identification is that the temple at Teos shows no sign of such alteration, while there is a small Ionic temple at Pergamum itself which has clearly been constructed out of re-used Doric parts, and whose Ionic mouldings are akin to those of Hermogenes' buildings at Magnesia. This, however, is a very unimportant building; the mouldings need only be the result of Hermogenean influence after the major incident concerning the temple at Teos. Nor is there any reason why the temple at Teos should show any physical signs of the change of design. The material would have been quarried and cut approximately to size, roughed out, perhaps, but not completely worked, since this was usually done on the site, in the course of construction. At this juncture Hermogenes, whether as the original architect of the temple or not, made his protest. Fearful lest the construction of an unwanted Doric temple should in some way offend these highly favoured subjects, the Pergamene king decided to entrust the whole work to Hermogenes himself to complete in the Ionic order. It would not be unduly difficult, provided construction had not yet begun (and this seems implied in Vitruvius' story) to alter the design to Ionic without any actual trace of the alteration appearing in the finished temple. Following this, the influence of Ionic spread to Pergamum, in the little rebuilt temple, and in the major post-Hermogenean temple of Apollo Chrestorios at Aigai.

In the end, Hermogenes' alarm was unnecessary, since both Doric and Ionic succumbed and Schleif, *Der Athenatempel von Ilion*. Goethert, following Dörpfeld, prefers to date this temple to the time of Augustus. For reasons which I have stated in my review of this book (see below) I am not convinced by his arguments.

²² *Altertümer von Pergamon* ii.

²³ References conveniently collected by Esther V. Hansen, *The Attalids of Pergamum* 417.

²⁴ This is not affected by the controversy over Hermogenes' date. Whether he belongs to the first half of the second century (Dinsmoor) or the second half (Robertson, Lawrence) his floruit must fall within the period of Pergamene independence.

to Corinthian, developed particularly by the Hellenistic kings at the expense of the earlier traditions and enthusiastically adopted from them by the Romans. But the old traditions died hard. Even though Doric cannot show anything as magnificent as the late Ionic buildings of Aphrodisias, the reason is once more the relative poverty of the Doric areas. Against Aphrodisias we can place only the humble temples of Kourno, but even in their differing circumstances, they show just as clearly the loyalty of the Greeks to the styles they knew best.

APPENDIX

Mixed Orders in the fourth century B.C.

The mixture of Doric and Ionic can take two forms:

(a) The use of both Ionic and Doric columns, and perhaps entablature, in one and the same building.

(b) The combination of Ionic and Doric elements in a single order.

The first is quite common. Examples are:

(1) The hypothetical Ionic columns in the west chamber of the Parthenon.

(2) The Ionic columns flanking the central passage of the Athenian Propylaea.

(3) In stoas, the use of an Ionic colonnade of greater height supporting the ridge beam behind an external Doric colonnade. The earliest example of this is perhaps the stoa of Zeus at Athens, of the second half of the fifth century B.C.

In all these the motive is severely practical; to achieve greater column height in special circumstances without increasing unduly the lower diameter of the column.

The second form occurs to a negligible extent in fifth-century Attic architecture (e.g. the use of Ionic friezes and mouldings in the Parthenon and the Theseum) where to all intents and purposes the order remains Doric, and in one or two subordinate buildings of the fourth-century Carian dynasts at Labranda, where we find the full combination of Ionic columns and Doric entablatures.²⁵

Jeppesen, however, considers that during the fourth century B.C. Athenian architects strove to achieve in the interests of symmetry a combination of major features from Doric and Ionic in single buildings. He bases his theories on examples of each form.

His example of the first form is the proposed porch for the Telesterion at Eleusis, for which the only evidence is the inscription IG ii² 1666. According to Jeppesen's revolutionary study of the stones, dimensions and quantities specified in this inscription, an Ionic porch was to be added to a building already decorated with a triglyph frieze. If this was in fact so, it would seem to me that the reason was the size of the existing telesterion, which would require ponderously large columns for a porch if it were to be Doric. Even if this porch, as seems likely, was abandoned as a result of Athens' financial difficulties in the late 350's, rather than the architectural oddities involved, it is significant that when the porch finally was constructed, the Doric order was employed. A comparison of Jeppesen's restoration of the proposed Ionic porch with Philo's later Doric porch suggests considerable aesthetic advantages for the alleged earlier proposal.²⁶ This, however, is entirely the result of special circumstances, and it would be folly to base general aesthetic principles on the evidence of these porches.

²⁵ Professor Martin Robertson reminds me also of a fourth-century example of Ionic columns supporting an entablature which includes Doric triglyphs over an Ionic architrave (Robertson, *Greek Painting* 162, 164). This is a Tarentine vase painting. To judge from the spindly character of the columns, it

represents a temporary wooden structure (presumably stage scenery) rather than actual architecture.

²⁶ Compare Jeppesen, fig. 80, with, e.g., Dinsmoor AAG³ pl. 66.

The example of the second form of a mixed order is Philo's design for the Arsenal at the Piraeus. Again, though this building was of course completed, we are entirely dependent on an inscription—IG ii² 1668—for our knowledge of it. Jeppesen seeks to show that the dimensions of the triglyphs (not given by the inscription) must have been such that there were fifteen triglyphs across the façade. This, he argues, presupposes a design based on the arrangements of octostyle temples. Further, the given dimensions of the façade are such that, if an octostyle arrangement underlies the design, the proportions of the 'columns' are such that they must have been Ionic. It is not, of course, suggested that actual columns were ever intended to be used for the façade. This argument depends on the number of triglyphs used for the façade; this in turn is based on an impressive series of mathematical calculations which are, I feel, unreliable in that they have to take into account too many unknown quantities, in particular the exact division of the frieze 'unit' between triglyph and metope (since the total length of the frieze consists of a certain number of units plus one triglyph) and the possibility of inclination of the walls.

Jeppesen, perhaps, accepts too readily that the number of triglyphs must have been fifteen, rejecting other possible figures and simply stating (page 88): 'The number of front triglyphs proposed by Marstrand and Dörpfeld (21 and 11 respectively) seems to be on the extreme: the size of Dörpfeld's triglyphs looks exaggerated, while Marstrand's appear to be too small. . . .' Marstrand's figure, I agree, is unacceptable, and would make the triglyphs far too small. Dörpfeld's number on the other hand, is much more reasonable. Given a frieze length of approximately 55 feet, the unit length for 11 triglyphs (i.e. 10 units + 1 triglyph) should be about 5 feet or a little more. On the Vitruvian proportions followed by Jeppesen, this gives a metope width of just over 3 feet, and a triglyph width just over 2 feet. These figures are not precise, because of the uncertainties mentioned above, but for our immediate purpose they will do. These dimensions, again following the Vitruvian proportions, give a frieze height equal to the metope width of just over 3 feet. This can be related to the known wall height of 27 feet; i.e. the proportions of frieze height to total wall height is 1 to 9. This compares admirably with the known proportion of other buildings. In the treasury of the Athenians at Delphi, according to the French publication, the frieze height is 0.674 m., the total wall height 5.499 m., proportions of about 1 : 8.2. In the Theseum, the total height of column, architrave and frieze is 7.365 m., frieze height alone 0.828 m., proportions of 1 : 8.9. In other words, a frieze of 11 triglyphs across the front of the arsenal, far from seeming exaggerated in size, is quite in accordance with normal Doric proportions. If we must relate this design to columnar arrangements, these should therefore be hexastyle, not octostyle. We may calculate further: a frieze height of about 3 feet would give an architrave height of about the same. Subtracted from the total wall height, this leaves 21 feet for the 'columns'. Six columns means five axial spaces of approximately 10 feet (not allowing for corner contraction, partly balanced by the additional column diameter). The Theseum proportions give axial spacing: column diameter as 2.55 : 1. Applied to the Arsenal, this gives us a 'theoretical column' diameter of about 3.9 feet, and proportions of column height to column diameter of approximately 5.5 : 1, proportions which cannot be other than Doric.

In other words, the proportions of the façade of Philo's arsenal can be explained entirely in terms of Doric, and they provide no evidence whatsoever for the crude combination of Doric and Ionic proportions by Attic architects of the fourth century B.C.

In theory, the various parts of the two orders should not be mutually exclusive. Both include essentially corresponding parts: platform; column shaft, with or without base; capital; architrave; frieze; cornice. In the Doric order the raking cornice over the pediments was similar in form to the cornice of the Ionic order, and it is quite likely that (in spite of Jeppesen's arguments) the *γείσα Δωρικὰ* and *γείσα Ἰωνικὰ* of IG ii² 1666 refer simply to the horizontal and raking cornices of a Doric pediment. It may have been uncertainty

of this sort (which may well have existed before the different orders were given their unfortunate 'racial' epithets) that enabled Attic architects of the fifth century B.C. to use what would seem to be other Ionic elements within the Doric order where special circumstances demanded—e.g. the need for a continuous frieze to depict satisfactorily the Panathenaic procession on the Parthenon, whether or not this has any antecedents in Peisistratid architecture. Elsewhere the distinctions were more inflexible, and everywhere the combination of Doric entablature over Ionic columns was regarded as a barbarism, as I am sure it must be at Labranda. On the other hand, the use of separate Doric and Ionic colonnades in the same building was quite acceptable.

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A TEUCRIAN AT SALAMIS IN CYPRUS

It is well known that in his eighth year, 1162 B.C.,¹ Ramesses III repulsed onslaughts by the Peoples of the Sea or Sea Raiders as they are often called. The migrating horde was composed essentially of a group of closely related tribes, the Philistines, Tjekker and Dene.² They were dressed identically alike in the Sea Raider kilt which was divided into panels and fell to a point in front, and they wore a headdress of feathers, as it is supposed to be, FIG. 1.³ This, it will be noted, is the very dress of the follower of the chariot on the ivory gaming box from Enkomi, FIG. 2.⁴ The panelled kilt falling to a point in front is there, as is the headdress on which even the row of circles or rings so often decorating the head band is shown, as well as the neck guard fitting to the back of the head. Another detail on the Enkomi box allies the scene to the Philistines and therefore to their relatives the Tjekker. It is that like those of the Philistines⁵ the charioteer leans forward over his horses. This is a very rare attitude to be adopted, though significantly enough it can be found occasionally among the Hittites at the Battle of Kadesh.⁶ It is also to be seen on one of the Megiddo ivories of a date not later than c. 1150 B.C.⁷ It does not occur in the Mycenaean paintings.⁸ The point to which the kilt falls is not quite like that of the Sea Peoples for it is broader. On the other hand, the Egyptian paintings show it on a man coming from 'the ends of Asia',⁹ that is to say the extremest north of Syria. Quite other evidence proves it to be a north Syrian or perhaps Hurrian detail, for it is shown much later on the Loftus ivories from Nimrud which are under this influence.¹⁰ Besides this, one of these ivories shows a kilt which is just that of the Sea Raiders, but without the tassels.¹¹

Now a word about FIG. 1. It is the third man in the row of prisoners who Ramesses says are 'the fallen great ones of Tjekker'. There are only five men shown, and as the

¹ The date is that originally fixed by Rowton (*JEA* xxxiv (1948) 72), and has been used in all my various studies of the Sea Raiders (for these see note 33, p. 149, to which should be added 'The Teresh, the Etruscans and Asia Minor', in *Anatolian Studies* ix (1959) 197-213); 'Some Sea-Peoples' in *JEA* xlvii (1961) 71-90. Hence, to avoid confusion it is still used here. It is based on the date of the accession of Ramesses II, for which only two dates are astronomically possible, 1290 or 1304 B.C., of which two Rowton originally decided for the lower (*op. cit.*, 69, 72). Since then, however, he has seen reason to prefer the earlier (*JNES* xix (1960) 15-22), and this of course would put Ramesses III back by 14 years, i.e. his eighth year would fall in 1176 B.C.

² These latter two peoples figure under a variety of versions of their names, such as Tjekker, Tchakaray, Zakkar, Zakkal, Thekel; Dene, Denyen, Dainiuna, Danuna. The first of these names is spelt in this article with the duplicated *k* when reference is made to the Medinet Habu sculptures which use this form. Everywhere else it is spelt with a single *k* as in the relative documents.

³ H. H. Nelson and others, *Medinet Habu* pl. 43, top row of prisoners whence our FIG. 1 is taken. They are stated to be Tjekker. For the others of the group see pl. 44 where they are named Philistines in the bottom row and Dene in the middle row of

prisoners. For a full discussion of the Philistine group see Wainwright in *JEA* xlvii (1961) 74-82.

⁴ A. S. Murray, Smith and Walters, *Excavations in Cyprus* pl. 1, top right, from which our FIG. 2 is taken.

⁵ *Med. Habu* pl. 34 right hand, two charioteers.

⁶ Breasted, *The Battle of Kadesh* has collected the various scenes and gives references to the large scale reproductions.

⁷ Loud, *The Megiddo Ivories* pl. 32, no. 159 and p. 17. For the date see p. 10.

⁸ Furumark in *Opuscula Atheniensi* i (1953) 61. It occurs again in Cyprus, this time on a very crude scarab engraved in an oriental manner (fig. 1, p. 48) and dating to the very end of Late Cypriote II C, c. 1300-1230 B.C. (pp. 52, 65).

⁹ N. de G. Davies, *The Tomb of Puyemré at Thebes* pl. 31, top register, 3rd man. He wears a medallion. The other two men are ordinary long-robed Syrians.

¹⁰ Barnett, *The Nimrud Ivories* pls. 18 S 1; 22 S 2; 26 S 20, and p. 42. These ivories date from the late ninth to the late eighth centuries B.C., p. 52.

¹¹ *Id.*, *op. cit.*, pl. 22 S 4. Tassels were worn in that part of the world, for they are shown on a Hittite sculpture from Cağın not far from Tell Bashar and Ain Tab, *Archäologischer Anzeiger*, 1940, col. 566 and fig. 4, and map, cols. 557, 558 = Bossert, *Altanatolien* fig. 567 said to be from Ahçaköy.

heads and faces are all damaged it has been necessary to pick the least unsatisfactory figure. Though the neck guard is hidden by the man's arm, it appears quite clearly on the second, fourth and fifth men. The head band of FIG. 1 does not show the more usual circles of the Salaminian and the Philistine confederates, but the less common zigzags.¹² The fourth and fifth men who are shown full face are wearing the curious laminated armour of the Sea Peoples. Hence, the marks on the forefront of FIG. 1, though strangely enough they do not enclose the body, represent armour and not ribs as might be supposed. Thus, the correspondences are striking between the little man from Salamis and the Tjekker of Ramesses III's sculptures.

However, not unnaturally, there are slight differences between the two. Thus, the little Cypriot carries an axe instead of the regular Philistine pair of spears. This is no



FIG. 1.



FIG. 2.



FIG. 3.

doubt because he is out hunting, not at war. Also he does not wear the tassels on his kilt so beloved of the Sea Peoples. Again, he is bearded which the Tjekker tribesmen are not as a general rule, though their chief and two of his tribesmen do wear this adornment.¹³

The little Cypriot is, therefore, one of the Peoples of the Sea, and of them the evidence goes to show that he is a Tjekker, or a Tjeker as the name is otherwise spelt.

Our little Tjeker proves to have been not the only Sea Raider at Enkomi, for another ivory carving from there shows a hero plunging his sword into the heart of a griffin, FIG. 3.¹⁴ This man is clean shaven as is usual with these peoples. He wears a patterned kilt which falls to a slight point in front considerably different from that of the little Tjeker but more comparable to those of the Sea Raiders. He also wears their characteristic laminated armour fitted to the shape of the ribs, and carries their round shield as well. He is armed not with the usual Philistine pair of spears but with a sword, as indeed the Philistines themselves sometimes are.¹⁵ His kilt is not tasselled, and he does not seem to wear the feather headdress, though what he is wearing is not clear. At any rate it has the regular head band of the Philistine group decorated with the usual rings or circles.

These Enkomi ivory carvings belong to a date about the age of Ramesses III¹⁶ or to a date round about 1200 B.C. The griffin slaying hero would be contemporary with the

¹² See *Med. Habu* pls. 34, 39 showing the Land and Sea Battles.

¹³ Wreszinski, *Atlas* ii pl. 160 a; *Med. Habu* pl. 98 bottom row last man = pl. 125 c. The man on the right in pl. 118 c also wears a beard, but though he is labelled Peleset (Philistine), he looks more like a Tjeker.

¹⁴ Murray and others, *op. cit.*, pl. 2. The design remained popular in Cyprus and at Nimrud lasted

on to the time of the Loftus ivories of the eighth century, Barnett, *op. cit.*, pls. 22 S 2, 4; 26 S 20, and p. 67.

¹⁵ *Med. Habu* pls. 34, 39.

¹⁶ For instance, du Plat Taylor in *Palestine Exploration Quarterly* 1956 35. An elaborate discussion by Miss Kantor puts the pieces to the Late Cypriot IIIa Period and to a date of c. 1200-1150 B.C. (*AJA* li (1947) 93 f., and cf. *JNES* xv (1956) 171).

little Tjeker, for Frankfort puts this carving at the beginning of the twelfth century.¹⁷ Hence, it is clear that there were Tjeker and others of the Sea Raider horde at Enkomi in Ramesses III's time.

Enkomi is Salamis. Hence, the inclusion of a name Salameski in the list of Ramesses III¹⁸ is important and in accord with the presence of the little Tjeker at Salamis. This list has often been published,¹⁹ and in it there are five names which interest us here, nos. 7-11. If it be permissible to read them syllabically instead of as merely cut down to the bald consonants, they are *sarameski* (salameski), *kathiin*, *lymar*, *sari* (*sali*), *itilu* (*itil*). These names Brugsch has very reasonably recognised as Salamis, Kition, Marion, Soli and Idalion,²⁰ and the mere collocation of so many possible Cypriot names and at this very time justifies the conclusion. It is only this once that they appear in the Egyptian records.

Now a word about the form Salameski. It has been plausibly suggested that the *-ki* at the end of the word is the cuneiform *-ki* determinative of a country, hence that the list would have been taken from a cuneiform one. This would be quite probable, as cuneiform was the international means of communication. The scribe seems to have noticed his error, if such it be, and has not repeated it in the names that follow.

Thus Salamis was known to Ramesses III and the Tjeker who attacked him had reached that place also. Various suggestions have been made as to the identity of the Tjeker, the most common and obvious being that they were Teukroi,²¹ and the following information leaves little doubt but that this is correct. It is, therefore, important that just as the Tjeker invaded Salamis so did the Teukroi. Legend said that Teukros founded the city and the temple to Zeus and that the kings were priests of this temple in their capacity of descendants of his.²²

We get the same state of affairs back in western Cilicia where we find the Teukroi in possession. Thus, Strabo says (xiv 5.10) that Olba was founded by Ajax son of Teucer, that most of the priest kings bore the name Teucer or Ajax, and that the country was called the Domain of Teucer. Gjerstad has pointed out that this tradition is independent of the Homeric one.²³ Indeed, Frazer pointed out long ago that the name Teucer in Cilicia is probably simply an adaptation from that of the native god Tarku,²⁴ as did Sir William Ramsay later,²⁵ and just recently Windekens has argued for a Pelasgic origin for the name.²⁶ Gjerstad also takes Teucer to have been an Anatolian though from the Troad.²⁷ Others have shown that his 'half-brother' Ajax belonged originally to the sixteenth century,²⁸ which no doubt would carry Teucer with him.

Teukroi, therefore, were strongly entrenched in western Cilicia from early days, that is evidently from the sixteenth century, and though we have no certain knowledge of Tjeker being there at that time, we have plenty of evidence for the presence of their close relatives

¹⁷ Frankfort, *The Art and Architecture of the Ancient Orient* 154.

¹⁸ One should not try to deduce therefrom that Ramesses claims to have invaded Cyprus. All that it means is that he defeated enemies coming from there.

¹⁹ For instance, W. Max Müller, *Egyptological Researches* i pl. 64; J. Simons, *Egyptian Topographical Lists* 165; Jirku, *Die ägyptischen Listen palästinensischer und syrischer Ortsnamen* 44 (*Klio*, Beiheft xxxviii, 1937). The names are recorded by Max Burchardt, *Die altkanaanäischen Fremdwörter und Eigennamen im Ägyptischen* passim.

²⁰ Brugsch, *A History of Egypt* ii (1879) 152. No one else has ventured to identify them.

²¹ For the various views see H. Gauthier, *Dictionnaire des noms géographiques* vi 69 f.; A. H. Gardiner, *Ancient Egyptian Onomastica* i no. 269, pp. 199* ff.

²² Gjerstad, 'The Colonization of Cyprus in Greek Legend' in *Opuscula Archaeologica* iii (1944) 108 f.

²³ *Loc. cit.*, 117.

²⁴ Adonis, *Attis, Osiris* (1907) 112, 115. In the Corycian Cave near Olba Tarku enters into the names of many priests of Zeus.

²⁵ *JHS* xxxviii (1918) 131, 149.

²⁶ Windekens in *Minoica* 448-50 (*Festschrift J. Sundwall*).

²⁷ *Loc. cit.* See the Appendix to this article for some connexions between the Troad, the Teukroi, the Philistines and Cilicia.

²⁸ Denys L. Page, *History and the Homeric Iliad* 234 f.; T. B. L. Webster, *From Mycenae to Homer* 61, 101, 115, 130.

the Philistines in western Cilicia, and for the presence of their other close relatives, the Dene, in eastern Cilicia. It is thus most probable that the Tjeker also would have been living in western Cilicia in early days, and that, as will be seen, means the sixteenth century.

Anyhow, Teukroi and Tjeker invaded Salamis, and much archaeological evidence points to an invasion of Cyprus from Anatolia, and the presence of foreigners is attested by a group of skulls from one tomb which differs from the others.²⁹ Further, Gjerstad has shown that there were two invasions of Cyprus about this critical time. The first took place in Late Cypriot III A at the beginning of the twelfth century, in other words at the time of Ramesses III, and that this was the one to which the Anatolian Teukros belonged.³⁰

The similarity of the histories of the Tjeker and the Teukroi makes it practically certain that the two were one and the same people.

The sixteenth century has just been mentioned as the time of Teucer himself and the time at which we get our first glimpse of the Tjeker's near relatives, both the Philistines and the Dene-Danuna. The Tjeker are indistinguishable in dress from both of them, and their history so far as it is known is closely similar to that of the Philistines.³¹

As for the Philistines. It is not later than 1500 B.C. that an Egyptian writing board gives a list of names of Keftiu (Caphtor), and among them are included ʔkš (M), ʔkš.t (F).³² These names are otherwise known to us as Achish ('akiš Hebrew, 'Αχχίς Septuagint), the Philistine king of Gath and Ziklag with whom David and others sought refuge (*I Samuel* xxvii ff., *I Kings* ii 39, 40). There is much evidence that Keftiu-Caphtor, whence came the Philistines, was the country of the Calycadnus River in western Cilicia.³³ It is here that the name Ακκισις has been recorded near Palaia Isaura just beyond the headwaters of that river. It is a woman's name just as the Egyptian writing board gives not only the masculine but also the feminine form.³⁴ It is also noteworthy that it was at the beginning of this very century, about 1600 B.C., that that unique object, the Phaistos Disc, shows the well-known Philistine-like heads and the round shields.³⁵

Again, the sixteenth century is the very time that we get our first glimpse of the Tjeker's other relatives, the Dene-Danuna. Their name is to be found in that of Adana, the well-

²⁹ Sjöqvist, *Problems of the Late Cypriote Bronze Age* 208.

³⁰ Gjerstad, *loc. cit.*, 123 and cf. 87. It was the second one that came from Greece in the Late Cypriote III B Period about 1100 B.C. Furumark accepts the immigration theory although he disputes some of the details, *Opuscula Archaeologica* iii (1944) 264. Schaeffer, *Enkomi-Alasia* 369 has much to say about various invasions at this time. He ascribes an early one, about 1300 B.C., to Achaeans, another, about 1225-1200 B.C., to the Mycenaean, and that of the Sea Peoples to between 1200-1150 B.C. He does not deal with an invasion from Anatolia.

³¹ The Tjeker accompanied the Philistines to Syria where later on the Prince of Byblos was a certain Tjeker-Baal and further south Dor is said to have been a Tjeker town (Breasted, *Ancient Records of Egypt* iv §§ 565, 567). Further south again they left their name in that of Ziklag the town belonging to the Philistine Achish, and they both fell upon Ramesses III.

³² Peet in *Essays in Aegean Archaeology presented to Sir Arthur Evans* 90 ff. For the date see pp. 98, 99.

³³ Wainwright, 'Caphtor-Cappadocia' in *Vetus Testamentum* vi (1956) 199-210; 'Some Early Philistine History' in *ibid.*, ix (1959) 73-84, and now once

more in *JEA* xlvii (1961) 77-81 and map. For companion studies of Keftiu see 'Asiatic Keftiu' in *AJA* lvi (1952) 196-212; 'Keftiu and Karamania (Asia Minor)' in *Anatolian Studies* iv (1954) 33-48. The idea that Caphtor must have been Crete was originally based on nothing more than that in the translations of the Bible that land is called 'the isle of Caphtor' and Crete is a large and suitable island. However, while the Hebrew word can mean 'isle', its primary meaning is only 'coastland'. Miss Kantor herself has shown a number of difficulties in the easy acceptance of Crete as Keftiu in *AJA* li (1947) for instance on pp. 48, 49, 74, 102. A number of fallacies in that idea is exposed in my article 'Keftiu: Crete or Cilicia?' in *JHS* li (1931) 1-38.

³⁴ Sterrett, *The Wolfe Expedition to Asia Minor* no. 165 (vol. 3 of the Papers of the American School of Classical Studies at Athens).

³⁵ Sir Arthur Evans, *The Palace of Minos* i fig. 483, nos. 2, 12, and for the date see pp. 647, 667. Recently efforts have been made to prove that the Disc is of Cretan origin, but they will hardly stand up against all Sir Arthur's evidence that it would be an import—presumably from southern Asia Minor.

known city of eastern Cilicia.³⁶ Under the form Adaniya it is named by Telepinus whose reign is now dated at 1525-1500 B.C.³⁷ Hence, the Dene-Danuna-Dananiyim were evidently already in eastern Cilicia when the Philistines were in western Cilicia, and that is the sixteenth century³⁸ the time to which Teucer himself goes back.

Coming to the Tjekers themselves it is just at that time that an isolated name appears which may be theirs in view of what we know of their relatives. It occurs in a list which also includes several belonging to north Syria, and is Tjeker. It is spelt exactly the same as that of the later Tjeker with only one *k* instead of the duplicated one which the Medinet Habu texts use. It is Tuthmosis III who lists it,³⁹ and as he reigned 1504-1450 B.C. it is of the same period as Teucer himself, as the Philistines and as the Dene-Danuna at Adana. Hence, it seems reasonable to take it as the same name.⁴⁰ If this be accepted, we must suppose that certain Tjeker had pushed through and into northern Syria at that time.

There is, thus, much evidence to suggest that the Teukroi and also the Tjeker would have been in western Cilicia a long time before the Sea Peoples' attack on Ramesses III. Hence, they would have been well placed to invade Cyprus at that time, and our little man at Salamis provides good evidence that the Tjeker at any rate availed themselves of the opportunity. He also shows two other things: first that the Tjeker really were the Teukroi, and secondly what the Teukroi looked like.

As is natural at such a place as Salamis we continually meet Achaeans in the pages of the various authors who have studied this very involved period of Cypriot history. But Sjöqvist says that the invaders of about 1200 B.C. 'can certainly not have been pure Achaeans', and Gjerstad notes that 'the invaders were Anatolians headed by Achaean leaders'.⁴¹

The ivory box from Enkomi-Salamis illustrates this state of affairs, for it shows us two types, the retainer and his lord. We have already seen much evidence that the retainer was a Tjeker, an Anatolian with his feather headdress, and no doubt from western Cilicia. But what about his lord whose chariot he follows? He is of a very different type, and instead of the retainer's feather headdress he wears his hair fairly long, combed back from the forehead, and over the top of the head. Does this qualify him as one of the long-haired Achaeans? But whether or no we very clearly see here that the leaders were of a different type from their Anatolian followers.⁴²

In the foregoing pages our little man has appeared as a Tjeker-Teucrian. But there is another similar possibility which must be mentioned before leaving him, and that is that

³⁶ In the late eighth-century bilingual inscription of Karatepe the Phoenician Dananiyim corresponds to the Adana of the Hittite version. See for instance Mellink in *Bibliotheca Orientalis* vii (1950) 146. For the date see p. 147. For all this and more see *JEA* xlvii (1961) 81. There the views of several scholars are quoted that the isles whence Ramesses III says the Dainiuna came were not those of the Aegean but the little ones off the Cilician coast. Since those views were expressed Laroche has also drawn attention to these Cilician islands and given further information, *Syria* xxxv (1958) 274.

³⁷ A. Goetze, *Kizzuwatna* p. 57. For the date see O. Gurney, *The Hittites* (2nd edn) 216.

³⁸ In a long study of Greek traditions and Egyptian history as we have it in the confused excerpts from Manetho and real Near Eastern history Bérard concludes that Danaos would have arrived in Greece say about 1550 B.C., *Syria* xxix (1952) 41.

³⁹ The list has often been published, for example,

W. Max Müller, *op. cit.*, i pl. 45, no. 47; Max Burchardt, *op. cit.*, no. 1170; J. Simons, *op. cit.*, 113, no. 136 (47); Jirku, *op. cit.*, 19, no. 47. H. Gauthier, *op. cit.*, vi 70 considers the identification of the two names as plausible, whereas A. H. Gardiner, *op. cit.*, i 200* is doubtful. Jirku would like to accept the identity of the two.

⁴⁰ It has only been doubted because it is a single occurrence and at a time so much earlier than that of Ramesses III. Unfortunately the list is not arranged in order and many of the names are destroyed, but it includes such well-known places in north Syria as Tunip and Niya. Of course Tuthmosis campaigned up there in the farthest north.

⁴¹ Sjöqvist, *Problems of the Late Cypriote Bronze Age* 208, 209; Gjerstad in *Opuscula Archaeologica* iii 87 and similarly p. 119.

⁴² Barnett would take them both to be Hurrians, *PEQ* 1939 11.

he is a Dene-Danuna. That is what Bossert takes him to be,⁴³ basing his view on the later name of Cyprus, Iadnana 'Isles of the Danana'. However, the many connexions of Salamis, whence comes our little man, with Teucer puts this out of court.

APPENDIX

Though at present it is impossible to draw firm conclusions about the very obscure subject of the connexions between the Troad and the Tjeker-Teukroi, Philistines and Cilicia, it is as well to put together the following scraps of information.

In the first place Gjerstad has already indicated that the Teucrians who invaded Cyprus came originally from the Troad⁴⁴ and had arrived at much the same conclusions about them as I have about their relatives the Philistines.

Then, Hamaxitos in the southern Troad was a settlement of the Teukroi and more than that there were mice there which were sacred to Apollo Smintheus, the god who sent disease (Strabo xiii 1.48). This introduces those close relatives of the Tjeker-Teukroi, the Philistines, who when afflicted with emerods sent away not only golden images of them but also of mice (*I Samuel* vi 4)—a connexion between mice and disease reminiscent of Hamaxitos.

The Philistines provide still more information pointing in the same direction. Thus, the name of their king of Gath and Ziklag was Achish or 'Αγχιός, as the Septuagint has it. This finds its counterpart in that of 'Αγχιόης father of Aeneas.⁴⁵ Again, the very name, Philistine, has long been thought, and again very recently shown likely to be, the same word as Pelasgoi.⁴⁶ If this should really be so, it brings us back once more to the southern Troad and the neighbourhood of Hamaxitos, where was Larissa whence came those Pelasgoi who were allies of the Trojans (*Il.* ii 840 f.). Also near Hamaxitos was Antandros another city said to be Pelasgian (*Hdt.* vii 42).

We get other and similar connexions between the Troad and Cilicia, the western part of which, as I trust I have shown satisfactorily, was occupied by the Tjeker-Teukroi and the Philistines. Though the better known Cilices are those of Cilicia in the south-east there were others in the southern Troad. They inhabited the city of Thebe (*Il.* vi 395, 415) and that was not far from Hamaxitos and Antandros, and as Strabo says (v 2.4) they bordered on the Pelasgoi. Some of them were driven south-eastwards to Pamphylia the neighbouring country to western Cilicia, and there they inhabited a town which they called Thebe (*Strabo* xiv 4.1).

Anchises was a Dardanian (*Il.* xx 219 f.) and the Dardanians were an Illyrian tribe (*Strabo* vii 5.6), as were the Philistines.⁴⁷ Certain Dardeny had appeared as allies of the Hittites at the Battle of Kadesh, 1285 B.C.,⁴⁸ and Ramesses II shows them in a mixed group of prisoners from Pidasia, Kirakisha, Masa and Luka.⁴⁹ These are the peoples well known in the Hittite records as Pitassa, Karkisa, Masa and Lukka. Unfortunately it is not possible to sort out the Dardeny from the other prisoners.

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⁴³ *Mitt. altorientalischen Gesellschaft* iv (1928-29) 280 f.

⁴⁴ Gjerstad, *op. cit.*, 117.

⁴⁵ For example, Reinach in *Rev. arch.* xv (1910) 41; A. R. Burn, *Minoans, Philistines and Greeks*, 162; Bonfante in *AJA* l (1946) 254.

⁴⁶ Bérard in *Rev. arch.* xxxvii (1951) 132.

⁴⁷ Kretschmer in *Glotta* xxx (1943) 152-4; Bonfante, *loc. cit.*, 251-62.

⁴⁸ Breasted, *Ancient Records of Egypt* iii §§ 306, 349.

⁴⁹ Kuentz, *La bataille de Qadech* pl. 25 lowest row of prisoners brought by the king continued in the right-hand bottom register, and p. 49 (3).

NOTES

The SYN Coins Again

In *JHS* lxxi (1961) 67 f. Professor J. M. Cook put forward the engaging theory that the SYN coins represent a pro-Spartan alliance formed in 391 and 390 during the resurgence of Spartan power in the Aegean. Without seeking to retrace the argument of my article in the *Numismatic Chronicle* xvi (1956),¹ I wish to comment on certain points of this new theory.

First, the Theban coins—a difficulty which Cook formulates as follows. 'The device on the obverse of the SYN coins is of course a well-known Theban one; and though the Spartans, having no coinage of their own, could not contribute a Spartan coin-type, it may be objected that they would hardly have used a well-known Boeotian emblem if they were at war with Thebes at the time.' Cook's solution is to propose that 'it is quite possible that in 391 (assuming that to be the year of Thibron's departure for Asia) the Spartans may have considered that a treaty of alliance existed between themselves and the Thebans'. In support of this he adduces the passage in Andocides *de Pace* (20) which speaks of the Boeotians coming to terms with Sparta—*Βοιωτοὶ δ' αὖ πῶς τὴν εἰρήνην ποιοῦνται*; — — — *Ὁρχομένον ἀφιέντες αὐτόνομον τὴν εἰρήνην ποιοῦνται* — — — οὗτοι δ' αὖ τούτῳ <τῷ> τρόπῳ τὸν πόλεμον καταλείπονται. If this were the only evidence,² we would be free to conclude that the Boeotians probably did make peace: certainly Andocides envisages the possibility that Athens, Corinth and Argos might fight on without them.³ But it is not the only evidence and two points tell strongly against Cook's suggestion. Firstly, at the very moment that Agesilaus received news of the Pathos in Lechaeum *πρὸς βεβαίαι δὲ ἄλλοθεν τε πολλὰ παρήσαν καὶ ἐκ Βοιωτῶν ἦκον ἐρησόμενοι τί ἂν ποιοῦντες εἰρήνης τόχουεν*: after the news the Boeotians *περὶ τῆς εἰρήνης οὐκέτι ἐμέμνηντο*.⁴ This was in early summer 390 at the time of the Isthmia.⁵ So Thebes can hardly have been at peace with Sparta for the preceding twelve months. Secondly, in the *Plataicus* (26 f.) Isocrates remarked on the faithlessness of the Thebans and said *γενομένου τοῦ Κορινθιακοῦ πολέμου*

διὰ τὴν ὕβριν τὴν τούτων, καὶ Λακεδαιμονίων μὲν ἐπ' αὐτοὺς στρατευσάντων, δι' ὧν δὲ σωθέντες οὐχ ὅπως τούτων χάριν ἀπέδοσαν, ἀλλ' ἐπειδὴ διελέλυνθε τὸν πόλεμον, ἀπολιπόντες ὧν εἰς τὴν Λακεδαιμονίων συμμαχίαν εἰσῆλθον—i.e. after the King's Peace they deserted Athens. If they had done so during the war, surely Isocrates would have said so. These two points seem to me decisive against Cook's suggestion. Nor does the Andocides passage present any great difficulty to the orthodox view. By the time that Andocides was delivering his speech the Boeotians had voted to accept the peace,⁶ as Andocides wished Athens to do, but presumably they were only a few days in advance of the debate at Athens: they had not yet acted on their decision, and that is why Andocides uses the present tense in ch. 20. So, when Athens decided against the peace, the Boeotians did not act on their own, but continued to endure the attacks of the mora in Orchomenus⁷ and so wanted peace eighteen months later. Thus Cook's explanation of how the Theban coin-type came to be used will not suffice, even if it is right wholly to dissociate the two gold Theban coins⁸ from the *συνμαχικόν*.

The next matter is the participation of Byzantium in the Alliance. Cook does not find it 'attractive or inherently probable' that, 'unknown to us, Byzantium may have joined Conon and Pharnabazus in 394 and subsequently returned to the Spartan allegiance prior to Thrasybulus' visit'. On the contrary, I find this hypothesis neither unattractive nor inherently improbable. When Dercylidas⁹ *οὐχ ὥσπερ οἱ ἄλλοι ἄρμους ἐξέλειπεν, ἀλλὰ κατέσχε τὴν Ἀβυδὸν καὶ διέσφρε φλὴν τοῖς Λακεδαιμονίοις*, he appears to have wholly neglected Byzantium, important though that city was from the Athenian point of view. He induced the people of Abydos to welcome the harmosts who came on their own to the city (*τοὺς ἰόντας*), and to send for those elsewhere (*τοὺς ἀπόντας*). Then crossing over to Sestus he assembled there *ὅσοι . . . ἐκ τῶν ἐν τῇ Εὐρώπῃ πόλεον ἄρμους ἐξέπιπτον* and encouraged the Sestians by asking *ποῖον μὲν ἂν ἰσχυρότερον Σηστοῦ λάβοιτε χωρίον, ποῖον δὲ δυσπολιорκητότερον*; Never

¹ There is an error on p. 75 of that article, which I would like to take this opportunity of correcting. In line 17 the words 'the battle of' should be deleted and in line 18 there should be a comma after 'the Spartans', not a full-stop.

² Cook (note 78) remarks that the citation from Philochorus by the Scholiast to Ar. *Ecd.* 193 (*περὶ δὲ τοῦ συμμαχικοῦ Φιλόχορος ἱστορεῖ ὅτι πρὸ δύο ἐτῶν ἐγένετο συμμαχία Λακεδαιμονίων καὶ Βοιωτῶν*) 'raises major problems'—major indeed, unless one makes the correction of *Λακεδαιμονίων* to *Ἀθηναίων*

which has been, I think, universally accepted. Major problems would also arise if Plato, *Menex.* 245 C-E were taken literally.

³ 24 f. and 28.

⁴ Xen., *Hell.* iv 5.6 and 9.

⁵ I follow the chronology of Beloch, *GG* iii².2 221.

⁶ Cf. 13.

⁷ Xen., *Hell.* v 1.29.

⁸ Cf. Cawkwell, *loc. cit.*, 74.

⁹ Xen., *Hell.* 4 8.3-5.

a sign of interest in Byzantium, which Cook supposes to have been a bastion of Spartan influence at that date. When Pharnabazus arrived on the scene, he threatened Sestus and Abydos, and set Conon to blockade the cities and *ἐντρεπίζεσθαι τὰς καθ' ἑλλησποντον πόλεις, ὅπως εἰς τὸ ἔαρ ὅτι πλείστον ναυτικὸν ἀθροισθείη*.¹⁰ True, Xenophon says no more about this than *τὸν χειμῶνα ἐν τοιοῦτοις ὄντες διῶγον*, but it seems likely enough that Pharnabazus felt free to sail in the spring because the task was complete and Spartan influence restricted to Sestus and Abydos.¹¹ Now all this is fairly detailed, and the total absence of Byzantium from the account, coupled with the remarks about the assembling of expelled harmosts, is at least encouraging to the hypothesis of a change in that city. If we inspect the evidence relating to Thrasybulus' doings at Byzantium in 390/89, we are emboldened still further. Historians tend to talk of the capture of Byzantium and this disposes them to think that it remained Spartan until then. The evidence, however, does not exactly support this. Xenophon says: - - - *πλεύσας εἰς Βυζάντιον ἀπέδοτο τὴν δεκάτην τῶν ἐκ τοῦ Πόντου πλεόντων μετέστησε δὲ ἐξ ὀλιγαρχίας εἰς τὸ δημοκρατεῖσθαι τοὺς Βυζαντίους*.¹² That is, he sailed there and farmed the tax and changed the constitution. One may contrast the situation in 410 after the battle of Cyzicus when to get this tax collected Alcibiades set up a *δεκατεντήριον* at Chrysopolis;¹³ Byzantium was still hostile to Athens. Something less than capture seems to be implied in 390/89. Similarly with the allusions of Demosthenes.¹⁴ In the case of Thasos a Spartan garrison had to be expelled, but there is nothing said about this in the case of Byzantium. Surely, if the city had remained pro-Spartan, it would not have been left for long unprovided with a garrison. There certainly was oligarchy there from 394/3 to 390/89, but this did not necessarily imply that the city was pro-Spartan. Indeed there is a hint to this effect. When Lysias attacked Ergocles, he accused him of advising Thrasybulus to seize Byzantium and establish an oligarchy.¹⁵ Presumably this could only be credible if oligarchy in Byzantium did not necessarily imply that Thrasybulus would be handed over to Sparta. We should not forget that the city had suffered unusually much under Clearchus,¹⁶ and it would not be surprising if there were oligarchs there

who wanted neither Sparta nor Athens and who, recognising the need for a patron, were ready in 394 to join a Persian-sponsored league for freedom. Not even Spartan control had been able to quell completely the dissident factions in the city,¹⁷ and the great collapse of Spartan naval power in 394 is likely enough to have been reflected in the affairs of the city. In view of all this, I do not find the hypothesis unattractive or inherently improbable.

Again, there is the matter of Cnidus. Cook invites us to suppose that the city remained under Spartan influence between the battle and the arrival of Ecdicus in 391/0. 'The Spartan fleet was based on Cnidus before the battle in 394, and Cnidus must have continued to serve as the Spartans' fortress after their defeat when Conon and Pharnabazus were liberating the Greek cities of the East Aegean. Fifty of the eighty-five ships in the Spartan fleet had been lost in the battle; but the crews had escaped, except for 500 sailors captured and the Spartan force guaranteeing Cnidus (including the East Aegean allies) must have numbered between 15,000 and 20,000 men. There is no mention of Conon and Pharnabazus attacking this hornet's nest, and it is scarcely conceivable that they should have done so.' But the hornets' nest may not have remained for long, if the crews Sparta had hired were like those of Athens. Blockaded on sea by Conon and with a hostile land behind, they had little prospect of pay or supplies and it would not be surprising if they cleared away *ὥσπερ νέφος*. Nor may we infer more from the fact that Ecdicus sailed to Cnidus in 391/0 than from the fact that in the same period Teleutias sailed to Samos:¹⁸ presumably the Spartans did not remain in control there. After the battle in 394 Pharnabazus and Conon *περιπλέοντες καὶ τὰς νήσους καὶ πρὸς τὰς ἐπιθαλαττιδίας πόλεις τοὺς τε Λακωνικοὺς ἄρμους ἐξήλανον καὶ παρεμβοῦντο τὰς πόλεις ὡς οὔτε ἀκροπόλεις ἐντειχίσουεν ἑασοῦν τε αὐτονόμους*.¹⁹ This had its effect. Xenophon speaks of the popularity of Pharnabazus; Diodorus says *τοιαύτη τῆς μεταστάσεως σπονδὴ τις εἰς τὰς πόλεις ἐνέπεσεν*.²⁰ In these circumstances Cnidus, relieved like others of her fears of Persia, may well have joined the League and left Pharnabazus free to go north. If the Spartan station was, as Cook suggests,²¹ outside the city, the change would have been the less difficult.²²

¹⁰ Xen., *Hell.* iv 6 f.

¹¹ Xen., *Hell.* iv 8.32 is generally taken to imply that Dercylidas remained at Abydos. (iv. 8.26 *οὐδενὸς ἀντιπάλου παρόντος* does not contradict this—Thrasybulus had a fleet of forty ships.)

¹² *Hell.* iv 8.27.

¹³ Xen., *Hell.* i 1.22.

¹⁴ xx 59 and 60.

¹⁵ Lys. xxviii 5 and 7.

¹⁶ Diod. xiv 12.3 f.; Polyaeus ii 2.7.

¹⁷ Xen., *Anab.* vii 1.39; cf. Diod., *loc. cit.*

¹⁸ Xen., *Hell.* iv 8.22 f. Diod. xiv 97.3 explicitly says that Samos revolted from Athens at that date.

¹⁹ *Ibid.*, iv 8.1.

²⁰ Diod. xiv 84.4.

²¹ P. 67 f.

²² What happened to the ships that got back safe to Cnidus after the battle (Diod. xiv 83.7)? If Cook were right in his view that the Spartans held on to Cnidus from 394 to 391, these ships should emerge from dock to play their part in the Spartan revival. To help the Rhodian exiles the Spartans sent eight ships (Xen., *Hell.* iv 8.20; according to Diod. xiv 97.3 seven) and they collected ships *πανταχόθεν* to form a fleet of twenty-seven (Diod., *loc. cit.*, 4), and in

Finally, with regard to the coincidence that four of the cities from which the coins originate are known to have been under Spartan control in 391/0, it should be remembered that very few of these coins exist and many more cities may have participated. How many, of course, we cannot guess. Evidently some took a more active part after liberation than others,²² and places like Erythrae may have been too pro-Athenian to join a league sponsored by Persia. At any rate in judging whether this league was sponsored by Sparta, it should be kept firmly in mind that the weight of these coins seems to be adjusted to the Persian system.²⁴

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this total, which Xenophon also gave, he included twelve ships under Teleutias from the Gulf of Corinth (*Hell.* iv 8.23 f.). There cannot have been many ships at Cnidus in 391 even if the Spartans had stayed there. Nor would one guess from Xen., i.e. *Hell.* iv 8.10 f. that ships from the East Aegean had slipped back to the Peloponnese in the meantime.

²² Diod. xiv 84.4.

²⁴ Cf. Cawkwell, *loc. cit.*, 74.

Cyrenaican Expeditions: Note

The reviewer of A. Rowe, 'Cyrenaican Expeditions' (*JHS* lxxii 199-200), in commenting on the catalogue of the coins from the excavations, criticises, without grounds for so doing, the absence of illustrations and the fact that the short descriptions in the text closely follow Robinson, *British Museum Catalogue of the Greek Coins of Cyrenaica* (CGC) and Svoronos, *Νομίσματα τοῦ κράτους τῶν Πτολεμαίων* (SN).

The reviewer appears not to have read the footnotes (especially n. 4, p. 29) in which I clearly discuss the whole problem of recording the coins in question:

'Many of the coins bearing these types are, as the present example, heavily corroded. Weights and sizes have therefore been omitted and the detail recorded comes from the composite evidence of other less damaged specimens (from CGC and SN). The same practice has been followed throughout wherever expedient. In view of the condition of the coins generally, references to standard catalogues are given instead of illustrations.'

The practice of referring to identical issues recorded in standard catalogues seemed to me preferable to the inclusion of a set of illustrations which no printer would have been eager to accept in view of the worn or damaged condition of many of the coins represented.

Two other points apparently need further clarification. Coin xiii (M. 458), as the text states, is a

worn specimen; a trace of a numeral letter is visible in the appropriate position in the field but this cannot be identified with any certainty. Secondly, I would like to assure the reviewer that the silver coin xviii (M. 801) was in fact independently dated on the basis of style and not, as he implies, by reference to the context of the find!

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The Borchardt Measure (PLATE VI)

The subject of this note is a bronze measure bought at Sotheby's in 1948. It was catalogued simply as 'A Byzantine bronze grain measure of cylindrical form, the exterior horizontally grooved and with four lines of inscriptions round the rim, dark patination, with malachite green encrustation, 5½ in., 7th/8th Century, fitted case'.¹ I bought it partly because the lettering was a rounded uncial somewhat similar to that of a manuscript in which I was then interested; partly because the longish inscription seemed to offer a possibility of finding out something of the circumstances of its production; partly because it was in itself a pleasant object. After puzzling for some time in moments of leisure over the inscription, I wrote to Professor Bernard Ashmole about it. He pointed out that if I had been using the up-to-date Liddell and Scott—a failure just slightly less unpardonable than it would be now—I would have found that the measure had already been published. As the articles indicated to which he referred me (*AA* 1923-24 153-64, in *JdI* xxxviii-ix) Borchardt, who first brought the measure to notice, was shown it in Alexandria by a dealer who told him that it came from Antioch. There seems, however, to have been doubt in Borchardt's mind as to whether he had understood what the dealer said on this point.² Viedebant discussed, at Borchardt's request, historical, metrological and other questions to which the measure gave rise. The only justification for a further note is that no photograph was published with the article of Borchardt and Viedebant; secondly, that Sotheby's catalogue did not identify the measure as Borchardt's; and thirdly, the possibility that a passage in Procopius, not mentioned in that article, may be connected with our inscription.

Though the inscription was printed and fully discussed (and I am in no way qualified to comment in detail on that discussion), it will clearly be con-

¹ The height is 0.132; diameter 0.103 (top), 0.105 (base); thickness of walls 1.5-2 mm. The handle is broken away but the rectangular points of attachment (*ca.* 0.019 × 0.024) are visible in the photograph. Its capacity is 960 millilitres, or 33.5 fluid ounces.

² 'So habe ich den Ortsnamen verstanden.'

venient to transcribe it again, as it stands, deeply engraved in the bronze. It reads as follows:³

ΕΙΤΑΛΙΚΟΣ ΤΗΣ ΔΕΣΠΟΤΙΑΣ ΤΟΥ ΚΟΜΗΤΟΣ ΟΥ
ΑΔΙΛΑ
ΑΠΟΚΕΛΕΥΣΕΩΣ ΤΟΥ ΑΡΧΟΝΤΟΣ ΣΕΓΡΑΨΑΚΑΙ
ΕΞΑ
ΓΙΑΣΜΕΝΟΝ ΕΞΕΣΤΙΝ ΑΠΟ ΟΝΟΜΑΤΟΣ ΕΟΡΤΑΣ
ΙΟΥ
ΔΙΚΑΙΟΝ ΟΝΚΙΩΝΙΚΟ ΣΙΤΕΑΡΩΝ ΣΤΡΑΤΙΩΤΙ
ΚΩΝ

or with the word divisions marked, presumably this:

ειταλικος της δεσποτίας του κομητος οναδila
απο κελενσεως του αρχωντος εγραφα και εξα-
γιασμενον ξεστιν απο ονοματος εορτασιου
δικαιον ονκιων ικοσι τεσαρων στρατιωτικων

The word *και* before *εξαγιασμενον* is a difficulty. Viedebant preferred the suggestion *κατεξαγιασμενον* to Borchardt's *και εξαγιασα μόνον*, and took it to be an emphatic form. He thought that the adjective *εορτασιου* might be the equivalent of *solemnis*, the phrase *απο ονοματος εορτασιου δικαιον ξεστιν* therefore meaning 'ein xestion, das rechtlich-formlich gesprochen, richtig ist'. The inscription may be paraphrased for our purpose as follows:

'I *Italicus* [or, an *Italian*] of the dominion of Count Wadila, inscribed in accordance with an order of the colonel [or more likely, perhaps, of my master, Count Wadila; but *αρχων* seems to be used by Procopius at least, often of a fairly senior army officer] this *Sestarius*, duly certified hereby as a precisely accurate measure of twenty-four soldiers' ounces.' It should perhaps be noted that *εορτασιου* might itself be a name, as Viedebant suggests in a footnote. If so, it would presumably pile up the emphasis on the accuracy of the measure by associating with it yet another personality, apart from the writer of the inscription and some senior officer or his overlord Count Wadila. The text, then, whichever way we take it in detail, has an exaggerated emphasis on the absolute accuracy of the measure that contrasts surprisingly with the terse certificates which measures sometimes carry, and which suggests accordingly some special situation. The specification of soldiers' ounces and possibly also the mention of the archon make it necessary to assume a military context. And what seems to the amateur to be an unlikely feature is the definition of the measure's standard in terms of weight, when it is in fact a measure of capacity. The weight of its contents in cornflakes would not be the same as the weight of its contents in lentils. If the one were twenty-four soldiers' ounces, the other

³ Mr John Boardman (to whom I am greatly indebted for his help with this note) draws my attention to punctuation points (lines of three or four dots arranged vertically) at the end of l. 2; between the words *απο* and *ονοματος* in l. 3; and

would certainly be something different. A reasonable deduction might be that it was not made for general use, but for a particular commodity. Indeed, even corn weighed differently according to its place of origin, as Pliny (*NH* xviii 66) mentions in a passage which Viedebant quotes.

Viedebant discusses the name Wadila, accepting as most probable Borchardt's suggestion that he was a Goth. He mentions that the Ostrogothic King Totila was also called Baduila; and moreover, that Theodoric the Great writes to an *Adilae v(iro) s(pectabili) comiti* some time between the years 507 and 511 who may have been established in Sicily and who might, so Viedebant seems to think, conceivably be the very Wadila of our measure. If so, this would put it in the first quarter of the sixth century or thereabouts. A Gothic contingent is mentioned by Procopius in his narrative of an expedition, against Chosroes, led by Belisarius in 546. He does not mention Goths in describing the forces sent with Belisarius against the Vandals in 533; he emphasises, however, the miscellaneous nature of those forces, saying that nowadays it is possible for anyone to call themselves '*foederati*'. In a footnote to the passage concerned (*Wars* iii 11.3-4) the Loeb editor writes: 'These *foederati* were private bands of troops under the leadership of condottieri; these had the title of "Count" and received from the state an allowance for the support of their bands.' A Gothic contingent, led by its Count, might be a fair hypothesis in the war of 533.

The context of the passage now to be quoted from Procopius (*Wars* iii [Vandalic War] 13.12-20) is the outward voyage of Belisarius' expedition to N. Africa in 533. We are concerned therefore in it with an event belonging to a period some twenty-five years later than that of the Theodoric letter mentioned. After rounding Cape Malea, the fleet put in to Methone and the soldiers were disembarked; and Procopius who himself went with the expedition says that there was a serious outbreak of sickness. 'John the pretorian prefect was a disreputable figure, and his manoeuvres designed to bring money into the treasury regardless of the damage they caused were such that I could not possibly do them justice in describing them. Something has been said of this already at that point in my narrative, but I will now relate the harm he did to this particular expedition. It is essential for the biscuit which is to be provided for an army on foreign service to be baked twice over; to be baked with such care that it will last for as long as possible and not go bad in a short time. Such biscuit of course weighs less than the normal weight [i.e. for the corresponding quantity of flour used, because it is comparatively dehydrated]. For this reason it is the ordinary practice for the weight to be reduced

between the words *δικαιον* and *ονκιων* in l. 4. Also to a correction made by the engraver in l. 4, where *ικοσι* was first written *ινοσι*, and *τεσαρων* written *τεταρων*.

by a quarter when the rations of biscuit are handed out to the troops concerned.⁴ Now John reckoned that if the dough could be taken to the Baths of Achilles and placed over the furnace there, savings could be made in the fuel and wages paid to the bakers, and at the same time there would be no loss of weight. When this primitive baking process appeared to be finished, the biscuit was put into sacks and stowed in the ships. By the time the fleet had reached Methone, the biscuit had disintegrated, and was virtually simply flour again, though not of course sound flour, but rotten and maggoty, with a nauseous smell. The officials whose duty it was to distribute this flour had to serve it out by measure to the troops, by quart and bushel measures [i.e. not by weight as was presumably normal]. It was summer and the place was very hot; and so when they ate it the men fell very sick and in a short time no less than 500 died. . . .

Procopius is, as usual, largely interested in the scandal occasioned by John's parsimony. We cannot necessarily trust him over all details. But he was himself a member of the expedition; and the words italicised in his account, with their explanation of the difference between the soldiers' standard and the normal standard of weight, suggest that the incident involved a dispute about the standard of measurement. To have a measure of capacity marked as so many ounces seems (as noted above) an unlikely proceeding, but explicable perhaps in this particular and very unusual context.

Procopius uses the words *χοῖνιξ* and *μέδμνος* for the measures employed to serve the disintegrated biscuit. Following Dewing in the Loeb Procopius, I translate them here 'quart' and 'bushel'. The Borchardt measure is inscribed as a *ξέστιν* (i.e. *ξεστίων*). *ξεστίων* seems to be a measure of just under a pint capacity. The capacity of this measure, however, is some half as much again, midway between that of the normal *ξεστίων* and the normal *χοῖνιξ*. Is it possible that our measure was made on this actual expedition, the inscription with its protestations of accuracy being designed to allay the recipients' suspicions that they were being cheated, and the curious feature of a measure

⁴ καὶ διὰ τοῦτο ἐν ταῖς τοιαῖσδε ἀρτοδοαίαις εἰώθασιν οἱ στρατιῶται τοῦ συνευρισμένου σταθμοῦ τὸ τεταρτημόριον ἀποτέμεσθαι. Dewing, *Procopius* (Loeb) ii 121-3 translates 'for this reason, when such bread is distributed, the soldiers generally received as their portion one-fourth more than the usual weight'; and suggests that *ἐπαποτέμεσθαι* might be the right reading. But surely the point is that the biscuit being drier, a lesser weight of it than of normal bread represented a given weight of flour? The 'soldiers' ounce' took this into account.

of capacity marked in ounces reflecting the unusual circumstances of the Methone incident?

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Greek Lyric Metre

In her review of my book (*JHS* lxxxii 171) Professor A. M. Dale writes: '... in Pind. *Pyth.* 2.21 θεῶν δ' ἐφετμαῖς | Ἰξίονα φαντί ταῦτα | βροτοῖς we are asked to hear doch., pher., iambus, the last five syllables forming another doch. U--U- by overlap (the extra syllable at the beginning of pher. is not explained). It is not always clear what happens to ictus in these overlaps where there is a conflict, e.g. *E. Alc.* 443 λῦμαν Ἀχεροντίαν πορεύσας ἐλάτα δικώπων, where λῦμαν is left stranded (link?), Ἀχεροντίαν πορεύσας is Anacreontic U-U-U-U-U-, and -σας ἐλάτα δικώπων is pher. by overlap U-U-U-U-. Has -σας ictus or not?'

The identity of the pherecratic-iambus Ἰξίονα φαντί ταῦτα βροτοῖς is established by the occurrence of the same phrase in the First Olympian, as I pointed out (pp. 76, 143). The initial syllable is explained by the fact that here the phrase echoes 2 ἵππων τε σιδασοχαρμῶν, just as it is itself echoed in the final cadence 24 ἐποιοχόμενος τίνεσθαι. All this is made quite clear, I think, in my analysis of the composition as a whole (pp. 141-2, 157).

The words λῦμαν Ἀχεροντίαν πορεύσας constitute a single phrase, which contains an Anacreontic, like τὸν ἀνάλιον οἶκον οἰκετεύεις and πολὺ δὴ πολὺ δὴ γυναικ' ἀρίστην in the same strophe, also Bacch. 17.4 σάλπιγξ πολεμῆται ἀοιδῶν, quoted in the footnote; and several more examples will be found on p. 153, where the phrase U-U-U-U-U-U- is classified as a development of the Anacreontic: *E. Alc.* 460 σὺ γὰρ ὦ, μόνῃ, ὦ φίλα γυναικῶν, *Hp.* 526-8 στάσεις πόθον εἰσάγων γλυκεῖαν/ψυχῇ χάριν οὗς ἐπιστρατεύσῃ, μὴ μοί ποτε σὺν κακῷ φανείης, *Hec.* 927 ἐπιδέμμιος ὥς πέσοιμ' ἐς ἐνὶν, *Rh.* 901 ἀπὸ δ' ἀντομένον πατρός βιαίως, *Pind.* I 7.1 τίνι τῶν πάρος, ὦ μάκαιρα Θήβα. So λῦμαν is not a link, nor is it left stranded.

The question about ictus is, of course, merely hypothetical because, as I remarked (p. 7), and as Professor Dale agrees, it is doubtful whether there was an ictus in ancient Greek music of the classical period; but, if there was, then presumably the word πορεύσας carries a double ictus here (U U) in virtue of the overlap, as in Hopkins' 'sprung' rhythm. The importance of Gerard Manley Hopkins' work on Greek lyric metre, which was unknown when my book was first published, is discussed in the preface to the new edition.

GEORGE THOMSON.

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NOTICES OF BOOKS

MEYER (E.) **Heinrich Schliemann Briefwechsel.**

II. Band: von 1876 bis 1890. Berlin: Verlag Gebr. Mann. 1958. Pp. 488, with 15 plates.

The first volume of Meyer's selection of letters to and from Schliemann covered the years 1842-75 (reviewed by Wace in *JHS* lxxvii 366 f.). The new one brings us down to the year of Schliemann's death, embracing the period of his work at Mycenae, Tiryns and Orchomenos, as well as his abortive plans for Knossos. There is much here in the way of factual evidence still to be gleaned by the archaeologist but the real value of the letters lies in the way they portray, more clearly than any biography, the character of Schliemann himself. He was petulant and impatient with authorities who impeded him and with scholars who would not see reason. To refute Bötticher, who said that the plans of Troy were faked and the site really a 'fire-necropolis' he reopened work there and invited delegations from the Academies of Europe. The attacks of those who said that his finds at Mycenae belonged to the invasions of the Gauls, not the Dorians, distressed him deeply. Evans defended him, and the Hellenic Society arranged a special meeting in 1886 to discuss the matter. Penrose, one of the critics, later visited Mycenae and Tiryns with Dörpfeld and politely recanted. Schliemann appears generous in his appreciation of those who helped him, like Dörpfeld: almost obsequious in his approach to some great scholars; sometimes naïve, as in his observation that the skeletons from the Chaconea grave had not got 'classical' noses; compassionate and tender in his letters to his wife.

Dr Meyer has earned our gratitude for his skilful handling of the letters and for presenting us with so vivid a picture of a truly great man. At the end of the book he mourns the loss of the letters to Max Müller, Oxford's first Professor of Comparative Philology, who helped Schliemann in his communications to the English press; as Müller puts it, 'I am a kind of franc-tireur for both papers' (*Times* and *Academy*). The letters have since been found and were published by Meyer in the last volume of *JHS*: a fair recompense for Schliemann's polite remark to a former editor of *JHS* 'Inconsistencies in a paper like the *Athenaeum* are pardonable, but they would be unpardonable if I committed them in such an important work as the *Journal of Hellenic Studies*'.

JOHN BOARDMAN.

KIRK (G. S.) **The Songs of Homer.** Cambridge: the University Press. 1962. Pp. xi + 424. 3 maps. 8 plates. £2.

An analytical critic, reading this book, might detect three hands: a Salutary Agnostic, who knocks

down other scholar's conjectures, so that a Gay Believer can erect his own conjectures, and a Conscientious Rhapsode, who has reported almost all the facts that have been thought relevant to the Homeric problem, thus making a book of the greatest possible use to students.

The first part gives the historical background of the Homeric poems. The Greeks are identified with Middle Helladic invaders (between 2000 and 1850). The rise of Mycenae is associated with a small influx of charioted Greek speakers from Asia Minor. The Trojan War is the attack on Troy VII A (c. 1240-30). Mr Kirk then uses the Linear B tablets to describe life in a Mycenaean state; the Salutary Agnostic, naturally, will have nothing to do with divine kingship (and yet the schoolboy's first question is why are heroes always divine, dear to Zeus, Zeus-born, Zeus-bred, god-like, like a god, sprigs of Ares, etc.?), and doubts whether Dionysos on the tablets is a god; he also notes that comparatively little of the complicated organisation survives in Homer (cf. also p. 123), but this point would only have chronological significance if it could be shown that the equally complicated organisation of Ugarit appeared in the Ugaritic epics. There follows an account of the Ionian migration with a welcome stress on Athens and high-dating of the migration.

The second part is called 'The Oral Poet and His Methods'. The account of Homeric formulae is excellent, and Mr Kirk makes a case that the *Iliad* could have been composed orally, with a good analysis of XVI, 102-357. The discussion of the Yugoslav material is very useful, and the Agnostic points out some of the hazards in the analogy: (1) there is nothing approaching the rigidity of the Greek formular structure; (2) the metre is uncomplicated (so also all early Near Eastern metres); (3) the poets were not creative but reproductive; (4) the difference in quality is vast. I should add (5) unlike the Greek poems, the Yugoslav poems were not continually brought up to date, perhaps because the social status of the audience (unlike the Greek) continually declined; (6) literacy killed the Yugoslav poets because it brought them into touch with a higher culture; there is no reason why it should have had the same effect on Greek oral poets, and the transition may have been much more gradual. (What I miss here is a survey of the two cases where we have at least intermittently texts of formular poetry over a long period: Gilgamesh and some of the Yugoslav poems. This might show what happens to the story and its formulae over 400 years.) The suggestion of a creative stage (down to and including the new monumental epic), a reproductive stage (early seventh century), a degenerate stage (from

the late seventh century onwards) is interesting; the second two stages cover the oral transmission of an essentially complete *Iliad* and *Odyssey* until it was recorded in the sixth century in Athens, although on p. 310 the possibility of an earlier text is admitted. (I still feel that the genius who created the monumental epic (p. 281) created it for continuous recitation, which implies an eighth-century text for his reciters.)

Part Three is called 'The Growth of the Oral Epic in Greece'. The Agnostic reduces to a minimum the evidence for Mycenaean epic in favour of stories in prose first versified in the Dark Ages (for which *Od.* 4.238-9 seems to me inadequate evidence); but belief in Mycenaean poetry ebbs back as the chapter proceeds, and on p. 237 we learn that Teiresias was 'perhaps already well-established in the North Mycenaean poetical tradition'. The emphasis on the poetic creativeness of the dark ages is of course right (but they could not create knowledge of a rich civilisation, and I do not see how, if Athens was 'quite the most important town in Greece' (p. 129) and the refugees flowed through Athens to Ionia, it is possible to exaggerate its importance for the epic). There follows a sober account of linguistic elements and Aeolisms both in language and subject-matter.

Then Plurality and Unity in Homer. This includes first an excellent original classification of Homeric styles into 'tired formula', 'majestic', 'decorated lyric', etc. Here more precise description and more examples are needed, and it is to be hoped that Mr Kirk will give them elsewhere. Then 'the cultural and linguistic amalgam'. On the archaeologically datable references from the Mycenaean period to the eighth century, I would note: p. 181, Ajax and Hector with body-shields, their survival implies an element of 'fiction' in the account of the Trojan War; p. 184 'writing was unknown in the crucial formative stage . . . and so the epic world is made illiterate', but (a) the Ugaritic epic world is also illiterate, (b) we do not know when Linear B disappeared; p. 185, on Athena's lamp cf. Pfeiffer, *SIFC*, 27/8, 1956, 426; p. 187, hoplite shields on Attic geometric vases should also be noted. In the linguistic section the discussion of anti-traditional (post-Homeric) phraseology is good, but I find nothing ludicrous in *Od.* 20, 13. In 'structural anomalies' the Agnostic does excellent work in preserving for Homer much that has been questioned; I would only put in a plea that *Iliad* 1, 356, etc., is entirely right from Achilles' point of view, that *Od.* 5 would be unintelligible without a divine council, that in *Od.* 11 the Heroines are part of a carefully balanced structure, and that the *Odyssey* must also have a final divine council (this point was, I think, made by Professor Pfeiffer in a lecture to the Hellenic Society). Thus with minor subtractions the *Iliad* and the *Odyssey* can be accepted as Unities, but the Agnostic is fierce with those who see dynamic pattern, static pattern (cf., however, p. 281) or pattern of similes: to see consistency of character and unity of plot is, however, permissible.

According to Part V, 'Development and Transmission', both poems were composed in Ionia probably in the late eighth century. The dating rests on a well-classified summary of the usual criteria. Miss Brann's 'Attic Death of Astyanax', c. 720 (*Agora* VIII no. 311) should be added. (Are these vases, as I thought, evidence of mainland poetry or was Ionian poetry already known in Attica? The inscriptions on the Dipylon jug and the Ischia cup perhaps suggest the possibility of the latter.) The poems are by different authors, who were, however, near to each other in time and place (Mr Kirk's list of formulae exclusive to the *Odyssey* contains seven which I should not have expected to find in the *Iliad*; I also doubt if the use of the 'rosy-fingered dawn' line twenty times as a refrain in the *Odyssey* is significant; the *Iliad* has only eight dawns and three of them 'appear rosy-fingered').

In the following section on the crucial phases of transmission it is suggested that the Doloneia may have been added in the sixth century. In discussing rhapsodes Mr Kirk says that their staff 'cannot have been a symbol of inspiration or poetical authority like the branch or wand with which Hesiod was touched by the Muses'; but Hesiod says 'they gave me a *skeptron*, a branch of laurel, to pluck' (*Theog.* 30), i.e. they made him a rhapsode. It is, of course, true that the transition from lyre to staff is the transition from singer to reciter (possibly with flute accompaniment, cf. back of the vase, Kirk, pl. 7b, and *JDAI* 76, 1961, 48).

I cannot here discuss more fully the interesting chapters on stages of development and on the songs and their qualities. I do not find the *Iliad* too long or the Embassy inessential to the main plot or the *Odyssey* excessively leisurely; but much is excellent here and, in spite of some concessions to the analysts, the essence of two superb poems is made clear. The few illustrations are well chosen and well reproduced.

T. B. L. WEBSTER.

SCHOECK (G.) *Ilias und Aithiopis: kyklische Motive in homerischer Brechung*. Zürich: Atlantis Verlag. 1961. Pp. 142. Price not stated.

In 1945 the Swiss scholar, Heinrich Pestalozzi, published a short monograph under the title *Die Achilleis als Quelle der Ilias*, in which he argued that the author of the *Iliad* had made extensive use of an earlier poem about Achilles, the last four episodes of which were Antilochus' self-sacrifice in defence of Nestor against Memnon, Achilles' killing of Memnon, the death of Achilles and Achilles' funeral. These four episodes are said to have formed part of the cyclic *Aethiopis*; and though Pestalozzi did not go quite so far as to assert explicitly that the *Achilleis* and the *Aethiopis* were one and the same poem, his view that the Memnon episodes, in the form in which they are alleged to have appeared in the *Aethiopis*, were used as source-material by the composer of the *Iliad* has found distinguished patronage, not only in Pestalozzi's native land but also in Germany. In

this work, as may be gathered from its title (and even more clearly from its sub-title, 'Kykliche Motive in homerischer Brechung'), Dr Schoeck, who was a pupil of Professor Howaldt of Zürich, is concerned to discuss the use made by Homer of this supposed source. Dr Schoeck's monograph, which began life as a doctoral dissertation under the title 'Die homerische Assoziationstechnik als Basis der Erfindung', is divided into a series of essays and notes, the first of which (pp. 7-31) deals with 'Ilias und Memnonis' with sections on structural parallels and on Homer's use of these parallels in dealing with Nestor's danger in *Iliad* V and with the deaths of Sarpedon and Hector. In the second (32-6), we meet 'Der Nebel als Indiz einer Leichenkampfschicht'; then come 'Die Thetisszenen in der Ilias' (38-48) and a detailed study of Book XVI ('Anwendung der Methode auf das II'; 49-80). Notes on Book XI ('Der A-Zug; 81-4), on 'Der Leichenkampf (Interpretation des P unter kyklichem Aspekt)' (92-103) and 'Das Leichenbegängnis des Patroklos unter kyklichem Aspekt' (104-8) follow; an 'Erweiternder Überblick ("pattern" und kykliches Grossfeld)' (109-20), an 'Anhang' (121-38; six sections, of which the last, dealing with the Chalcidic amphora which shows the death of Achilles, is the longest), a short bibliography (139-40), and a table of contents complete the book.

I have never been able to take Pestalozzi's theory of the priority of his *Achilleis* to the *Iliad* quite seriously, especially when *Achilleis* and *Aethiopsis* are identified (cf. *CR* xli [1947] 29); and Schoeck's pompous new terminology and, above all, his stern rebuke to all Homeric scholars who accept Aristarchus' doctrine of the post-Homeric character of the cyclic poems for their 'Grundirrtum' would hardly have disposed me to contemplate his meta-Pestalozzian lucubrations with a particularly sympathetic eye in the best of circumstances—but luckily it now seems to me quite unnecessary to make a detailed examination of his arguments, since Karl Reinhardt's discussion of 'Ilias und Aithiopsis' (*Die Ilias und ihr Dichter*, 1961, 349-90) may reasonably be thought to have demonstrated once for all that there are no grounds for believing that the author of the *Iliad* was acquainted with any poem about Memnon at all, let alone the poem so imaginatively reconstructed by Pestalozzi and his followers out of the fragments. One may condole with Dr Schoeck for the unfortunate concatenation of events which not only allowed him to produce his book when the demonstration of its 'Grundirrtum' was already in existence but also ensured that his book and Reinhardt's should have appeared practically simultaneously.

J. A. DAVISON.

FOUNDATION HARDT. Entretiens sur l'antiquité classique, vii. Hésiode et son influence.

Vandoeuvres-Geneva: Fondation Hardt (Cambridge: Heffer). 1962. Pp. vi + 311. £2 10s. od.

Of the six papers in this volume three, 'Das

Hesiodische in den Werken Hesiods' by von Fritz, Kirk on the structure and aim of the *Theogony* and Verdenius' similar examination of the *Erga*, discuss the poems by Hesiod, while the others, contributed by Solmsen, La Penna and Grimal, look at Hesiodic elements in Plato, Virgil and Tibullus. I must admit being somewhat perplexed by the subjects chosen to illustrate Hesiod's influence. Solmsen's article contains much of value, but surely, if one wants to trace the impact of Hesiod on philosophy, the Pre-Socratics offer more scope. A single essay on the tradition of the Golden Age after Hesiod might well have covered much of the ground traversed in all three papers. This would have left space for something to be said about Hesiod in the Hellenistic period and the *Catalogue* as a source-book of mythology. The nearest we get is a page and a half about Hesiod and Callimachus tacked on to the end of the first contribution and odd remarks elsewhere. One appreciates that special factors determine the contents of a collection of this kind, but it certainly comes as a bit of an anticlimax to find that it ends with Grimal quoting parallels to show that Tibullus 'était imprégné de la pensée d'Hésiode'.

Von Fritz has interesting things to say about Eros and Aphrodite and the idea of order and justice in the *Theogony*. He also discusses passages from the first half of the *Erga*. If his article seems long-winded and not very original, it is probably because it suffers from a comparison with those by Kirk and Verdenius. These two provide the meat of the book. Kirk's approach, however, does give added force to von Fritz's remarks about the inadequacies of a strictly logical analysis of the text. Kirk springs a surprise on us at once by dating the *Theogony* not earlier than around 675. One wonders how this date is to be reconciled with the political and economic background of the *Erga*, which he acknowledges to be the later composition. It is worth noting also that already about this time we appear to have a couple of illustrations of the *Theogony*. I am thinking of the birth of Aphrodite on a terra-cotta plaque from Perachora and Perseus and Medusa on the Proto-Attic amphora from Eleusis, although its publisher Mylonas would dispute an identification with Hesiod's version of the episode. Surprise is followed by shock as we read Kirk's views on the Boeotian school of epic and his list of interpolations and passages which experienced later expansion. Why should Kirk deny epic poetry a long history in mainland Greece, going back in fact to the Mycenaean era? Both poems by Hesiod contain Mycenaean reminiscences like the *Theogony* story of the birth of Zeus in Crete and the expression 'Ἀθηναίης δμῶνος' (*Erga* 430). When we are dealing with a literary language of a pan-Hellenic character, how can we claim that a 'native dialectal influence... would be the inevitable result of a strong pre-Homeric local tradition'? Kirk's comments on untraditional and anti-traditional elements in Hesiod's language are most valuable, but I wonder what they really imply. Even Kirk himself admits

that their distribution is not always what one would expect.

If Kirk deserves the highest marks for his refreshing audacity, Verdenius rates even more because of his profound insight into the workings of Hesiod's mind. The two offer a complete contrast in method and conclusions. The iconoclastic Kirk dispenses with footnotes, whereas Verdenius lavishly annotates his text. It is hard to find any omission, although he might have found Palmer's study of dike in *Trans. Philolog. Soc.* 1950 useful. Verdenius staunchly defends our text, and, while he sometimes reads too much between the lines, he is one of the few who have tried to understand Hesiod rather than take the easy way out and suggest interpolation. He adheres closely to a study of the actual text and is full of penetrating observations. He argues that Hesiod followed no rigid scheme of composition, but that the transitions within the poem are made by a process he calls association where one idea suggests other related topics (cf. *REG* lxxiii (1960) 345 ff.). Particularly admirable are his discussions of verses 342-82 with the ingenious suggestion that the idea of sacrifice leads on naturally to that of a feast, of verses 722-59 where the same two ideas occur in reverse order, and of the *Days*. Indeed he is at his best when he traces the development of thought within the different sections of the poem, but is less convincing when it comes to explaining the link between the major divisions of the poem in terms of association.

Over eighty pages are devoted to the discussions which followed each paper. Both they and the book as a whole make this a memorable addition to the study of Hesiod.

P. WALCOT.

BURTON (R. W. B.) *Pindar's Pythian Odes: Essays in Interpretation*. London: Oxford University Press. 1962. Pp. viii + 202. £1 15s. 0d.

The sub-title of this book is 'Essays in Interpretation'; in his preface (pp. v-viii) Mr Burton explains that it 'has grown out of a course of lectures . . . delivered to undergraduates at Oxford over a period of several years'. 'Its method,' he goes on, 'is to examine the structure and content of each ode as a finished work; and a continuous essay on each has seemed to me the most suitable form.' In consequence of this decision the main body of the book consists of twelve chapters, in which the twelve Pythian odes are discussed successively in what Mr Burton believes to be their chronological order: 10, 6, 12, 7, 9, 11, 3, 1, 2, 5, 4 and 8 (it should be noted that the chapter mainly devoted to 5 is headed 'Pythians 4 and 5'; '5 (and 4)' would perhaps have been more accurate). The only point at which I should be inclined myself to question Mr Burton's arrangement is over his placing of Pythian 11; after reading his detailed argument in favour of dating the poem to 474, rather than to 454, I remain unconvinced that the truth has yet been found—he should, I think, have kept Pythian 11

out of the chronological sequence altogether. Apart from this I must say that his decision to deal with the poems in chronological order seems to me entirely justified, though many years' experience of lecturing to undergraduates on Pindar lead me to suggest that it would have been kinder if Mr Burton had given his readers an introductory chapter on what we know of Pindar's life; a concluding chapter, summing up the results of his detailed inquiries into the twelve odes would also have been very helpful.

When one comes to read the essays on the separate poems, I think one is bound to feel that a certain duality of purpose has blurred the writer's vision: the studies are in essence running commentaries on the subject-matter of the poems (and Mr Burton has already warned us that the book can only be read 'in close conjunction with the Greek'), but at the same time a great deal of material which is required to explain the subject-matter has been deliberately excluded: 'textual, linguistic and grammatical points are not discussed except where relevant to my main purpose, which is to interpret Pindar's meaning; and the whole subject of metre is virtually ignored'. How can one interpret the meaning of any ancient author without involving oneself *ab initio* in problems of text, language and grammar I for one simply do not see; and to strip Pindar of his metre and his language leaves one with precious little which is really worth consideration. But there are other things which Mr Burton jettisons: he proclaims himself an unbeliever in the symbolical interpretations of Gilbert Norwood, and in the *o altitudo!* school of Pindaric criticism exemplified for him by Professor J. H. Finley and Mlle Duchemin (no sensible person will complain of him for this), and the very first paragraph of his first essay makes it plain that he has thrown Greek history overboard as well—he describes Thorax as 'a member of the ruling class of Thessaly, the Aleuadae, and hereditary lord of the country' (for 'Thessaly' read 'Larissa', or should one replace 'hereditary' by 'de facto?'). This is no flash in the pan; on p. 33 'the Thebans themselves had medized' (this is before 487) and on p. 34 the Alcmaeonidae are said to have faced Apollo's temple at Delphi with marble instead of poros-stone (this is what Herodotus says, but if so what became of it?). But to return to Pythian 10: I should like to know what makes Mr Burton so sure that the choir on this occasion was composed of 'Thessalian nobles' or that Hippocleas was 'the ruler's favourite' (a term which Mr Burton uses elsewhere in an erotic sense). Indeed, the whole essay on Pythian 10 invites far more detailed discussion than there is room for here: there is no consideration of the relevance of Perseus to a Thessalian occasion, no discussion of the proposition that 'there is an obvious association between Thessaly in Northern Greece and the name Hyperborean' (p. 8; so perhaps it might be logical to speak of Yorkshiremen and Eskimos in the same breath?), no mention of the odd paronomasia in v. 39 (*λυπᾶν τε βοῶνι κρυφαῖ τ' αἰλῶν*), and no serious consideration of Pindar's parting salute to his first patron. It will not

therefore surprise anyone who has worked through Mr Burton's rather monochrome reflections on the meaning of the first eight odes to find that Mr Burton rejects Sir Maurice Bowra's brilliant (and to me convincing) interpretation of Pythian 2—though if it is wrong Pythian 2 must surely join Pythian 11 in the limbo of the undateable.

I am sorry to have been forced to adopt such a negative tone in dealing with this book. English students have long needed a straightforward study of Pindar to counteract the excessive influence inevitably exerted by Norwood's well thought out and attractively presented ingenuities, and I should have liked to be able to say that this book went some way to meet the need. In fact, it seems to me to strip Pindar of all colour and life and to justify up to the hilt Voltaire's reproach against Pindar, of possessing the talent of talking a lot without saying anything.

J. A. DAVISON.

BREITHOLTZ (L.) *Die dorische Farce im griechischen Mutterland vor dem 5. Jahrhundert. Hypothese oder Realität? [Studia Graeca et Latina Gothoburgensia, X.]* Stockholm: Almqvist & Wiksell, for Göteborg University. 1960. Pp. 258. Sw.kr. 28.

The author describes himself as 'an historian of literature, specialising in the poetry of the seventeenth and eighteenth centuries'; a disarming claim, but we need no disarming, for his judgment and discernment are fully adequate to the difficult subject which he discusses in this book. His classical learning and scholarship require correction or supplementation only in a few details which do not affect his argument significantly (e.g. p. 58: ὡς περ οἱ Μεγαροῖ = 'as the choregoi, or their equivalent, do' at Megara; p. 59: παράδοξ in the architectural sense is at least as old as 290 B.C., cf. *SEG* xiii 462.55; pp. 75 f.: Le Paulmier's restoration of *Marmor Parium* A39 has too many letters and is stylistically odd, cf. A37, 43, 46). He betrays unfamiliarity with the handling of his subject only by a certain tendency to defer to secondary authorities (e.g. p. 26, Else; p. 73, Gude-man; p. 108, Deubner; p. 191, an infelicitous and misleading generalisation by Nilsson) and by occasional defects of proportion and emphasis: Arist. *EN* 1123a 23, which could be quietly disposed of in a three-line footnote, gets six pages (pp. 55 ff.), and much trouble is taken (pp. 43 ff.) to demolish the fantasies of the long-forgotten Denis.

Breitholtz's thesis is that there is no significant evidence at all for the existence of comedy, in the sense 'comic drama with spoken dialogue', in the Doric-speaking areas of the Greek mainland earlier than the Old Comedy of Athens. He writes with an agreeable vigour, and most of the props upon which the hypothesis of Dorian comedy rests are quickly knocked away. Aristophanes' references to Megara, even if it were certain (v. *infr.*) that they implied the existence of comic performance at Megara, tell us nothing about its priority. Old Comedy did not inherit 'stock types' from the

Dorians, or from anyone else, since it has none; Breitholtz's pages (83–101) on this subject leave no one any excuse for haziness in definition of the concept 'stock type'. The Megarians' claim to have invented comedy during the time when Megara was a democracy—a claim made, presumably by a Megarian historian, before the writing of Aristotle's *Poetics*—was backed by a feeble etymology which Aristotle reports without comment, but once it was made it coloured subsequent speculation on the subject; no doubt that is why Polemon was able to consider the possibility that 'Maison' came from Sicilian Megara, which was destroyed in 483. The verses attributed to Susarion, or at least the second verse *ὡδὲ Φιλόνων Μεγαροῦθεν Τριποδίακιος*, may have been composed long after the *Poetics*, but even if they were all authentic they would show only that Susarion himself came from Megara and would not conflict with the tradition (*MP* A39) which made Ikaria the place where he created comedy. Semon of Delos speaks of the *φαλλοφόροι*, etc., of his own day, and does not speculate on their antiquity (p. 120); Sosibios called the *δαικηνιστοί* of Sparta *κομικῆς παιδιᾶς* . . . *τρόπος παλαιός*, but how old, and what was his evidence (*ibid.*)? No amount of masks from Sparta or pictures of padded or masked or megalophallic dancers can prove the existence of comic drama as opposed to mimetic dances and songs. Even the resemblance between the Athenian comic actor's dress and supernatural or dressed-up figures on Corinthian vases does not imply historical connexion; Greek ideas of the physically grotesque were much the same at different times and places, and the vase-painter's imagination was apt to break loose in phallic fantasy, as any browser through Licht's *Sittengeschichte* will have discovered.

When the dust of demolition has subsided, what remains standing? First, the Dümmler krater. Humfry Payne thought it 'obvious' that this famous vase depicted a comic performance. Breitholtz's very thorough discussion shows that that is far from obvious. No one but the single dancer is dressed up—the gigantic penis of the man with the stick has its peer in that of the mineworker on a well-known Corinthian plaque (Pfuhl, *Masterpieces &c.*, fig. 17)—and Breitholtz's own suggestion is that the scenes represent events in a potter's workshop; cf. *MDAI* (A) xiv (1889), pp. 150 ff. But the weakest element in an argument of which much is cogent and salutary is the interpretation of *Εἰνός*, *Οφελανδρός* and *Ομηρός* as slaves' names. Secondly, Ekphantides fr. 2. This fragment, corrupt and obscure though it is, certainly said something derogatory about 'Megarian comedy' (why, otherwise, should Aspasius have cited it in a comment on *EN* 1123a 23?), so that emphasis on its 'uncertainty' is beside the point. Breitholtz deals with it by a double plea: (a) 'Megarian' was a general word of derogatory application, = 'cheap', 'coarse', 'tasteless', and (b) in any case, Ekphantides does not imply that Megarian was older than Attic comedy. The latter is true, and the former is defensible and could be true. Breitholtz

demands *proof* (his italics) of the priority of Dorian comedy. But the student of Greek literary origins is seldom in a position to prove anything; he must content himself with pointers, and I am not sure that Breitholtz has shown that *all* the pointers which appear to converge on archaic Dorian comedy really point nowhere at all. Some consideration is owed to the compromise hypothesis that comic drama with spoken dialogue was substantially older both in Attica and in the Doric-speaking regions than the incorporation of comedy in the City Dionysia.

K. J. DOVER.

BACON (HELEN H.) **Barbarians in Greek Tragedy.** New Haven: Yale University Press. 1961. Pp. xii + 201. £2 2s. od.

The author has carried out a useful piece of research with skill and good judgment. She set out to examine, as factually as possible, the references to foreigners in Greek tragedy, in order to discover what use was made by the three tragedians of the knowledge available to them about the barbarian world (physical appearance, costume, culture, religion, and so on). She finds interesting and, I think, valid differences between them. Contrary to a general impression, Aeschylus does not 'orientalise' vaguely or decoratively, but uses available knowledge concretely and realistically, as his dramatic themes demand, so that he appears in the welcome light of a poet in full intellectual control of his art. The impression of a strictly 'Hellenic' Sophocles rejecting the 'oriental' interests of his predecessor results from the subject-matter of the plays preserved and disappears when the Sophoclean fragments are taken into consideration: Sophocles uses 'foreign local colour' even in plays which are relatively late. He seems, however, less addicted than Aeschylus to foreign geography; he avoids fabulous lore, having perhaps a bias towards 'the more scientific criteria of truth set up by Herodotus'. Euripides is in contrast with both his predecessors. What interests him is not the specific details of nationality, but the fact—and the implications—of foreignness. 'Nobly enduring, or wildly and cunningly vengeful, [his barbarians] are the types of the outsider, the exploitable caste, or nation, or sex, of every period. What this theme means in the plays of Euripides is material for a separate study.'

It is to be hoped that Miss Bacon will give us such a study. Although in her present book she has aimed to be factual, fact can often not be separated from interpretation; her discussion of the *Septem* on pp. 37 f. is good literary criticism. Occasionally perhaps, she could have gone a little more deeply into context, even within the limits she sets herself. She cites two passages of Euripides in which the word *βάρβαρος* 'loses all reference to nationality and means only savage, evil, cruel, etc.' (p. 12). This goes too far—farther, it may be, than she really intends. *Tro.* 764–5 contains a deliberate paradox; *Hec.* 1129–31 is spoken by a contemptuous Greek. The reference in both cases is to the intrinsic moral

inferiority attributed to barbarians by Greek convention. An ironic handling of this theme is characteristic of Euripides. It may be noted that, when, more rarely (*cf.* p. 44), it occurs in Aeschylus, it can also be ironical. Aesch. *Suppl.* 760 f. and 962 f. have irony, if (as is probable) the Egyptians defeated the Argives in battle. In so far as the theme is found in the *Agamemnon*, the arrogant assumption of Greek superiority is exposed by the bearing of Cassandra (*cf.* p. 12).

Despite a few misprints, the book is well printed and attractively produced. On p. 30, in the footnote eight lines from the bottom, 'bad with' seems to have dropped out before 'foreign'. On p. 76 the reference to Aristoxenus should be 'ap. Athen. 635e'. (P. 151, n. 33) Though the *Cretans* was probably an early play, it was the *Cretan Women* that was produced in 438.

R. P. WINNINGTON-INGRAM.

LINFORTH (I. M.) **Antigone and Creon.** (University of California Publications in Classical Philology, 15, 5.) Berkeley and Los Angeles: University of California Press. 1961. Pp. 183–260. Price not stated.

This study of Sophocles' *Antigone* is above all concerned with the characters, and I must begin by objecting to the author's view of them. Professor Linforth virtually identifies himself with Antigone. He makes her gentle and affectionate, as well as high-minded and courageous. The other characters he makes creatures of straw. Creon is simply a foolish, tyrannical man, Ismene a silly, despicable coward. Antigone has a universal and exclusive claim on our sympathy throughout. So, on her arrest, she stands 'an object of pity to the chorus and the audience' (this though the chorus plainly speak later, line 801, as if it had only just occurred to them to pity her), and the rashness in the second strophe of the second stasimon is meant really for Creon.

I cannot believe that this gives a fair impression of an audience's reaction to the play (and it is audience-reaction that Linforth claims to recapture). Surely it is the *conflict* of personalities that gives the play its bite (*cf.* G. M. Kirkwood's recent study of this aspect of Sophoclean art). No one in his senses would deny the towering moral stature of Antigone; yet, if the other side is denied significance, the conflict disappears and the play becomes melodrama. Persons who have one virtue developed to an extreme commonly lack others. Sophocles seems to make it perfectly clear that Antigone is harsh and hurtful, at least effectively and for the purpose of these particular dramatic exchanges, to Ismene. On the other hand, he makes Ismene display those very qualities which Linforth denies her and transfers to Antigone—a sense of realism as well as a genuine, self-effacing affection for her sister. And surely Ismene displays a courage of her own in facing Creon? How has Linforth contrived to represent matters otherwise? Partly by imputing to Antigone a great many 'stream-of-consciousness' thoughts which are not in

the Greek, and partly by reading wrong implications into what is. I have space here for only one illustration of this: from 548 *καὶ τίς βίος μοι σοῦ λελειμμένη φίλος*; he concludes that Ismene 'can lament the mere prospect of living unhappily' (my italics).

As he underestimates the conflict of character, so he underestimates the ideological conflict. He never discusses those vague and elusive entities, the Unwritten Laws (merely quoting a non-committal statement about them from Ehrenberg's *Sophocles and Pericles*), but assumes that there is an absolute duty to bury the dead, including Polynices. Creon's conduct therefore is 'reprehensible throughout'. Legally and socially, this is naïve. Even if the law about burying all the dead was absolute in fifth-century Athens (and it is by no means clear that it was), this would not necessarily apply to the dimly-apprehended legendary past as pictured by Sophocles. Creon has a case, and this is what largely makes the drama. What he does suffer from, as Linforth points out far more cogently at the end of the book, is *dysboulia*, i.e. political unwisdom, failure to read correctly and deal appropriately with the particular human situation with which he is faced.

I am sorry to disagree so strongly with Professor Linforth on these points, because on others I admire him greatly. He shows much sensitiveness and perceptiveness about the Greek *as poetry*. He is illuminating on the meaning of individual words (such as *ὀμός* in line 471). He writes in a clear, elegant English, which is always readable, and he encourages one to think about the *Antigone* as literature.

J. H. KELLS.

ROMILLY (J. DE) *L'évolution du pathétique d'Eschyle à Euripide*. Paris: Presses Universitaires de France. 1961. Pp. 148. NFr. 12.

This book is mainly concerned with comparing the way in which Aeschylus and Euripides present certain aspects of their themes, especially their treatment of deeds of violence and their consequences, and the relation between their dramatic habits and their general outlook. The author's main thesis is that whereas in Aeschylus the stress is on action together with the fears and tensions that accompany it, in Euripides what happens is less important because it has no framework of reference and thus no meaning; what matters, or what interests Euripides, is the effect on the minds of those concerned, 'le contre-coup psychologique'. Of the two emotions which Aristotle especially associated with tragedy, fear is more prominent in Aeschylus, pity in Euripides, because in Aeschylus men believe in a divine government and though they fear its anger they can seek to appease the divine powers and conform to their will. In Euripides, however (in the tragedies), men face suffering in which they see no meaning or purpose; they can only endure and be pitied. So in the *Eumenides* Orestes feels terror in face of the Erinyes but reacts against it, seeking purification at Delphi and judgment in Athens. In Euripides Orestes is the helpless, passive victim of bouts of

illness physical and mental, though in this case he does know why he suffers and does wake to feverish energy before the end of the play. In the *Heracles*, however, there is no explanation, other than the purely mythological, for the madness of Heracles, and he awakens to realisation of a dreadful and irrevocable act which he could not possibly have foreseen or avoided, whose consequences he must now endure.

To take one or two further illustrations of the same basic contrast, the author reminds us that in the *Choephoroi* Aeschylus presents on the stage the encounter of Orestes and Clytemnestra, the drawn sword, the bared breast, all but the final stroke, and characteristically includes in this scene the first sign of doubt or hesitation in Orestes. Euripides on the other hand limits the actual murder scene to a line and a half spoken by Clytemnestra within and four lines of comment by the chorus, but continues with a scene of sixty lines full of the mental anguish of Orestes and Electra as they look back on what they have done. Again, spectacular effects are characteristic of both Aeschylus and Euripides, but in Aeschylus they are generally used to emphasise action and conflict, as in the last scene of the *Suppliants*, in the pursuit of Orestes by the Erinyes, in the last scene of the *Prometheus*, whereas in Euripides effects of *ὄπας* are associated more with scenes of helpless lament, as at the end of the *Troades* or when the family of Heracles arrayed for death are before us.

I have given only a specimen of the author's method. The validity of the main argument depends on the cumulative effect of a large number of comparisons, some of which in isolation might be otherwise explained. To appreciate this one must of course study the author's own exposition, which is much better put, more *nuancé*, than any summary could suggest. The contrast between Aeschylus and Euripides is the main point, but there are also references to Sophocles, whom the author regards as in this connexion intermediate between the other two dramatists. She rightly finds some correlation between the differences she is discussing and differences in the *Zeitgeist* of the first half of the fifth century and of its last decades; whether we can speak of a general tendency in Greek drama, as the title of the book suggests, is more doubtful, since so little of Sophocles has survived and practically nothing of the bulk of tragedy apart from the big three.

Not all comparisons put forward are equally illuminating. For instance to illustrate a contrast between Euripides and Sophocles the author compares scenes in the *Trachiniae* and the *Hippolytus* where in each play the hero is exhibited mortally injured and in great agony. She contrasts the pathos and resignation of Hippolytus with the rage and fighting spirit which Heracles even now displays; but this contrast may be due to the pre-established character of Heracles more than to any general characteristic which distinguishes Sophocles from Euripides. The danger of special studies is of course that the writer may tend to see everything in terms

of his particular thesis, but in the present study the author shows in the Introduction her awareness of this and other hazards, and in general her good judgment enables her to avoid them. Sometimes it seems to me that she overstates the relative indifference of Euripides to everything except the suffering of the victims and the extraction from this of the maximum emotional value. It may be right that Euripides was much more interested in the anguish of Heracles than in how or why he was driven to madness (and if so perhaps it is a mistake to examine too minutely difficulties about the role of Hera and what if anything she is supposed to symbolise); but in the *Bacchae* is it true that Euripides' only concern is to provoke pity for the victims, and that if he had been chiefly interested in moral or religious problems he would have dealt with them and his characters would have discussed them? In the choral odes and elsewhere, and indeed in the presentation and shaping of the action of the play, questions about the nature of Dionysiac religion, the meaning of *sophia* and so on are surely raised though no clear answer is given or implied.

There are other points on which some doubt may be felt. For instance to say that in contrast with Euripides Aeschylus 's'intéresse avant tout à un conflit, à une action' is a little one-sided, since it could also be said that Aeschylus' tragic thinking often leads him away from the presentation of a stage conflict. In the *Septem* there is no clash between Eteocles and Polyneices (as there is in the *Phoenissae*), Agamemnon and Clytemnestra meet but the real antagonism is not expressed, and in the *Persae* everything is seen through Persian eyes. The agon is characteristic rather of Euripides and Sophocles. Such scenes of course consist of argument rather than action, but in a way this is true of the encounter of Orestes and Clytemnestra in *Cho.* 885-930, though these tense moments have a more powerful impact than any Euripidean debate, and we see that Orestes has a sharp answer with which he will soon end all argument.

But these and other minor points that might be questioned do not affect my opinion that this is a valuable study, both for the broad sweep of its main argument and for many shrewd comments on matters of detail. It is written with the charm and vigour that we now expect from the author and those interested in Greek tragedy should not miss it.

P. T. STEVENS.

ARISTOPHANES. *Scholia in Aristophanem*. Ed.

W. J. M. Koster. iv. Jo. Tzetzae commentarii in Aristophanem. 1. Prol. et Comm. in Plutum. Ed. L. M. Positano. Pp. cxxviii + 365. 2 plates. 2. Comm. in Nubes. Ed. D. Holwerda. Pp. 367-689. 2 plates. Groningen: J. B. Wolters. 1960. £7 2s. od.

Dr Johnson said that a woman's preaching was like a dog's walking on his hind legs: 'It is not done well; but you are surprised to find it done at all.' It would be too harsh to say the same of the new

edition of Tzetzes' commentaries on Aristophanes, since some parts of it are in fact done very well. Nevertheless, the fact that it has appeared at all remains the most remarkable thing about it. These commentaries have never been published before. They are difficult to edit and difficult to print. Now we have a first part (called 'fasciculus', though the diminutive ending hardly suits these tomes) containing 'Prolegomena' by all three editors and the commentary on *Wealth* edited by Lydia Massa Positano, and a second containing the commentary on *Clouds* edited by D. Holwerda. A third part, which has yet to appear, will contain the commentary on *Frogs* edited by W. J. M. Koster, together with the notes of Tzetzes on *Knights* and *Birds* and indexes to the whole work. This will give us all the writings of Tzetzes on Aristophanes, except for his 'Prolegomena'; these the editors are reserving for a later volume which will contain all 'Prolegomena de comoedia', and anyhow they have already been published elsewhere. The editors and publishers deserve much gratitude from students of Aristophanes for undertaking this massive task.

That said, it must be admitted that there are some aspects of the work to which the first part of Dr Johnson's dictum may be applied. Take the editors' 'Prolegomena'. They include detailed studies of the manuscripts and their mutual relationships, and also discussions of such topics as the dates of the commentaries and their character. They run to 110 pages. A user of the book will seldom want to read straight through this mass of material from beginning to end, but he may often want to refer to discussions of particular points. He will find it difficult. The 110 pages are divided into three chapters; they have the illuminating titles 'Prolegomena I', 'Prolegomena II', and 'Prolegomena III'. How is the material distributed among the three? By subjects? No: a topic like the relationship of manuscripts U and Amb, for example, is discussed in all three. By authors? I thought so at first; but then I found that 'Prolegomena I' was written partly by Koster and partly by Holwerda. I am driven to conclude that the distribution of material among the chapters is dictated merely by chance.

The arrangement of material within the chapters is not much better. The first chapter does contain four sub-headings (which is more than can be said for the other two): 'I. Commentarii apud hodiernos historia', 'II. Commentarii recensiones et aetas', 'III. Exegeseos Tzetzae indoles', and 'IV. Codices'. I forbear to ask why it is thought appropriate to discuss the recensions (that is, the relationship of one manuscript to another) before describing the manuscripts themselves (on which discussion of the recensions must be based), or why generalities about the character of Tzetzes' work should be placed in between; instead I merely point out that the four sections of the chapter do not in fact cover the topics announced in the sub-headings. A reader who wants, for instance, an account of manuscript Lut will not find it in the section headed 'Codices'; he

will find it (if he ever does find it) on pages xciv-xcvii, in the middle of 'Prolegomena II'. The section headed 'Commentarii recensiones . . .' discusses recensions of the commentary on *Frogs* only; 'Prolegomena II' and 'Prolegomena III' must be consulted on the recensions of the commentaries on *Wealth* and *Clouds*. In short, one can find what one wants in the 'Prolegomena' only by plodding through from the beginning until one chances upon it. The indexes, when they are published, may perhaps give some assistance, though I doubt whether it will be possible for even the best index to make the search easy.

But within the maze the 'Prolegomena' contain a great deal of accurate study, ingenious reasoning, and sound judgment. The hardest problem discussed is that of the recensions: did Tzetzes produce only one or two or more successive editions of his commentaries, or how are we to explain the differences, in some places only trivial but in others fundamental, between the versions offered by surviving manuscripts? The problem varies from play to play, and even from one part to another of a single play, and it is discussed separately by the three editors for the commentaries which they respectively edit. All three discussions are admirably careful and thorough, and to me (who, I should make clear, have not personally collated or inspected the manuscripts involved) carry conviction in all essential points. The most ingenious passage of all is the one (pp. cxiii-cxvii) in which Holwerda reconstructs the pagination of Tzetzes' own autograph text of his commentary on *Clouds*. This is a masterly piece of palaeographical detection.

I turn now from the 'Prolegomena' to the text. The difficulties of presenting a text of which different recensions are offered by the manuscripts are satisfactorily solved: where two quite different versions exist, they appear in parallel columns; where a scholium is common to both recensions (or where only one recension exists), it is printed right across the page. Below is the *apparatus criticus*, and beneath that, at the foot of each page, are references to parallel passages in the other Aristophanic scholia and elsewhere. For the commentary on *Wealth*, Massa Positano has also two further sections at the end: one for glosses, and one for 'Animadversiones criticae'. Holwerda, dealing with the commentary on *Clouds*, dispenses with these two sections by including the glosses among the scholia (while distinguishing them by the sign J), and by combining his notes with the references to parallel passages at the foot of each page. Holwerda's arrangement is the better of the two, because with Massa Positano's the reader has to keep his eye on three different pages simultaneously (or four, if he also has before him a text of Aristophanes).

The *apparatus criticus* is selective, not exhaustive; on pp. cx-cxi Massa Positano gives examples of the trivial scribal errors which she omits from it. Nevertheless many very small errors are still included. This policy is the right one for an edition of this

kind, which is likely to be used mainly by specialists. The important thing is to make available to scholars the text offered by the manuscripts, and it is better that scholars should be able to judge for themselves which variant readings are important than that the editors should suppress any evidence which might conceivably be of interest. There are, however, other ways in which the *apparatus* might well have been shortened. For example, Massa Positano uses double brackets (()) in the text to mark words contained in Lut but omitted in Amb, and yet for every such passage she also writes superfluously 'Lut, om. Amb'. Again, on p. 72 appears the note (one of many similar notes): 'θυμός: signum mensurae longae supra v Lut, signa mensurae brevis et longae Amb'. Why not just 'θυμός Lut, θυμός Amb'?

In her footnotes to the text and in her 'Animadversiones criticae' Massa Positano adduces a large quantity of parallel passages and illustrative material. Her industry is laudable, but perhaps she might have done better to omit a few of the less relevant references and include instead rather more explanatory notes and comment of a kind really deserving the epithet 'criticus'. And with all her assiduity she occasionally misses an important reference. For the commentary on *Wealth* 1146, for instance, in connexion with the amnesty of 403 B.C., she mentions (p. 230) *Ath. Pol.* 39.6 and (p. 363) the almost worthless sch. Luc. 44.7, but omits the much more detailed account in Andokides 1.80-99.

Holwerda is a neater editor than Massa Positano, and avoids her prolixity. But this very virtue of neatness sometimes leads him into a different fault: unnecessary endation. em P. 393 line 4, has an example. The manuscripts agree in offering ἡγαρόμην. The imperfect would be more appropriate, and is actually used soon afterwards, and so Holwerda emends to ἡγούμην. But perhaps in his use of tenses Tzetzes was less precise than Holwerda. Massa Positano is superior in this respect; she is more ready to write (e.g. p. 50): 'ne Tzetzam ipsum corrigerem timui'.

Holwerda's *apparatus* records about 225 changes of his own in the text, Massa Positano's about 180. Many of these are very small, sometimes involving no more than a change of accent or the addition of a subscript. But some are of more significance. On p. 194, lines 24-5, for instance, the text has εἶτα ἐγγύτατα δεικνύοντες, εἶτα ἐν τῇ χειρὶ, where εἶτα ἐν τῇ is Massa Positano's emendation of αὐτὸν μὴ. This is excellent. I will mention just two conjectures which seem to me less satisfactory. On p. 3 line 20, Massa Positano's *δυσάρεστον* will hardly do, because it means 'hard to please' (of a person) and we need 'unpleasant' (of a thing); possibly *δυσχερές* (which can have either meaning) may be right. On p. 434 line 1, Holwerda emends ἐν τῇ πολιτείᾳ to πρέπον τῇ πολιτείᾳ. I dislike this, not merely because he has to postulate corruption in four distinct stages (which is an improbably large number, though of course by no means impossible), but

because ἐν τῇ πολιτείᾳ may be right, meaning 'constitutional' (Demosthenes 20.122 has a phrase which is similar, even though not quite an exact parallel).

I look forward to seeing the final part of this edition, and also the future volumes containing other Aristophanic scholia.

DOUGLAS MACDOWELL.

GELZER (THOMAS) **Der Epirrhematische Agon bei Aristophanes: Untersuchungen zur Struktur der attischen alten Komödie.** Pp. xiv + 296. Zetemata, Heft 23: C. H. Beck, München, 1960.

This very careful and full re-examination of one of the recurrent structural patterns in Aristophanic comedy is based on the author's Basel dissertation of 1953. It might, not too fancifully, be said to mark the centenary of its subject; for if the main impetus to studies of structure in Aristophanes came from Zieliński's *Gliederung der altattischen Komödie* of 1885, which is commemorated in much of the terminology now standard, the general modern concept of an epirrhematic agon can be held to originate with Rossbach and Westphal (1856); and the term *agon* itself derives from a suggestion made by Bergk in 1859.

For this study, as for Koerte in *RE*, s.v. 'Komödie' (1921), the canon of the epirrhematic agon is constituted by twelve passages in the extant plays, which are grouped together by criteria based on their form. After an Introduction, and a section of brief general remarks on each of the twelve passages in turn, Gelzer proceeds to a hundred pages of analysis and discussion which give a valuable and generously detailed account of their many points of similarity and variation, and examine the form, as a whole and in its parts, in terms of basic pattern, content (i.e. themes, arrangement, style), and dramatic function. This part of the book, though sometimes slow-moving to a degree, makes a well-balanced distinction between observation and inference, and alongside much that is obvious or commonly known, it brings out much that is neither. There appear from the outset two sub-groups of epirrhematic agon. By comparison with the analyses in Pickard-Cambridge, *Dithyramb* . . . (now available in a revised edition by T. B. L. Webster, 1962), Gelzer's major sub-group of nine comprises 'Agon II' of *Knights*, 'Agon I' and 'Agon II' of *Clouds*, and those of *Wasps* (526-727), *Birds*, *Lys.* (not 1014-42), and *Frogs*, together with the 'Half-Agons' of *Ecll.* and *Plut.*; it is called 'Agon in the *Diallage*', being regarded as akin to a process of arbitration (pp. 47-72). The other three are *Knights* 303-460 ('Agon I'), together with *Wasps* 334-402 and *Birds* 327-99, which Pickard-Cambridge analyses differently: these are distinguished as 'Agon after the Parodos', not without some reservations about *Knights* (p. 72). From this limited but complex material, generalisation is sometimes desperately difficult, and inevitably, on returning to the texts, one pauses to wonder how often the constructions

made are asking too much of them. This consideration recurs apropos of the next forty pages, which first consider a series of passages regarded as analogous to the epirrhematic agon in form or content (but excluded from the canon), and then discuss the three plays without a canonical epirrhematic agon at all, *Ach.*, *Peace*, *Thes.* Pp. 138-51 argue at length that *Clouds* 314-477 is made up from rearranged remnants of a 'diallage-agon' from the first version of the play. This goes far beyond the evidence, and is open to serious objections: but in general, if *Ach.* and *Peace*, the plays of 425 and 421, are considered untrue to type as a result of special circumstances, other special circumstances can be thought of with reference to the first part of the *Clouds* of 423. A dramatist may exploit a pattern repeatedly without feeling rigorously bound to it; and this, in some sense, is certainly what Aristophanes does.

The last part of the book (pp. 179-288) is rather less fully documented and worked out, and professedly more speculative, since it attempts in effect to give the study a wider context by tracing the history of Comedy with special reference to the agon; it is here that the limited evidence for structure from the fragments is invoked, and it is here too that the lack of extensive pre-Aristophanic texts makes itself felt: cf. K. J. Dover, in *Fifty Years of Classical Scholarship* (ed. Platnauer), p. 112. Gelzer recognises the difficulties of arguing back from Aristophanes, but nevertheless does so, arriving at a *brauchtmäßiges Diallage-Spiel*, consisting of parodos, epirrhematic agon and exodos (in which the victor of the agon was prone to go off in marriage-revel with a *Baálēua*); this somehow fused with two other distinct elements, a kind of choral performance corresponding to the anapaests and syzygy of the parabasis, and an actors' performance corresponding to the prologue (?) and episodic scenes. The treatment of the evidence for early comic performances strikes me as one of the less satisfactory things in the book, and is marred by avoidable errors and misconceptions (e.g. on early padded dancers and on *phylakes*); but in this field some further work is promised (p. 230, n. 2). I am considerably happier with pp. 238-76, a general essay on developments of the agon in Aristophanes, which says some good things well, and effectively reiterates some of the points made earlier from a different viewpoint.

In matters of detail and bibliographical documentation, there is again much that is good, even though in a work of this scale there are bound to be remarks where additions, subtractions and qualifications seem necessary. I mention some examples: p. 21, the note on bird dances (after Lawler) should also quote Fraenkel in *Eranos* 48 (1950) at p. 83; p. 80, in referring to *Frogs* 905 f., should mention the arrangement of speakers in the MSS.; p. 163 n. 2, on *Frogs* 1309 ff.: see also A. M. Dale, *Lyric Metres*, 146 f. (this book is lacking from the bibliography). I do not see what τὸν χολοποιόν in 846 or χολὸν ποιῆς in *Ach.* 411 have to do with metre. P. 173, the note

on the plot of Eur. *Telephus* (after Miller), requires modification in the light of the new fragments referred to, and now republished by Rea, *P. Oxy.* 27. 2460; p. 285 ff., on the Arbitration Scene in Menander, *Epit.*, could well quote Quintilian, *Inst. Or.* x 1.70. There are three efficient indexes; the few slips of pen or print I noticed were trivial, including 'Philokleon' for 'Strepsiades' (p. 102, last para.), and the misplacing of the first two lines of p. 288 at the foot of the block of text. [A corrected page has since been supplied for the review copy.]

E. W. HANDLEY.

ARNOTT (P.) **Greek Scenic Conventions in the Fifth Century B.C.** Oxford: the Clarendon Press. 1962. Pp. xii + 147. £1 7s. 6d.

Conclusive answers to the problems of the form and function of the classical theatre can only be expected if the approach is made from many angles. Therefore, the contribution of scholars who have enriched their methods through the experience gained in 'the hard school of practical production' is indeed invaluable, provided that they are not misled by exclusively modern stage notions into adopting principles inapplicable to the theatrical reality of the fifth century B.C.

The promise of this 'combined' method, explicitly stated in the preface, is conscientiously kept by the author throughout this book, which goes much further than his *Introduction to the Greek Theatre*, in so far as it is a *specialist's* work mainly addressed to a public with a fairly advanced knowledge of the subject. A great deal is taken for granted; many details are simply hinted at: the author does not aim at giving us a detailed picture of the theatre at a certain stage of its development. As he is primarily concerned with the production of the plays, it is very reasonable that he should only deal with those aspects of the whole question which might, in his judgment, have a bearing on the performance: raised platform, permanent stage properties, mechanical devices, scenery, all of which have given rise to interminable arguments among scholars during the last eighty years.

The problem of whether or not a raised platform existed in the fifth century is fully discussed in the first two chapters. A. adopts the view that a stage of moderate height existed even as early as Aeschylus. This assumption has been unjustifiably rejected by many scholars, including Dörpfeld and Pickard-Cambridge, who overlooked its merits; for such a platform would in fact have provided a conspicuous place for the actors, as the histrionic element grew more and more predominant in the performance, without hindering the free intermingling between chorus and actors, which, as the author points out, is confined to a few rather exceptional cases. The tradition, whose unanimity he claims for this view, hardly amounts to more than a few ambiguous passages found in very late sources, and the archaeological evidence does not offer sufficient support. We are, therefore, on much safer ground when the texts, on thorough examination, yield reinforcing

indications, viz. (a) allusions pointing to some kind of *elevation* in the scene of action; (b) chorus' and actors' movements accounted for only by the presence of a raised stage.

It is mainly on the same kind of evidence that A. bases his theory (third and fourth chapters) that the altar, so persistently recurring in many plays, was a permanent stage property. It should be noted, however, that: (a) many of the extant plays *do not* require such a property; (b) those which need one could not be easily staged if the altar always occupied the same position, i.e. before the door; (c) sometimes it was only by adding such movable objects that elementary changes of scenery could be made. Besides, why should a wooden altar, which was never used for ritual purposes, be left there to obstruct, e.g. the opening of Philoctetes' cave? Because, as A. maintains (sixth chapter), all the plays, no matter what scenic decoration is suggested by their scene of action, were enacted in front of an unchangeable background. The Greek theatre of the fifth century was highly conventional; the tendency towards illusion and spectacle was typical of later times, when the responsibility for the performance was transferred from the poet to the interpreter. The Elizabethan and Japanese theatre are constantly used as sources of arguments in support of this view. But, quite apart from the fact that a close comparison between, e.g., Euripides and Shakespeare, would reveal many obvious differences in plot construction and in the exploitation of time and, above all, space possibilities, if this extreme attitude about scenic conventions were pressed too hard, A.'s line of approach to the whole question would suffer from a serious inconsistency: when the spoken word is given the absolute power of transforming an unchangeable background at any given moment, by merely appealing to the audience's imagination, how could the texts be used as sources of information? The author, however, taking as his basis the reasonable assumption that no ancient playwright-producer could ever pose himself an insoluble problem, and using *exclusively* text indications, attempts many interesting interpretations of crucial scenes—three appendices are wholly devoted to the most problematic of these. And here lies the chief value of the book.

But it is for this reason, perhaps, that a striking carelessness in argumentation here and there must be pointed out. P. 22, e.g. we are asked to believe that the chorus of Aeschylus' *Suppliants*, consisting of fifty members, is performing out of the orchestra for nearly one-third of the play (208–508), whereas p. 40 it is stated that, if the chorus 'have to leave the orchestra, they must return to it as quickly as possible'; pp. 33–4, owing to an apparent misinterpretation of *Wasps* 1342 ff. it is implied that the official flute-player, presumably dressed as hetaira, plays the part of a mute, although verses 1345 and 1368 ff. clearly indicate that the auletris came from the banquet in the house; p. 38 we read that Iphigeneia in *Iphigeneia in Tauris* 1068 ff. 'supplicates some

persons unspecified' who 'may have been the attendants who brought on Orestes and Pylades', and all this in complete disregard of the text which informs us that it is the women of the chorus that she is beseeching (cf. in particular verses 1056-7, 1061, 1071-2 and 1075 ff.); p. 52 the altar in the scene of action of the *Ion* is called 'inessential but appropriate', whereas one of the most exciting scenes of the play, the one leading to the anagnorisis (1250-1405), is largely enacted round the altar; p. 43 an approximate estimate of the height of the scene building is based on the fact (?) that 'two scenes in Euripides call for an actor to leap from the roof to the ground', i.e. the Phrygian in the *Orestes* and Eudæne in the *Suppliants*; yet the former scene is most unlikely to have been performed in such a way and in the latter the actor could not have leapt to the ground, simply because there was no 'ground' behind the scene. There are many arguments in the book which are marred by mistakes of this sort.

N. HOURMOUZIADIS.

STOESSL (F.) **Personenwechsel in Menanders Dyskolos.** (Sitz. österreich. Akad., phil.-hist. Klasse, 234, 5.) Vienna: R. M. Rohrer. 1960. Pp. 91. ö.S. 60.

In recent articles on the *Electra* (*Rh. Mus.*, ic 1956, 47-92) and the *Heraclidae* (*Philol.*, c. 1956, 207-34) of Euripides, Stoessl has put forward the view that our manuscripts faithfully preserve the poet's own attributions of the speeches to the individual characters. The present work, whose format closely resembles that of the *Electra* article, makes a similar claim for the *Dyscolus* papyrus. St. presents his main argument in the opening section of his book: in the papyrus, he alleges, the method used to indicate speaker and change of speaker (*paragraphi*, double points, and occasional marginal or interlinear indications of speakers' names) is a systematic one, the play for this purpose being divided into some forty-six 'dialogue units' in which a fixed number of from one to three characters participate. At the beginning of each unit the names of the participants are normally indicated on the papyrus, the subsequent changes of speaker being marked only by *paragraphi* and double points. According to St., this system goes back to Menander's own autograph of the play, which has been copied with surprising accuracy by a succession of scribes down to the extant third-century Bodmer papyrus.

St.'s conclusions run counter to the views generally accepted today about the textual tradition of both Menander and Euripides,¹ although there is perhaps something to be said for the theory that the method of speaker identification used in the

Bodmer papyrus goes back beyond the Alexandrian editions to the time of new comedy at least (cf. M. Pope, *Acta Classica*, iii 1960, 40 ff., citing papyrological evidence that St. ignores). But even if we allow that this method is as old as Menander, it has first to be shown that the generations of scribes between Menander and the Bodmer papyrus have introduced no (or practically no) errors in copying down the *paragraphi* and double points, before we can accept the speaker indications in our papyrus as authentic. In fact, although St. frequently claims that scribal errors of this kind are surprisingly rare in the *Dyscolus*, his claims go beyond the facts. For instance, St. fails to notice that the postulated scribal accuracy falls off badly towards the end of the play: in the last two hundred lines we must posit at least twelve errors of ascription or speech division. Thus the papyrus is no more infallible a guide to this aspect of the tradition than it is to the text of the play itself, and one of the main props for St.'s arguments is shown to be faulty.

Nor are these arguments themselves always completely convincing. St. maintains that the identification of the character who enters with the sheep in Act II as Σίκων μαγειρός could have been made only by Menander and not by any later scholar. In this, he seriously underestimates the intelligence of Alexandria. There are several motifs in the *Dyscolus* which Menander repeats in other plays (e.g. the joke at 438-9 = Plautus (Menander), *Aul.*, 567-8: on the close relationship between these two plays see especially W. Ludwig, *Philol.*, cv 1961, 247 ff. and W. Kraus, *Serta Phil. Aenip.* 1962, 185 ff.), and it is unlikely that Menander's *Dyscolus* was the only new-comedy play where the cook entered with a sheep; thus the scholars of Alexandria had only to compare other parallel scenes in order to argue that in the *Dyscolus* also the man who brought the sheep was the cook, even if they were not intelligent enough to take the hint from 263 f. St. uses this same argument when discussing the identification of Chaereas as ο παραστροφός in the character list (but see Lloyd-Jones' edition, *ad loc.*) and of Simice as γρᾶς in the margin at 574 ('hardly the work of a scholar or copyist', St., but the description γρᾶς is an obvious one, cf. 587).

In fact, all that St.'s arguments prove is that the person responsible for the method of character indication was reasonably intelligent and fairly consistent—not absolutely consistent, for imperfections in the alleged system reveal themselves. At 249, for instance, we find Γοργὴ written in the margin, but the characters who have spoken 233-48 continue speaking: it is special pleading on St.'s part here to say that a new 'dialogue unit' must begin at 249. The same is true of the marginal name appended to 419: this does not mark the beginning of a new 'unit', as St. claims, but was presumably added just to help along the reader of the papyrus. At times, too, St.'s theories would lead us to expect marginal names at places in the Bodmer papyrus where they do not in fact occur:

¹ Cf. (for Menander) A. Gudeman, *Grundriss zur Geschichte der klassischen Philologie*,² 38 n. 2, J. Andrieu, *Le Dialogue antique*, 93 ff., R. Cantarella, *RIL* xciii 1959, 82, and the *Dyscolus* editions (e.g. Kraus, 12 f., J. Martin, 19); (for Euripides) D. L. Page's edition of the *Medea*, pp. xxxvii ff.

666 is a case in point. Here Sostratus enters to deliver his monologue about Cnemon's rescue, and accordingly St. posits the beginning of a new 'dialogue unit', but Sostratus' name is absent from the margin. St. finds this difficult to explain; drawing attention to a dark shadow which appears according to the photograph on the left margin of the papyrus at this point, he asks whether this may indicate an erasure of Sostratus' name here. Of course, a certain decision could not be reached without investigation of the papyrus itself;² but why should we thus be led to assume a completely senseless erasure rather than adopt what seems to be the likeliest explanation: a stupid omission on the part of a copyist?

Yet even though St.'s basic theories may be challenged, the value of his book is only partially affected thereby. The larger part of the work is a running commentary on the play, with particular but not exclusive attention paid to the distribution of the speeches and identification of the speakers. This commentary contains much interesting and often valuable illustrative and interpretative material. St.'s eagerness to accept the division and attribution of the speeches as indicated in the papyrus enables him to show that many scholars (including the *ed. princ.*) have been too ready to neglect the directions of the papyrus without sufficient reason: I single out St.'s comments on 102 (ὡς ὁρῶντες); 203 ff. (correcting V. Martin; St.'s suggestion is now *communis opinio*, but it is important to remember that it is only because of such arguments as St.'s here that general agreement has been reached); 363-5 (given to Gorgias, against Post and Lloyd-Jones); 375 ff. (a vindication of St.'s methodology); 503 (with Diano and Zuntz); 556 (with Thierfelder and Turner); and 617-19 (where St. suggests, with Kraus, that Davus enters Gorgias' house while Gorgias and Sostratus enter the shrine). Of course, given the particular difficulties associated with this papyrus, it is inevitable that some of St.'s interpretations will appear wrong. In 138-44, for instance, many scholars will not agree with St. when he denies the existence of a double point after *βελτιστε* in the Bodmer papyrus, or when he is led by his theories to give 142-3 to Pyrrhios. One doubts also St.'s interpretations of 177-8 (the conventional view of Gallavotti, Turner and the Australians seems preferable), 301-2 (see van Groningen), 430 ff. (Ritchie's theory of a speaking part for Sostratus' mother requires one to assume a basic omission of information on the part of the papyrus, and perhaps for this reason is too easily rejected by St.).

² But we ought to bear in mind the late P. W. Harsh's comments on shadows and the like in the photographs of the Bodmer papyrus: 'useless dots of ink, sometimes flecks . . . occur throughout the manuscript . . . sometimes a dot appears in the photograph where there is nothing remarkable in the papyrus' (*Gnomon*, xxxi 1959, 579).

In most of the above places, the distribution of speeches and the identity of the speakers have been cardinal factors in the argument. Appended is a selection of passages where St.'s interpretations may be challenged on other grounds. 142: *ἐκλεπτον* is surely first person. P. 29, n. 74: can one assume from the frequency of his oaths that Sostratus is a very pious young man? 353-4: the implausibility of the supplements is indicated by the tortuousness of the arguments sustaining them. 552: St. (with Barigazzi) explains Getas' failure to recognise his master as due to the fact that Sostratus is still wearing the clothes of a farm labourer; more plausible explanations are Getas' steadfast refusal to look in the right direction (comic delaying tactics: cf. Duckworth, *Nature of Roman Comedy*, 112) and the smoke in Getas' eyes (Post, *AJP* lxxxii 1961, 103). 599-600: there is no question of malice: the offer is a genuine one. 637, 660-2: St.'s theory that Gorgias' cry (637) induces some of the women in the shrine to mill out on to the stage is based on a misinterpretation of 660-2, where Sicon is speaking back into the shrine. P. 74, n. 265: perhaps the proposition about Gorgias' marriage was adumbrated in the missing verses 756-60, but Gorgias could not have agreed to it there, or else the repudiation at 822 ff. would be nonsensical.

This book seems to have been produced in a hurry, as is indicated partly by the large number of misprints, not all of which are noted in the *corrigenda*. And yet, despite all the imperfections alleged in this review, I have no doubt that St.'s *Personenwechsel* will act as a very useful *ολοστρος*, stimulating all students of the *Dyscolus* to reconsider their own cherished theories.

W. G. ARNOTT.

MENANDER. **The Bad-Tempered Man, or The Misanthrope.** Trans. P. Vellacott. London: Oxford University Press. 1960. Pp. xxi + 50. 10s. 6d.

One happy result of the discovery of the *Dyskolos* is that it is now possible to produce on the stage a complete play of Menander more or less as he wrote it and, through the media of translations, to introduce to a wider public the most celebrated comic poet of antiquity. This translation by Philip Vellacott, already well known as the translator of Aeschylus and Euripides for the Penguin Classics, was commissioned by the B.B.C. for a broadcast performance of the play on the Third Programme in October 1959.

It is based mainly on the Oxford Text of the play, as Vellacott explains in an interesting introduction where perhaps the excellence of what is largely a producer's play is underestimated. However, on one or two occasions the translator makes conjectures or interpretations of his own; these may be dramatically more effective, but they are not always what Menander intended. Thus Vellacott names the daughter of Cnemon Myrrhine, although scholars

are pretty well agreed that Myrrhine is Cnemon's wife, not his daughter (pedants will find the translator's defence of this renaming on p. xiv distinctly odd); and Simice is introduced into the final scene as a participant in the ragging of Cnemon, although this would require four speaking parts.

The translation itself is done in free verse, the length of the lines very roughly corresponding to the original. The style is perhaps correctly and in the best sense prosaic, but illuminated by flashes of wit (e.g. 'state of holy acrimony', 17). Occasionally one feels that a rendering is stilted, or out of touch with modern idiom. This is especially true of the oaths and invective: 'to the crows' (432), 'you damned offal' (460), and 'plague rot you' (510) are not the expressions that one would expect the modern counterparts of Menander's characters to use. But the following passage (490-8) will give a fair indication of this translation's many merits and occasional weaknesses.

SICON: I go and cook for thousands of people up in town;

I pester their neighbours, borrowing saucepans right and left.

If you want anything, you've got to butter 'em up. Suppose

An old man answers the door, I say, 'How are you, Dad?'

Or, 'Grandad'; if it's a hag, I call her Ma; or if

She's middle-aged, 'Priestess'; a kitchen-maid I call housekeeper.

But you—you're only fit for the rope: you're ignorant.

Inaccuracies of rendering seem to be very few indeed: perhaps one should note 288, where 'crime' misses the overtones of *ἀτοπον*; 320, 'for the chance of that' turns an indefinite into a final clause; and 675, where the slang use of *χρησσοῦς* is not understood. At 425, 'you was' deserves castigation: there is no bad grammar in the Greek; and the pointed remarks in 245-6 and 644-7 could have been turned more incisively. But these are small flaws in a workmanlike version.

I have noted only one misprint: on p. 21, two lines from the bottom, the semi-colon should precede, not follow 'get'.

W. G. ARNOTT.

KAKRIDIS (J. T.) **Der thukydideische Epitaphios: ein stilistischer Kommentar.** (Zetemata, 26.) Munich: C. H. Beck. 1961. Pp. xii + 119. DM 14.80.

This is a revised version of a work which appeared in Modern Greek in 1941. Hitherto it has been known mainly through the references made to it by Gomme in his Commentary. It consists of a detailed study of Thucydides' style in the *Epitaphios*, from which it is claimed that certain conclusions can

be drawn about the nature of the speech and the date of its composition.

Professor Kakridis' stylistic analysis is delicate and perceptive. He succeeds in showing that Thucydides' use of ornamental figures is far more subtle than is generally realised, and that the form of this speech adds emphasis and meaning to its content. Word-order, word-length, rhythm, sound, syntax, etc., are all instruments used by Thucydides with infinite skill for this purpose. Kakridis' chapter by chapter analysis on these lines makes fascinating reading, and a variety of interesting points emerge. He brings to light, for instance, some highly complex interlacing of stylistic figures; a good example of this is shown in 39.4. I think he is inclined to overstress the connexion between form and meaning and, perhaps, to overestimate its significance. Nevertheless, his original and sensitive treatment for the most part carries conviction and makes a significant contribution to the study of Thucydides' style.

Far less acceptable are some of the general conclusions which Kakridis draws about the speech. He believes that the *Epitaphios* was written by Thucydides as an *apologia* for Pericles after the end of the Peloponnesian War, and that it was addressed to the disillusioned and embittered post-war Athenians. Hence he takes τῶν λειπομένων in 41.5 to refer, not to Pericles' audience of 431, but to the citizens of Athens about 400 and their reaction against Pericles' foreign policy. The arguments made in support of this theory seem to me weak and unconvincing. Kakridis claims that the *φθόνος* mentioned in 35.2 would be inappropriately ascribed to Pericles' audience who would all be *εἰνοῖ*, but that it would naturally reflect the bitter feelings of the Athenians after their defeat. This seems fanciful. The sentence in question is merely one of many statements in Thucydidean speeches which have a general rather than a particular significance. Again, Kakridis' deduction that the dead are made to appear as defeated in 43.1 is unwarranted. This is based on the theory that ἀσφαλεστέραν must refer to the military failure of Athens, rather than to the personal fate of the fallen. The reasons given for deducing this seem to me entirely inadequate. In these as in other points of interpretation Kakridis over-elaborates and sees subtleties of meaning which are not in the Greek.

Kakridis is no doubt right when he argues that the speech is Thucydidean rather than Periclean. He may also be right in believing with Schwartz and others that it was composed after the end of the Peloponnesian War, although I do not think that he provides any new evidence of importance to prove this. What I find entirely unconvincing is his theory that the *Epitaphios* was a sort of political tract addressed to the Athenians embittered by their defeat.

It seems to me a pity that Kakridis tried to combine his admirable stylistic study with a consideration of these larger questions to which, as far as I can see, it has little relevance.

H. LL. HUDSON-WILLIAMS.

KNOCH (W.) **Die Strafbestimmungen in Platons Nomoi.** (Klassisch-philologische Studien, 23.) Wiesbaden: O. Harrassowitz. 1960. Pp. x + 170. DM 17.

This little book performs the useful service of bringing some order into chaos. Part one elucidates the political principles of the *Laws* that affect the legislation; parts two and three present (in German only) the entire penal code of the *Laws*, book by book and crime by crime, with explanatory comment; part four then collects and discusses the penalties according to category—corporal punishment, imprisonment, etc. This is all admirable: anything that arranges the penal code of the *Laws* in an intelligible fashion is to be welcomed.

But is the book anything more than codification? Knoch indeed claims that the historical background to the *Laws* has had enough attention (pp. 22 ff.) and that Plato's penal code now needs thoroughgoing examination in the light of Plato's own political principles rather than against the background of contemporary practice. This sounds reasonable enough; but the results seem curiously meagre. Do we really need to be told, after a discussion of life-imprisonment for heresy, that this penalty shows how important religion is in the life of the state (p. 146)? This must surely be abundantly clear to the least attentive reader of the *Laws*. The 'Schlusswort' contains only a glimpse of the obvious (p. 161): '[die Strafbestimmungen] weitgehend aus Platons eigenem philosophischen Denken erklärt werden können und müssen'. We should indeed consider it a paradox if the case were otherwise, and in any case it is explicit on every page of the *Laws*. But what is explicit needs no explanation; what does need explanation in Plato's penal code are those features which are original to it—that is, where Plato has not adopted, but significantly adapted, contemporary law. About these points Plato is seldom explicit; but they are precisely those which are most likely to be illuminating, for when Plato goes to the trouble of re-writing Greek law, we can be sure something important is at stake. It is of course true that the gaps in our knowledge of Attic law are huge, but when anything is known, we cannot afford to ignore it. Yet Knoch, as a matter of policy, consistently does so. Something can be gained, by way of clarity of arrangement, by confining one's attention to the *Laws*, but much is lost.

Knoch makes two elementary analyses of the penal code—by crime and by punishment. These obviously have their uses, but they are limited ones. Properly to appraise Plato's legislation would require a great many different approaches to the criminal code: what penalties, for instance, are associated with what particular crimes? What determines whether a penalty is subject to appeal? What determines whether a foreigner shall receive a lighter or a heavier punishment than a citizen? Many such questions need to be answered, and they all require a different arrangement and analysis of the material. Such topics are indeed glanced at in this book, but are never treated systematically.

Readers would be wise to treat with reserve Knoch's translations in parts two and three: his own glosses sometimes creep in (e.g. p. 55 n. 5). It is best to turn straight from the reference to the Greek text. The comments appended to the translations can be treated lightly: they contain a large element of paraphrase.

This book, then, codifies rather than analyses, but the codification has been thoroughly and conscientiously done. One could register disagreement on a number of small points, but it is difficult to point to actual errors. The work is likely to be most useful as a reference book; it contains the preliminary sorting and arrangement necessary for those who may wish to penetrate more deeply into the complexities of Plato's later thought.

TREVOR J. SAUNDERS.

DEMETRIUS. **A Greek Critic: Demetrius on Style.** Ed. G. M. A. Grube. (The Phoenix, supplementary vol. iv.) Toronto: University Press (London: Oxford U.P.). 1961. Pp. ix + 171. £2.

Professor Grube, who during the past decade has made a number of contributions to the study of Greek Rhetoric and Literary Criticism, including a rendering of 'Longinus' (New York, 1957), has now produced a more substantial work in this annotated translation (with full-scale Introduction and two Appendices) of the treatise of Demetrius, *On Style*. The volume is the fourth in the series published under the auspices of *The Phoenix* and inaugurated in 1952 by *Studies in Honour of Gilbert Norwood*. It is intended to serve a double purpose, to introduce the essay of this sensible, and often sensitive, Greek critic to a wider circle of classical and non-classical readers, and at the same time to incorporate the results of the author's investigations into the complex problem of its date. The general reader will find this a serviceable translation, with good notes (including the original Greek of many of Demetrius' quotations) neatly and conveniently placed at the foot of the page—though it may be regretted that, perhaps through considerations of space, Professor Grube has not included any of those apt illustrations from English poetry and prose which distinguished the earlier commentary of W. Rhys Roberts. Useful, also, to the general reader will be the lucid account of ancient criticism (pp. 1–22), which brings out the difference between the philosophical and the rhetorical approach and the salient characteristics of individual critics. But the classical specialist who is familiar with the treatise will be particularly interested in the discussion of its date (pp. 39–56), and the new examination of its technical terminology and linguistic usage (Appendix I).

It has been the almost unanimous opinion of scholars for the past eighty years that this is not the work of the Phalerean. With this opinion the author agrees, but he argues in favour of a date far earlier than most of the various dates which have been proposed. It is true that Wilhelm Kroll (who,

incidentally, is twice wrongly referred to as F. Kroll) considered that it might have been written c. 200 B.C., but he admitted that some of the arguments for a much later date could not be lightly dismissed (P.-W., *RE Suppl.* 7, *Rhetorik*, col. 1079-1080). C. Hammer had placed it c. 100 B.C. (*Progr.*, Landshut, 1883), but there has long been a strong body of opinion which attributes it to the Roman imperial period. The work of late nineteenth-century dissertation-writers who took this view filtered down into the edition of Rhys Roberts (Cambridge, 1902), and L. Radermacher, in his edition (Leipzig, 1901), had already reinforced it with new and independent arguments. Grube is very critical of this opinion, and maintains that the treatise is a product of early Alexandrian scholarship, composed c. 270 B.C.

He examines the arguments drawn from allusions to persons and other allegedly 'late' references and decides that they are not cogent (pp. 39-46). He argues that the manner in which Demetrius adapts and develops Aristotelian doctrine shows that he must have used Aristotle directly (with occasional supplements from Theophrastus) rather than through some intermediate source (pp. 32-9). But, in the reviewer's opinion, he devotes too little space to proving his contention that the presentation by Demetrius of a fourfold classification of style is appropriate to the immediately post-Aristotelian era (pp. 50-2), especially as he has elsewhere rejected the Theophrastean origin of the three 'types' (*AJP* 73 [1952], pp. 251-67). On the other hand, some important observations are made on rhetorical terminology. He rightly remarks that even as early as the fourth century B.C., this was quite highly developed. He cites, for instance (p. 8, n. 6), examples from Plato's *Phaedrus* (255-67) and, most important, in Appendix II, studies the *Rhetorica ad Alexandrum* carefully with a view to showing that, if the date of this treatise were not already fairly established, much of its terminology might well strike a reader as late. Fortified with a better Lexicon (L.S.⁹) than his predecessors, Grube is also more prepared to look for cognate forms which existed in the classical period. This is a sound approach, and, even though at some points his work contains an element of special pleading, the upshot is that in future the evidence drawn from terminology will need to be more carefully weighed. There seems, nevertheless, to be a serious omission in the discussion (p. 150) of the term *λόγιος*. Many scholars (including Rhys Roberts, p. 55 and especially p. 290) have drawn attention to the rather striking similarity of Demetrius 38 ἀρξομαι δὲ ἀπὸ τοῦ μεγαλοπρεποῦς (sc. *χαρακτήρος*), ὅνπερ νῦν λόγιον ὀνομάζουσιν and Phrynichus, p. 198 Lob.: *λόγιος* ὡς οἱ πολλοὶ λέγουσιν ἐπὶ τοῦ δεινοῦ εἰπεῖν καὶ ὑψηλοῦ οὐ τιθέασιν οἱ ὀρχαῖοι κ.τ.λ. It was this section of Demetrius which led Emil Orth (who had published a German translation in 1923) to devote a substantial monograph to the term (*Logios*, Leipzig, 1926). Whilst Orth would not claim (pp. 34 ff.)

that the Demetrius passage enables us to pinpoint his date (which he regards, however, as 'lange nach Aristoteles'), he collects the fullest evidence on its relative frequency at different periods and its various meanings. It would seem hardly possible to reconcile Orth's results with the claim that Demetrius was writing as early as 270 B.C. But, although Grube's dating is by no means free from difficulties, it should be acknowledged that his book contains a good deal of independent thought and is a contribution to the study of Demetrius which subsequent scholars will not be able to ignore.

One small point in conclusion: with regard to p. 22, n. 26, it should perhaps be observed, in fairness to Rhys Roberts, that his smaller edition of Demetrius, which forms part of a handy volume in the Loeb series, was not merely a 'reissue' of his original text and translation; apart from writing a fresh Introduction, he revised his previous work, and, whilst necessarily omitting much of his major commentary, introduced some notes which were quite new, and brought his Bibliography up to date (1927).

S. F. BONNER.

Lucian. Trans. M. D. Macleod. Vol. vii. (Loeb.) London: W. Heinemann. 1961. Pp. xi + 476. 18s.

The text provided in this volume is a great improvement on any that was previously available. In the Dialogues of the Courtesans it mainly follows the edition of K. Mraz (1930): in the Dialogues of the Dead, of the Sea-gods and of the Gods it is based on Mr Macleod's own collations of *I*, *B* and *Q* and makes use of some unpublished work of Nilén. In these three groups of dialogues the order of *I* is restored, and makes Lucian's incorporation of earlier material more apparent. For example, five of the *D. mort.* which deal in rather a pedestrian way with inheritance are in *I* and this edition numbered consecutively 15 to 19: the traditional numbering is 26, 11, 7, 5, 27.

The conjecture *αἰεὶ* for *αἰεὶ* in *D. mort.* 15 (346) neatly restores a Homeric reference, and *Ἀκαπρέως* for *Ἀκαπράνιος* in *D. mer.* 7 (298) is the most satisfactory solution proposed for this passage, though the *φασί* is awkward. Rothstein's conjecture, quoted by Macleod, accounts for the *φασί* but expresses a sentiment inconsistent with the speaker's other remarks in the dialogue. *Ἄν* appears with the future in *D. mar.* 11 (306) *ἔσ' ἂν διανήξεται* and *D. mer.* 4 (288) *ἀποσώσει γὰρ ἂν*. These examples seem less certain than those collected in the author's article in *CQ* vi (1956) in spite of the manuscript authority. In the second case *ἀλλ' εἰ τινα εὐρομεν* precedes and both passages would be subject to phonetic confusion (unaccented *ε* with *η* and *ει* with *ειε*).

Introductions and footnotes are concise and helpful, giving all the explanation that most readers will require of such matters as the 'horns' and the 'crocodile', mythology, art and references to other authors. There are longer discussions of Lycurgus

(*D. deorum* 22), *Adrasteia/Nemesis* (*D. mer.* 6) and *Μεγαρεός* (*D. mer.* 15). The pun on the name *Dromo* (*D. mer.* 10, footnote) raises the question whether other names such as *Chenidas* and *Philostratus*, which have meaning in the New Comedy manner, might also be better explained.

There are a few misprints, mostly in the English but unlikely to mislead anyone.

Ten of the dialogues in this book were omitted, *pudoris causa*, from the admirable translation of the Fowler brothers (1905). Mr Macleod copes tactfully with all difficulties of subject-matter and his version is accurate and vigorous, with language beautifully accommodated to character. E.g. *D. deorum* 9 (217) Zeus: *Εὖ γε ὁ κατάρατος· ἐπ' ἐμὲ αὐτόν* . . . (Fowler) 'Whew! I have a rival, I find'. (Macleod) 'The enterprising old devil! Supplanting me!' *D. deorum* 19 (232) Aphrodite: *ὥστε πολλάκις ἀπειλήσα* . . . (Fowler) 'I have told him many a time that if he would not behave himself I would break his artillery for him.' (Macleod) 'So I've threatened him time and again, if he doesn't stop it, I'll smash his archery set.'

Two minor points: *D. deorum* 22 (249) *τοῦ Διονύσου τὰ κάλλιστα* is surely 'D's achievements' rather than 'all that's best in D.': and *D. mort.* 16 (347) why 'Methuselah'? If a proper name is needed, *Tithonus* is at hand in the next dialogue.

B. J. SIMS.

PLUTARCH. *Diatriba isiacae e dialoghi delfici.*

Ed. and trans. V. Cilento. (Iside e Osiride. La E delfica. I responsi della Pizia. Il tramonto degli oracoli.) Florence: Sansoni. 1962. Pp. xxxv + 486. 1 plate. L. 5,000.

This attractively produced volume should do a good deal to gain more readers for some of Plutarch's essays which are of particular interest to students of ancient religion—and not only, of course, of ancient religion. The *De Pythiae Oraculis*, among other things, gives a vivid picture of ancient tourism: things are not so very different at Delphi today, except that the guides, mercifully, cannot read the inscriptions (cf. 395A). Cilento has not attempted a full critical recension of the text, and there is no apparatus. But, as is clear from his *Tavola delle Lezioni Adottate* (pp. 407–11), he has shown the sound judgment and intelligent, by no means excessive, conservatism in his handling of a number of textual problems which anyone who knows the superb critical commentary, which accompanies his translation of Plotinus, will expect. At *De Iside* 359 B one would perhaps like to know rather more about the inscriptions mentioned to confirm the reading *μηδίδης* on the strength of some MSS. notes of Donadoni, and a cross-reference seems to be needed here to the *Indice dei Nomi e delle Cose notevoli s.v. Albero*. This index is of rather doubtful usefulness: the information given is sometimes inadequate, and even misleading to a reader who is not already well informed. On the other hand the references to sources and parallel passages at the foot of each page,

with the *Tavola* at the end, are reliable and extremely useful to the reader of an author as addicted to, often inaccurate, quotation as Plutarch.

The translation (facing the text) is clear, accurate, and, as far as a foreigner can judge, elegant. The introduction is a most attractive essay on Plutarch and Delphi which, with Cilento's other Plutarchan essays (in *Trasposizioni dell' Antico*, Milan, Ricciardi 1961), everyone interested in either should read. There is also a short and very clear history of the text prefixed to the tables at the end.

A. H. ARMSTRONG.

VILBORG (E.) *A Tentative Grammar of Mycenaean Greek.* (Studia Graeca et Latina Gothoburgensia, ix.) Göteborg: Institute of Classical Studies. 1960. Pp. 169. Sw.kr. 32.

The author is well aware of the 'premature and presumptuous' nature of this essay: we may be glad he still obeyed the inner promptings leading to this serviceable systematisation of the Mycenaean language material. He presents the phonology, morphology, syntax and word-formation thoroughly, and prefaces the grammar proper with sections on the dialectal position of this type of Greek, and on the signs, their values, and the spelling rules. His bibliography is fairly full to 1958, sporadic for 1959; the word-index is helpful, but would be more so if throughout the book the section-numbers were better placed to catch the eye. It is good policy to accompany each paradigm with a more or less exhaustive list of class-members; an occasional excursus ventilates a particular problem (e.g. *ekeqe* on pp. 129 f.). The tabular presentations, as of ablaut forms (pp. 48 f.) and of adjectives of material (p. 150), are very convenient. Fresh editions of the texts, improved bibliographical compilations, new conventions of transcription (e.g. 'Wingspread'—hence the *scriptio continua* of this review) and new finds and theories have already conspired to make this book obsolescent. But very little of it is obsolete; the following comments are offered in the hope that its title of 'tentative' may soon be transmuted at least to 'provisional'.

On sign-values (to the table on p. 24 add sign 89 and renumber 42b as 90) Vilborg is excessively forbearing to others' guesses. He leaves open one door which might be firmly shut, for *qu* is not a possible value for the table on p. 28: *kunaja* is wrongly explained (pp. 49, 53) in that *γυνή* shows a samprasāra treatment of initial cluster *g²n-, and Myc. *ku*- is expected; there remain *q²u, etc., and *q²u, etc.—the former is simplified in Greek as in *κατρός* (see Allen, *Lingua* 7 (1958), pp. 130 ff.) and Myc. obligingly offers *kapinija* PY Vn 46, and the latter is sometimes reduced to velar plus labial vowel, and so in Myc. *perekuta* PY An 172, removing the last hope of *qu*. *Aite* PY Un 1321 (found 1957) might have been added to PY An 218 as further support for the equation 34 = *ai*₂ (p. 26).

One more recent suggestion would have filled two gaps: the absence of an acc. sing. for -*eu* stems (p. 93)

and the lack of explanation, except for panicky attempts (cf. p. 43), for the anomalous feminines without *u* (*ijerea*, etc.). If Lejeune (*RPh* 35 (1961), pp. 195 ff.) is right to offer acc. sing. *-ên*, the feminines may derive from a *u*-less masculine, as *léauva* derives from a *t*-less variant of *léov*. Of course, *wanakato* exists beside *wanasoi*. On p. 53 *meno* is read as *mên(n)os* with no suggestion of this ever being a nom. sing. This is sound: the apparently nominative accompanying nouns in the KN Fp tablets are probably genitives lacking the final *-jo* (cf. Vilborg, p. 57: Pedersen thought that the IE *o*-stem nom. and gen. sing. were originally identical anyway).

A different opinion is possible tediously often; this is not Vilborg's fault. Pp. 51, 103: *enewo* is hardly from **e-neun* with prothetic vowel, especially if read as *ennewo* (**enum/neum?*). P. 83: nasal stem dat. pl. rather in *-así* (<**nsi*)? P. 104: *apedoke* may be *ape(k)sdoke*; not a 'certain' example of augment, anyhow. Pp. 115 f.: on *tereja* see Szemerényi, *MLS* January 29, 1959; and Arc. *ēzēter* is a poor basis for assuming an athematic infinitive in *-en* (preferably, *porēna* read as *porēnau* would balance Arc. *porēnau* and corroborate the verb-type *CoC-ej-ō*, as in *torogējo-mēno*). Pp. 108, 118 f.: does not *dekasato* KN L 641 give verbal status to *dekoto* KN L 642 (cf. Thumb-Scherer, p. 354)? P. 133: this 'dative of respect' is strictly instrumental (hereabouts see Householder, *Glotta* 38 (1959), pp. 5 ff.).

More corrections include: p. 20: the middle ending *-to(i)* was common-Greek. P. 21: Attic *larjip* is quotable (Soph. *Tr.* 1209); *ēiv* is not certainly Ionic (correct on p. 121). P. 44: [η] should not be listed as a phoneme. P. 46 line 20: for 'palatal' read 'labial'. P. 53: *goukoro* is misplaced (the dissimilation is here *after u*; and it could be caused by the preceding labiovelar). P. 100 line 25: for 'Cypr.' read 'Cret.' P. 118 line 10: the reference to Heubeck is missing.

N. E. COLLINGE.

SHIPP (G. P.) **Essays in Mycenaean and Homeric Greek.** (Australian Humanities Research Council Monographs, 8.) Melbourne: University Press for A.H.R.C. (London: Cambridge U.P.). 1961. Pp. vii + 56. 15s.

This booklet may be fairly described as an appendix to Professor Shipp's *Studies in the Language of Homer* (Cambridge, 1953), which was completed just too soon to take advantage of the decipherment of the Linear B script. However, rather than exploit the new material, Shipp prefers to deny its relevance to the problems of the Homeric dialect. He seems to favour an extremely recent date for the latest material in the poems, since he suggests that B 362 f. is a protest against Cleisthenes' reforms of the Athenian army organisation (p. 37).

The first half of the booklet is a fierce broadside directed against a small article I published a few years ago in *Minoica* (Berlin 1958), pp. 116-22, comparing the Homeric vocabulary with that of the Mycenaean documents. Had this been presented

as a watertight proof of the Mycenaean origin of the Homeric vocabulary, it would have been sunk without trace. In fact most of Shipp's shot passes harmlessly through the loopholes I had left. Evidence which by itself is open to attack is none the less valuable when it reinforces a generally accepted theory. Since there seems to be some misconception here, it may be advisable to restate the theoretical basis for such a comparison.

Poets use a number of words which are not part of the vocabulary of ordinary speech; these are either inventions of the poet (as new compounds or derivatives, transferences to new senses) or inheritances. The latter include a number of obsolete words which have reached the poet through a literary tradition, whether written or not, and have in many cases been part of the poetic stock-in-trade for centuries. But at some point in time an obsolete word must have been in current use; many of the expressions of the Authorised Version which strike us as poetic were normal English in the reign of James I. Now it has been demonstrated that some factual items in the Homeric poems have been transmitted from the Mycenaean age; and Sir M. Bowra has shown that some words or usages of words in the epics, which are alien to the prevailing Ionic dialect, recur in ordinary use in Arcadian and Cypriot, thus establishing the probability that they have a common origin in Mycenaean Greek. While it is impossible to assert that any word *must* have been part of the poetic vocabulary in the Mycenaean period, it is none the less of interest to compare the Mycenaean and Homeric vocabularies, bearing in mind the different subjects of administrative records and poetry, and subtracting any words which remained in later Greek part of the everyday vocabulary. If we find that more than a few words or usages, obsolete in Homer, were current in Mycenaean, we shall probably be justified in concluding that some at least owe their place in Homer to continuous transmission from the Mycenaean period.

Some of Shipp's strictures on individual words may be justified; *ἀμπεξ* should perhaps be withdrawn from the list, since it is now clear that the Mycenaean sense is, at least in some contexts (e.g. PY Sb 1315), 'head-stall for horses', which was apparently maintained in Thessalian, and could therefore have reached Homer from that source. But I will put up two more targets which have emerged from recently published texts: *ἐργεῖη* = *e-ke-i-ja* PY Va 1324.1, a more specific form than *ἐργος*; and *αἰσα* = *jai-sa* PY Eq 1426.3 (the word is almost certainly complete, since *ai* stands only as initial syllable of a word or of the second member of a compound).

A note by Dr A. P. Trewick (p. 18) rightly suggests abandoning the derivation of *τράπεζα* from **k^otwg-*; to the phonetic difficulties may be added the observation that tables with four legs are more sophisticated than those with three. The metrical difficulties would be obviated if we accept H. Mühlestein's

suggestion (*Athenaeum* 46 (1958) pp. 361-5) that τ survived in Mycenaean speech but was represented in writing by σ : thus * $\tau\acute{\iota}\rho\epsilon\alpha$, like perhaps * $\text{Aph}\acute{\eta}\delta\acute{\iota}\lambda\acute{\alpha}$, cf. Cret. $\text{A}\phi\omicron\rho\acute{\omicron}\delta\acute{\iota}\tau\alpha$, or $\acute{\alpha}\nu\theta\rho\acute{\omicron}\tau\eta\tau\alpha$ [---] for * $\acute{\alpha}\nu\tau\acute{\iota}\tau\acute{\alpha}$.

The second section is a postscript to Shipp's earlier study of $-\mu$. He now concludes that the older function is more prominent in Homer, while that which is secondary from the I-E standpoint was found more useful in Mycenaean. If this is correct, it leads to the interesting inference (not mentioned by Shipp) that, whether or not Mycenaean represents a separate line of development, the tradition represented by Homer actually goes back further than the Mycenaean period. It is probably impossible to infer the original usage of this suffix on comparative grounds; and it tends to be overlooked that Vedic has it only combined with other terminations ($-\sigma$ instr. pl., $-\sigma$ dat. abl. pl., $-\acute{\alpha}\mu$ instr. dat. abl. dual). The Mycenaean use does appear to be consistent: the number is normally plural, the function either instrumental, or with place-names probably ablative rather than locative (see P. H. Ilievski, *Ablativot, Instrumentalot i Lokativot*, Skopje, 1961). The inconsistencies of the Homeric use are at least partly due to incorrect archaism, and this is a shaky foundation on which to build theories.

Four short essays attack, with some justification, Leumann's views of $\nu\alpha\iota\epsilon\tau\acute{\alpha}\omega$, $\pi\omicron\lambda\upsilon\pi\alpha\iota\tau\alpha\lambda\omicron\varsigma$ and $\epsilon\pi\iota\omicron\rho\kappa\acute{\epsilon}\omega$, and, with less, Page's remarks on the epithet $\epsilon\upsilon\mu\mu\epsilon\lambda\iota\omega$.

JOHN CHADWICK.

DAVIS (S.) **The Phaistos Disk and the Eteocretan Inscriptions from Psychro and Praesos.** Johannesburg: Witwatersrand University Press. 1961. Pp. 36. Price not stated.

This book is, as it were, an appendix to one not yet written. We are invited to accept the author's decipherment of Minoan Linear A into Hittite as proved, and to join him in a mopping up operation against the remaining pockets of resistance. His first objective is the (assumed single) script of the Arkalochori bronze axe and the Phaistos Disk, and his method of attack is the discredited one of unassisted sign-recognition. 'Fifteen signs at least are common to Linear A and to the Phaistos Disk', he claims (p. 9), and ingeniously adds (p. 13) that this is 'many more than Pendlebury supposed'. Even so thirty signs are left unmatched. But these too are promptly (p. 15) allotted sound values. We are never told how, and therefore can only judge by the plausibility of the result. Is the writing system reasonable? Do the translations make sense?

The answer to the first question is a decided no. The proposed syllabary, if true, would be a grammatical wonder. Only thirty syllables are allowed for. Eleven of these have no sign to represent them, while others are granted three, four, or even five apiece. The possibilities of variation are further multiplied by the assumed spelling rules. For example μ (which has five signs of its own) can be represented by any of the signs for a , wa , or wi

(making a total of fourteen). Initial h can be omitted or inserted at whim. So can all final consonants including stops. The resulting freedom, by which any word can be more or less spelt to choice, strikes Davis as a virtue (p. 16): 'The artist printer, for such he may safely be called, extended the spelling of his words as much as he could (cf. English Chomondoley, Featherstonehaugh), even introducing different signs—all of which still had the same value as each other—in order to enhance the artistic effect that picture writing could produce. (Cf. the Egyptian Hieroglyphics.) To achieve this effect he often introduced numerous silent vowels and consonants.'

The deciphered texts, thus unconvincingly obtained, are themselves unconvincing. All that is read of the axe (sixteen syllabic signs) is 'Mana . . . dedicated . . .'. The Disk is said to record the building and dedication of the Palace of Phaistos by a man called Nokeul, information which is repeated with few variations some dozen times. This is sense one could only agree to under compulsion. But there is no compulsion either in the arbitrary spelling rules or in the imperfect Hittite that emerges. For example Davis six times translates $arai-$ ('rise') as 'raise'. Since he does not defend the transitive meaning (elsewhere unattested) but merely refers (p. 21 n. 3) to Friedrich one can only suppose that he has misunderstood Friedrich's German gloss 'sich erheben'.

The Eteocretan inscriptions of a millennium later are also translated into Hittite dedicatory texts. Proper names (unverifiable) are frequently assumed. The certainty of the Greek lettering imposes little restraint on the phonetic interpretation. Within a single inscription θ is taken to stand successively for t , h , and k ; η for \ddot{a} ; α for \ddot{e} . The letters $\epsilon\upsilon\eta\eta$ are said to spell the Hittite $henkta/hinkta$ (which was pronounced $\chi\acute{\iota}nkta$)—the same word that was recognised on the axe under the guise of $no-da$. But for the greater part of the material Davis offers only occasional suggestions and the claim that from his 'tentative translation it can be seen that the language is definitely Hittite'.

The book contains no internal analysis of the scripts that can stand independently of the linguistic interpretation. It is therefore all or nothing.

M. W. M. POPE.

JEFFERY (L. H.) **The Local Scripts of Archaic Greece.** Oxford: the Clarendon Press. 1961.

Pp. xx + 416. 72 plates. 1 table. £7 7s.

This book has been long awaited, and in a few months it will be hard to imagine how we ever managed without it. It will be indispensable not only for those who are concerned with the subject suggested by the title, but for anyone concerned at all with archaic inscriptions, whether their interests are historical, archaeological, linguistic, legal or religious.

Part I, 'The Origin and Transmission of the Greek Alphabet', lucidly argues for an origin at a trading post on the Asiatic mainland, with Al Mina

as the obvious leading candidate. The author finds the argument from absence of inscriptions on Geometric objects very strong, and comes down for a date of origin c. 750 B.C. The great strength of this section is its realism about the psychology and practice of learning a new script. The ingenious inventor of vowels disappears; error, pronunciation-differences and deliberate alteration are allowed to play their part in the creation of local scripts.

Part II, 'Writing in Archaic Greece', first discusses the direction of the script and methods of inscribing. J. argues for boustrophedon as a normal practice from the first, with left to right writing very early, and allows continuous retrograde no real history in Greek. Here alone in the book does its long pre-publication history appear to have affected the argument. I feel that the Pithekoussai cup, with its continuous retrograde, intruded too late into a view already formed. There is a good section on materials for writing, a most instructive one on the subjects of early inscriptions, with the emphasis on the absence of early historical documents, and a cautious one on letter-forms as dating evidence, with a salutary warning that writing on other materials developed faster than writing on stone.

Part III, 'The Local Scripts', is far the longest. Area by area, we are taken through the history of each script, and, by the way, through the problems involved in its inscriptions. The catalogues of inscriptions, with select bibliographies, have 1088 items, and J. has something helpful to say about a surprising number of them. To take some examples, there is a model discussion of the Perachora kerbstones (pp. 122-4), with the earliest put c. 650; a treatment of Kleobis and Biton (p. 155) which sticks to facts for a change; the inscription on the Dipylon oinochoe is shown not to be genuine Attic (p. 68; cf. p. 16; see now the Attic abecedarium, *Hesperia* xxx, 1961, p. 146, R 22, possibly as early, and without the sidelong alpha); a plague is cured (p. 269 n. 3); the Apollo temple at Syracuse loses some columns (p. 265 n. 5). There are few problems of archaic Greek history which are not somewhere touched on; J.'s treatment is generally sound and economical.

It would be surprising if, in matters of such uncertainty, she always carried conviction. On the Peisistratos altar (p. 75), for example, she hardly does justice to Thucydides or the archon-list, implying, but not saying, that 511-10 is just tolerable for the letter-forms, but that 522-21 is not. The weight allowed to letter-forms here is denied them in discussion of the Aiakes statue (p. 330), where there is an uneasy compromise between her better judgment (nearest epigraphic parallel 500-490) and Buschor's authority in setting the date of the statue c. 540. But how far was Buschor originally influenced in his dating by the belief that Aiakes was Polykrates' father? And do the statue and the inscription have to be contemporary?

The problem of how to discuss letter-forms in print has been as nearly solved as is possible. Each

area has a text-figure in which various letter-forms are drawn and numbered. Side-long alpha on the Dipylon oinochoe is thus Attica α1, and letter-forms can then be referred to as γ3, etc. The strain on the printer is therefore much eased, and the reader is as well served as he can reasonably expect. Illustration is generous. The plates give us more than 500 inscriptions; 16 pages of transliteration help the faint-hearted to read them, and form a unique collection of archaic texts.

The most serious complaint to be made about this book is the lack of a survey of the history of the subject. J. addresses herself to those who have grown up with the old literature and share her background. But, frankly, I do not see why those new to the subject in the future should have to read Kirchhoff and the rest. They should and will read J., and there is just sufficient allusion made to 'red' and 'blue' alphabets to puzzle them, without enough guidance to solve their puzzles. Some things, on the other hand, have been totally suppressed. One would not, I think, gather from this book that it is not so long since the Abu-Simbel inscriptions were regarded as crucial evidence for the development of the alphabet. It is clear why J. thinks they are not, but she does not say so, and anybody who starts from this book and reads just a little further will soon have some surprises.

The book has a staggering amount of detail, but Press and author have been equal to their task. On p. 53 the reference for κέρβη should be to *IG* xi 2; on p. 78, no. 39 read *IG* i² p. 272. But that is nearly all I have so far found.

D. M. LEWIS.

RICHTER (G. M. A.) **The Archaic Gravestones of Attica.** London: the Phaidon Press. 1961. Pp. viii + 184. 96 plates (= 216 figs.), 22 drawings in text. £3 13s. 6d.

This splendid re-edition of *Archaic Attic Gravestones* (1944) is virtually a new book, as the changed title signifies. The text is in great part re-written, and the catalogue of examples contains much additional material. Miss Alison Frantz contributes 108 of the 216 superb photographs, and Professor Margherita Guarducci an excellent Appendix on the inscriptions. Here Dr Richter renews and amplifies her original reconstruction of the development of the characteristic Attic grave-stele. Its disappearance somewhere round 500 B.C. she ascribes rather to the general climate of this time of rising war than to any specific sumptuary laws. This does indeed seem sufficient reason, especially if one recalls also the perceptive remarks of Miss Evelyn Harrison (*Hesperia* 1956) on the effects of a change in social climate upon an established artistic fashion; nor should we forget that by the late years of the sixth century the growing popularity of bronze sculpture must have won over many a young apprentice who earlier would have gone in for marble work.

A short Introduction reminds us of the salient points of the technique, thoroughly discussed by

Dr Richter in *AAG* and elsewhere: tools, colouring, and the six linked types into which she subdivides the stelai. She also discusses briefly the origins of the funeral stele and its peculiar Attic type (the stele of Keramo, *IG* i² 997, could perhaps be added to the prototype in the Kerameikos, p. 9). Then follows the Catalogue itself, a masterly exposition of treasure after treasure, including a whole pride of early Attic sphinxes, at last properly illustrated as a sequence, their alert faces helping to fill the many gaps in our portrait-gallery of korai between the Berlin 'goddess' and the Peplos kore. The following notes concern minor points only. *Type Ia*. Is 6 necessarily a stele-shaft? Three fragments: 1-2 (moulded edges) certainly belong together; 3 has a smoothed underside (no moulded edge). None preserves original height or thickness. Raubitschek (*DAA* no. 34) associated these in a stele-shaft because he also associated a cavetto capital (here 5). But if this capital does not belong (and Dr Richter's argument against it seems cogent), what grounds remain for this stele-shaft which, if 3 does belong, was, abnormally, not monolithic? 3's smoothed underside suggests rather the top block of a stepped base, or a pillar-capital; 1-2's moulded edges could suit either of these.—Would the shaft-decoration of Type Ia be better left undecided in a reconstruction? The drawing (p. 14) shows, *exempli gratia*, sphinx 1 (capital) with a hypothetical kouros-relief on the shaft. Geometric patterns are the only identifiable decorations on the existing shaft-fragments (7, 8. The relief on 9 (Dermys and Kittylos, Boeotia) is not quite analogous, since its figures are frontal, like the female ones from Halai (*Hesperia* 1940, 413, fig. 57) and Malessina (*Mon. Piot* xx, pl. 3); type peculiar to the provincial areas of Boeotia and Locris?). Moreover sphinx 1 sits to left—a rarity. A relief figure below would surely face left also, as in the drawing; yet virtually all of these archaic grave-relief figures face right. This might suggest that the original shaft of 1, at least, had no relief figure. *Type Ib*. Did all these capitals carry sphinxes? A long rectangular plinth with a rectangular tenon under it, required by the top of capital 21, differs markedly from the existing plinth of sphinx 11; and the unusual cutting on top of the Lamptrai capital (20) seems too wide and shallow for a tenon, yet too small and asymmetric for a sphinx-plinth: is a statuette-plinth conceivable here—a horseman to right, as in the relief below?—In what wall was 26 'tooled for use'?—27: the youth holds a javelin (Jüthner); for the gorgon below, see now the fragment *Bull. Met. Mus. N.Y.* 1958, 187.—35, p. 158: σοφός for craftsmen, see especially Chr. Karusos, *Epitumbion Chr. Tsounta* 544 ff.—36: νεῖσθε ἐνὶ πράγῃ ἀγαθόν. The assumption on p. 158 that the traveller is here wished good fortune is surely right, not Friedländer's 'proceed to worthy tasks', used as the translation. Add to the comparative material for line 3 *Hesperia* 1949, 167, and to the bibliography of 53 (Sigeion stele) Page, *Sappho and Alcaeus* 152 ff.—*Type Ic*. 52: the likeness to 37 is noted; could one add

to this workshop *Kouros*² no. 142?—*Types IIa-b*. 54 still seems to me, as it did to Miss Harrison, better ascribed to the end of the sixth century.—59, the fragment with a seated mother and baby, is clearly sepulchral, as Dr Richter says; which may perhaps suggest that the similar stelai showing seated women, to which she refers in Excursus Two ('Heroizing Reliefs'), are not in a special (votive) category, but are gravestones like 59, the iconographic convention for dead wives, no more—nor less—heroised than the others. (Is the votive plaque here cited, showing worship of a seated female deity, necessarily relevant? and the seated figures on Laconian stelai, which undoubtedly have a heroizing element in them, are not women only, as these.)—60 A-B. ('Theron's stele'). Dr Richter evidently thinks that the similarities in provenance and looks outweigh the two small discrepancies between the extant Agora stele-shaft (B) and the lost stele of Theron (A) as drawn by Vulliamy; and surely she is right. To the date here 'c. 525' some may prefer the rather earlier dating of Miss Harrison, who stressed the thick shaft and the separately-carved finial, in type unlike any others assigned to Types IIa-b.—61 (also 9, 70): does not 'to' or 'for' in this translation, rather than 'over', blur the meaning of ἐπὶ ἔθηκε?—65: measurements of top and bottom widths reversed? but even if transposed, they hardly fit those given for 66, thought by some to belong to 65.—75: inscription verse (as Appendix), not prose (as catalogue); stoichedon (not noted here), indicating that the battered letter above omikron, if centred squarely, should be alpha rather than iota: ἐπὶ Ἀγαθόν, not ἐπὶ Γάθον? Perhaps a youth and older man rather than two youths, since the second figure is larger, and wears a himation.—79: the Appendix seems to suggest two dates for this inscription: c. 510-500 on p. 51, but a date near 37's (i.e. 540-35?) on p. 172.—The drawing of 8 is reversed, and fig. 82 is inverted. Some few trivial slips or printer's errors were noted in the text, but to list them here would be not merely ungrateful but absurd, in view of the masterly quality of the work itself, and of the great service done to classical scholars and all students of art by the Phaidon Press, in producing at this modest price a volume whose appearance is worthy of its content.

L. H. JEFFERY.

LAMPE (G. W. H.) Ed. *A Patristic Greek Lexicon*. Fasc. 1 (α-βαρβαρώ). Oxford: the Clarendon Press. 1961. Pp. xlix + 1-288. £4 4s. od.

LAMPE (G. W. H.) Ed. *A Patristic Greek Lexicon*. Fasc. 2 (βαρβαρέω-εὐσυνμαθήτως). Oxford: the Clarendon Press. 1962. Pp. 289-576. £4 4s. od.

The appearance—so long awaited and now becoming a fact—of this monumental work is an event; primarily for students of the Fathers,¹ only

¹ From this angle, and in a more general way, the PGL is reviewed in *Journ. Eccl. Hist.* 1963, part 1.

less so for all scholars whose interest in matters Greek is not bounded by the date of Demosthenes' death. 'Both Patristic and Byzantine literature' were excluded as a matter of principle from the purview of Liddell-Scott-Jones (*Preface*, p. x); everything Christian, that is, except the Greek Bible (an exception though was made for 'the historians and poets of the reign of Justinian'). The decision no doubt was a practical necessity; it meant, however, that the presentation of the Greek *copia verborum* down to 'roughly A.D. 600' was deficient with regard to both the impact of the new religion upon old-established words and the changes and innovations attested especially in the popular idiom of Christian *Kleinliteratur* such as the apocryphal Gospels and Acts or the stories and sayings of Egyptian monks. The principle adopted moreover led to certain oddities. The words *ἀρχιδιάκονος* and *ἀρχιεπίσκοπος* appear in *LSJ* (legitimised by their occurrence in Justinian's *Novellae*), but Synesius—as Greek a poet, rhetor and thinker as any—disqualified himself by consenting, at the end of his life, to become a bishop; Nonnus has been allowed in by halves, but his Johannine paraphrase, though cast in the same mould as the *Dionysiaca*, was rejected; Procopius of Gaza (less lucky than his namesake) likewise had his rhetorical output included but not his theology.

These deficiencies are now being made good. What is lacking in *LSJ* will be supplied by *PGL*—and *vice versa*; for 'no word which is well attested in *LSJ* and has no particular interest for the reader of the Fathers is included in *PGL*' (thus on p. ix of the *Preface*—which every user of *PGL* will have to study from end to end). The two lexica then have to be used in combination. The purpose and method of the new lexicon though are different in so far as its primary aim is to serve theological interests. Accordingly, words which convey essential theological concepts are treated in detail; not with regard to grammatical or stylistic peculiarities but in evidence of the range and development of the ideas expressed by them. This approach is in fact entirely to the point. Most of the 'Fathers' wrote classical, or at least educated Greek; hence new words (apart from elementary compounds such as *ἀτιμοποιός* or *ἀποκαθαρισμός*) occur only rarely in their writings and the changed connotations of traditional words are by far more significant than these. The student of classics will from this point of view survey with interest such articles as *ἀπερή*, *δαίμων* ('devil'), *δόξα* ('glory'), *εἰκών* and *ἔσως*. He will note, probably with some surprise, that even such new and essential Christian coinages as *ἐνανθρώπησις* and *ἐσαρκωσις* had non-Christian antecedents (*LSJ* quote *ἐνανθρωπίζω* from Heliodorus' novel and *ἐσαρκός*—with vastly different meaning—from Porphyry); moreover, he will be struck by the evidence, so conveniently and fully presented, for the extensive use which Christian thinkers made of the tenets and methods of Greek philosophy. In principle these antecedents are not

indicated in *PGL*, and the few indications which have slipped in are not invariably conclusive. Thus s.v. *αἷμα* I A 2 (p. 48 r.) 'Empedocles ap. Stobaeus *ecl.* I. 41.53' is quoted for the notion of 'blood as substance of soul'. This is 31 B 105 in Diels-Kranz⁹; an uncertain warrant, even though Aristotle (*de an.* 405 b 4) and many later writers thus understood him; for E. is there speaking of 'thought' (*νόημα*); he never used the word *ψυχή*. Our scholar though would need no explicit reference in order to find an indubitable and noteworthy echo of the same Empedocles quoted from Origen by Epiphanius (s.v. *ἁμαρτία*, II B 3 b, p. 82, left) *τὴν ψυχὴν . . . προεπάργειν ἄγγελλον δὲ ταύτας εἶναι . . . ἐν ἁμαρτίαις δὲ ἀμπλακησάσας καὶ τοῦτον ἐνεκεν εἰς τιμωρίαν εἰς τοῦτο τὸ σῶμα κατακεκλεισμένας*. A few lines earlier in the same article (*sub* 2 b) the Aristotelian types of behaviour, *φιληδονία*, *φιλοδοξία*, *φιλαργυρία*, are described as the essence of sin by the abbot Dorotheus (cent. VI): he echoed the hymn of Kleantes (v. 27 ff.). Likewise Stoic are (*ibid.*, II A 1 a and 2 b) the definitions *ἡ ἑκκλησις ἁμαρτία ἐστὶν κτλ.* and *ὁ νόμος . . . ἐπαγορεύει τὰ ποιητέα εἰργεῖ τε ἕκαστον τῶν ἁμαρτημάτων*, and the adjective *ἀτρεπτός* can be seen (s.v.) developing, from a Stoic basis, into a central theological term and producing the new noun *ἀτρεπτότης*. Again, no one needs to be told where to look for the origin of the definitions of *ἄσθροπος* (s.v. A; p. 141, left) *quod sursum spectat* and *ζῶον λογικόν, θνητόν, κτλ.*, but these elementary instances show that entries of this type may help—quite apart from their historical interest—in the endeavour to recover *fragmenta philosophorum* from patristic writings.

The care and labour spent upon the elaboration of the specifically 'theological' articles have not caused the more properly 'lexicographical' aspect of the *PGL* to be neglected. While the recurrence, in the Christian literature, of well-attested traditional (and non-theological) words and meanings has not been recorded, many hundreds of entries serve to underline peculiar features of the diversified language used by the writers in question; from the hymnic turgescence of a Synesius to the homely vulgarism of John Moschus and from the classicism of the Cappadocian Fathers to the crudity of Malalas. The spread of the educated Koine of the first centuries stands out, e.g., in the wide adoption, by the more literary Fathers, of the adverb *ἀπολύτως* 'absolutely' (as in Epictetus); the same incidentally confirm (s.v. *ἀκριβής*) the soundness, in M. Aurelius ii 5, of the transmitted phrase *μετὰ τῆς ἀκριβοῦς . . . σεμνότητος*; the adjective indicating that its noun is to be taken 'in its full and exact meaning' (thus also, e.g., in Dio Cassius 67 18.1 and 4 *μάγος ἀκριβής*; cf. Kron, *Mnemos.* 1938, 39). Among ancient words re-emerging, *βρέθ(ε)σιον* will be hailed by readers of Sappho; *ἀεροβατῶ* recalls Aristophanes; *ἀκρόδρνον* as defined (s.v.) by Cyrill may be noted by those wrestling with the wording of Theocr. xv 112. Among many hackneyed new formations, less

obvious ones are not lacking; e.g. ἀποσκίασις and ἀποσκιρῶ and poetical ones like ἄζαλος, ἀγαθόρρητος, and many more coined by Synesius (it matters little that two of these—ἀκαμαντοπόδας and γαιωστραφής—have been overlooked; but his adj. ἐνοτήσιος means 'cause of oneness' and not merely 'uniting'; εἰδωλοχαρής too is wrongly translated; see Terzaghi *ad hymn.* I. 92). While in this section many more spelling errors (such as αἰειδιότης and ἀκράτειστος) have undeservedly gained entry, the beautiful vulgar metathesis ἀμλεῖνω (*Passio Perpetuae* 4) ought to have been quoted s.v. ἀμέλγω.

This brings us to properly 'new' words. There are, first, many borrowings from other languages; chiefly from Latin (e.g. ἄρμα, ἀρμάδα, ἀματόν, etc., βέρνακλος, βέργος, βήλων, βότα, i.e. *vota*, βούκινον), but also Gothic (δροῦγγος and -άριος, through Latin *drungus*), Syriac (ἀρούβατον), Persian (βαρζαμανᾶται) and even Hunnic (δόχια). Finally, there are new and pregnant Greek words; some from identifiable roots; such as ἀπομαδαρόω 'to shave' (cf. μαδαρότης Hippocr.), ἀγκαδικῶν (a garment), βούνευρον and -ίζω ('whip'); also the odd malformation ἄτεξ (ἄτοκος); and there are newcomers like βονκί(ο)ν, βοῦγλιν, βόζια, βυζάνω (cf. Schwyzler i 259), βυτίον. Here we are right in the Byzantine Middle Ages.

No further illustrations will be needed to show the outstanding value of this great work. Its editor, his collaborators, and the Press deserve our gratitude and admiration and every good wish for the completion of a task as vast and arduous as it is meritorious.

G. ZUNTZ.

PEARSON (L.) **Popular Ethics in Ancient Greece.**

Stanford: University Press (London: Oxford U.P.). 1962. Pp. vii + 262. £2 4s. od.

The aim of this book is said to be 'to discover how the Greeks who were not philosophers reasoned with themselves and argued with others over ethical issues'. The object is analysis of the principles involved rather than mere description, and it is mainly concerned with Athens in the second half of the fifth century—the fourth and later centuries are deliberately excluded. It is a matter for some surprise that the evidence discussed turns out to be wholly literary. Inscriptions, vase-paintings and other material records, one might suppose, would be very important here, and when M. P. Nilsson published his *Greek Popular Religion* in 1940 it was to these rather more than to surviving writers that he turned for his material. As for religion, so one would expect for popular ethics also that the literary evidence alone would be insufficient, and it is dangerous to assume in advance that the literary evidence for Athens is unique in ancient and modern times in that it is always evidence for the feelings of the whole community.

There is, however, much important work still to be done in analysing the ethical implications in the literary tradition, and to this the present book makes interesting and stimulating contributions. There is first a general sketch of the position in the fifth

century based upon material from Plato, *Rep.* I–II, from which it is concluded that the conflict between justice and expediency was the main theme of late fifth-century ethical discussions. Then successive chapters deal with Homer (ch. 2), Hesiod, Solon and Theognis (ch. 3), the tragedies of Aeschylus (ch. 4) before returning in greater detail to the analysis of authors such as Herodotus, Thucydides, Sophocles and Euripides, whose work actually belongs to the second half of the fifth century.

On the relation between Homeric thought and that of the fifth century Pearson takes a view in some ways similar to that of A. W. H. Adkins, *Merit and Responsibility*, Oxford, 1960—the Homeric conception of Arete had 'success' as its essential feature, and only later did Justice become essential for Arete. But he rightly insists that we have no grounds for supposing that Arete exhausts the range of moral judgments open to Homeric man, and he points out that the poet is often explaining, not condoning, men's behaviour towards each other, and the ingredients for a conflict between 'success' and 'justice' as ideals are already present. The ever-increasing insistence upon justice as an ideal in the literature after Homer is well and clearly described. Pearson would say that in Aeschylus the search for Arete in the Homeric sense seems to have given way before the search for justice, with Charis also regarded as an important quality. But in the age of Euripides and Thucydides the faith in justice seems to weaken and give way, at least in the world of the Melian dialogue, in favour of expediency. The conflict between justice and expediency was insoluble at the level of popular ethical feeling and could only be dealt with satisfactorily by philosophy.

Such, if I understand it rightly, is the general theme. But it is developed through detailed discussions which are of great interest and may well be the most valuable part of the book. I would incline to query two general points. Firstly, Pearson often speaks as if Justice had in the fifth century become supreme and absolute as the highest popular value. Important though it was, this may be doubted. When Plato reconciles justice and expediency for the individual by appealing to the nature of man he may be doing in a highly sophisticated way what popular thought had already begun to do, namely appeal to something higher than justice in order to find a solution. Certainly many of the sophists seem to reflect an awareness that there is something unsatisfactory about a justice which is ultimately and not merely temporarily inexpedient. Secondly, it may be doubted if there was any very direct relation between Homeric Arete and the 'self-interest' which was felt to conflict with the demands of justice at the end of the fifth century. Sometimes no doubt the equation was more or less consciously made. But Homeric Arete could itself conflict with self-interest, as when a hero is expected to do something dangerous to himself, and Plato is probably reflecting ordinary thinking when he identifies Justice with Arete and then finds both in apparent conflict with Expedi-

ency. He treats a conflict between Justice and Arete as inconceivable, and when Thrasy machus implies that there is one this seems intended to be a shocking paradox which perverts the ordinary meanings of the words.

G. B. KERFERD.

LYNCH (W. F.) **An Approach to the Metaphysics of Plato through the Parmenides.** New York: University Publishers Inc. for Georgetown University Press, Washington. 1959. Pp. xiii + 255. \$6.

This is yet another attempt to interpret the eight hypotheses of the second half of the *Parmenides* as a systematic exposition of Plato's own metaphysical views. Fr. Lynch finds here 'a kind of textbook summary of Plato's metaphysics, presented in a comprehensive and ordered way'. The hypotheses are 'an analysis of the constitutive elements of anything that is a true one, or unity, in any order of being'. And this 'philosophy of unity' constitutes 'a more sophisticated theory of Ideas (and of being)' which meets the criticisms of a 'relatively crude form' of the theory of Ideas in the first half of the dialogue.

Many obvious objections can be raised against any thesis that all eight hypotheses together present a positive and consistent theory. Among them are: (i) the two sets of conclusions in each of the four successive pairs of hypotheses contradict one another; (ii) in hypotheses II, III, V, and VII a series of contrary predicates is shown in each case to be applicable to the same subject; (iii) the combined results of the whole analysis are presented at the end as contradictory and apparently absurd; (iv) the interlocutors agree that the conclusion in hypothesis I cannot be true. To meet these objections Fr. Lynch assumes (a) that eight different 'aspects' of 'the one' are explored in the eight hypotheses (e.g. 'the one' is studied in hyp. I as 'a pure indivisible without parts' in hyp. II as 'a composition of parts', in hyp. III in its function as a limit in giving definite unity to its parts, in hyp. V as a being which incorporates a principle of relative non-being or 'otherness'); (b) that 'the others' always (except in hyp. V) mean the elements in any unity. With these assumptions it is shown that the contradictions between the conclusions of the hypotheses are merely apparent; they are true of 'the one' or 'the others' in different respects. The same general explanation is valid for the application of contrary predicates (thus in hyp. II the series of contrary predicates applied to 'the one' marks 'the basic contrariety' that the one is here both one and many).

Fr. Lynch's detailed arguments have much ingenuity. But his thesis commits him to more than the most ingenious interpretation can consistently explain. Thus he has to assume the validity of Plato's reasoning. He admits, however, that at 147 c-149 d Plato 'has his tongue somewhat in his cheek'. The arguments are 'somewhat sophistical' and 'are in no ways meant to supply actual answers to our original dilemma'. This makes them

incredibly odd exceptions. For it is assumed that the rest of the arguments offer a systematic and progressive exposition of metaphysical theory. Again, it is admitted that assumption (b) above is not valid for hyp. V. And again the exception raises doubts. For, without Fr. Lynch's assumption, a general sense can be given to 'the others' which not only suits Plato's arguments more easily and more naturally, but obviates the need to appeal to an exceptional usage. There are other places where the attempt to square text with thesis involves ingenious but unconvincing juggling with different senses of a word. A good example is the treatment of knowledge in hypotheses I and VI. In hyp. I it is said that if there is a one it cannot be known, in hyp. VI that if there is no one it cannot be known. 'Unknowability' appears to be asserted in each case (142 a and 164 b) on the same ground (knowledge of $\tau\acute{o} \mu\eta \delta\upsilon\tau$ is impossible). Fr. Lynch accepts that this is the ground at 164 b. But he cannot accept it at 142 a, since 'the one' of hyp. I is, for him, 'an absolute existence'. He asserts therefore, most implausibly, that this 'one' is unknowable only in 'the technical Platonic sense' of 'that scientific type of knowledge that is the object of analysis for the *Theaetetus*'. As for objection (iv) above, he mentions it but does not answer it; he merely emphasises that the apparently negative conclusion of hyp. I masks a positive doctrine on which the argument of hyp. II depends.

There remains the argument that the positive metaphysical doctrine of the second part of the dialogue answers the objections to the theory of Ideas advanced in the first part. The development of this argument is, essentially, a variation on the theme that in the late dialogues Plato, recognising the 'common structure' of sensible and Idea, developed a new doctrine of 'participation', with accompanying techniques of analysis, and thereby bridged the gap between sensibles and Ideas. Fr. Lynch restates this in terms of a 'philosophy of unity'. But he does not think that it entails the abandonment of 'the transcendental existence of the Ideas'. Yet he cannot have it both ways. Either 'the more sophisticated theory of Ideas (and being)' is a theory of non-transcendent Ideas. In that case the objections raised against the earlier theory are overcome by abandoning it (I do not think, however, that either the *Parmenides* or later dialogues provide evidence for such a new theory). Or this 'more sophisticated' theory retains the transcendence of the Ideas. In that case it does not answer the objections.

Fr. Lynch fails, then, to substantiate his thesis. But his book is both ingenious and stimulating. And his arguments are presented with a sincerity and modesty which command respect.

NORMAN GULLEY.

RUNCIMAN (W. G.) **Plato's Later Epistemology.** Cambridge: the University Press. 1962. Pp. viii + 138. £1 15. 0d.

This book is a useful contribution to the unending

reassessment of those two great concerti of logical difficulties, the *Theaetetus* and the *Sophist*, and students of these dialogues will find that it is a complement and corrective to the famous, but by today's standards, hypanalytical, *Plato's Theory of Knowledge*.

Mr Runciman is interested in the question of how far Plato distinguished between *knowledge how*, *knowledge that*, and *knowledge by acquaintance*; also whether Plato consciously approached significantly close to the concept of *truthvalue*. In playing the fascinating game of measuring Plato by some of the yardsticks (some of which are pliable) of logical analysis, Mr Runciman has avoided the most obvious pitfall of over-emphasising Plato's concern with logic. A sound appreciation of the peculiarity of the Greek language has kept before his attention the ontological gravitation exerted by the *Forms* upon Plato's thought even when the problems under consideration were apparently the same as those which interest our logical analysts. Plato is represented (not unfairly I think), as one who was prevented only by a caul, as it were, of ontological preoccupation from emerging into the light of linguistic and perhaps non-metaphysical sophistication. 'If ever a philosopher can be said to be doing logic without knowing it', says Mr Runciman, 'then we may say that Plato in the *Sophist* is doing logic' (p. 126).

Plato almost begins to do logic in the *Theaetetus* when he says that knowledge is more likely to reside in our *συλλογισμός* about perceptions than in the perceptions themselves, and he seems to be within sight of attributing to propositions some of their modern significance when he says that our *αναλογίσματα* (judgments) about perceptions have to be learned (185 e). The implications of this incident in the dialogue were not pursued. Plato did not distinguish between *intellectual knowledge* and *knowledge by acquaintance* at this stage, and since knowledge tended to be of entities (and so almost inevitably of simplicia), it is reasonable for Mr Runciman to hold (against Cornford and Ross) that Plato intended to solve the paradoxes involved in sense-perception by means of the *Forms* rather than a not yet sufficiently developed theory of logical relations, though he supposes that a foreshadowed influence of relations cannot be excluded. Nor did the ontological solidity of the *Forms* prevent Plato from reaching some appreciation of 'grades of existence' so that *ὄντως ὄν* was no mere 'tautology' for him, though he never quite managed to realise in the *Theaetetus* that 'both true and false statements could be made about things that do not in fact exist'. He was prevented from seeing the difference between the *meaning* or *proposition* and the *truthvalue* by the ghost of Parmenides, which was only exorcised, and then not completely, by the *Form of Difference* in the *Sophist*. On p. 36, however, the author records the interesting suggestion that Plato did reach an almost Fregean position in *Philebus* 38 d ff.-39, where the mind is represented not as an aviary with individual birds flying about in it nor a wax tablet with various imprints but as a book in

which *λόγοι* are written. These *λόγοι* should be translated as *propositions*, according to Mr Runciman, and not as *words*. Thus, as in Frege, we might have judgments as comparisons between propositions and their *truthvalues*. Mr Runciman maintains against Gould that *ἐπιστήμη* is *knowledge by acquaintance* rather than *knowledge how*; he remains in the view that the *knowledge by acquaintance* of simplicia seems to involve some awareness of how simplicia combine, which could be, I suppose, a kind of *knowing how*, but need not necessarily be so.

Mr Runciman does not underrate Plato's appreciation of the difficulties provided by *εἶναι* in the *Sophist*, and he opposes the Cornford-Ackrill view that Plato marked off *εἶναι* = exist and at least one other sense; he thinks it much more likely that Plato marked off identative *εἶναι* from at least one other sense, for *τὸ αὐτόν* is, after all, separated from *τὸ ὄν*; also he notes that there is no explicit marking off of *εἶναι* as a *Form* whereby it might be used without 'a filling' such as A is (not) x or y (x or y predicative or identative). However, *κίνησις ἔστιν* or *στάσις ἔστιν* do not really seem to need such a 'filling' (p. 85). On 256 d 12-e 6, Mr Runciman is in favour of translating *εἶναι* as *be*, a word which embraces the ambiguities of the Greek, rather than have the Eleatic Stranger talking such nonsense as 'for each of the *Forms* there are many things that it *exists*' (as against 'that it is'). I wonder if this is quite so nonsensical in Greek? The interweaving of the *Forms* remains obscure. Mr Runciman gives a concise account of what it cannot be (p. 111 ff.). It has to be admitted that Plato never arrived at a satisfactory way of distinguishing between particulars and *Forms*. Plato, like most of us subsequently, had to fight hard not to regard *Forms* as super-particulars.

The book is written in a serviceable style with some traces of Quinean sinuousness. Its interest and brevity make one wish for a more expansive blow by blow commentary on these dialogues à la Cornford, whose *PTK* was first published in 1935. There is an index and a bibliography.

H. D. RANKIN.

ARISTOTLE. *Politique, livres i et ii*. Ed. and trans. J. Aubonnet. [Budé.] Paris: 'Les Belles Lettres'. 1960. Pp. ccvi + 175. NFr. 21.

The editor of the Budé *Politics* describes his text as composite, based on both families of MSS., but with a preference for the second. Consequently it is in essentials the text of Newman and Immisch, and where they disagreed the reading of the one or the other seems always to have been taken, usually in line with Ross. Orthography and punctuation have been altered in many places, but without altering the sense. A quite full apparatus criticus is given, selected from Susemihl-Immisch but amplified by some more recent conjectures, mainly those of Ross. No emendations since Immisch have been taken into the text, and the editor proposes none himself. In thus adhering to the MS. readings he

has returned, for example, to *Χάρητι δὴ* at 1258^b40 (with Bekker, Immisch); his brief note here seems to suggest that this 'Chares of Paros' is often quoted by Pliny, but Pliny quotes only the Apollodorus here mentioned and none of the other persons named Chares whom Pliny quotes could be this one: there is in fact no other evidence that this one existed, and Susemihl's conjecture *Χαρητίδῃ* was defended by Newman and accepted by Ross. Further, he retains harsh MS. readings at 1254^b17 *δὲ* omitted; 1261^a27 *ἐλκύσει* after *ὥσπερ ἂν εἰ* (*ἐλκύσει* Coraes); 1270^b22 *αὐτά* from H^a giving a forced word order (*ταῦτά* Susemihl).

He gives a short but thorough account of the MSS., but it is tiresome that he has invented new sigla for some. The translation is close and clear, rather than elegant. The notes are compact, largely quarried from Newman who is frequently translated verbatim without acknowledgment. The long introduction discusses Aristotle's development, the place of the *Politics* therein, its composition, and its influence upon later ideas. M. Aubonnet does not, like some of the distinguished Budé editors, offer an original contribution to such problems; but he gives a well-balanced account of the present state of the question together with ample references. The Greek text is beautifully clear of misprints: all that I have detected are the wrong breathings on 1255^b1 *ἀξιούσι*, 1259^b36 *οἶόν*, 1260^b30 *αἷς*, 1265^a17 *οὖν*, while at 1274^a33 *βακχιαδόν* should be a proper name.

D. M. BALME.

WEIL (R.) *Aristote et l'histoire: essai sur la 'Politique'*. (Études et commentaires, xxxvi.) [Thèse.] Paris: C. Klincksieck. 1960. Pp. 466. NFr. 40.

Weil describes the ultimate aim of this book as being to define the relation between Aristotle's political thought and his time. He asks whether, as is generally assumed, Aristotle limited himself to a narrow notion of the *πόλις*, or whether he conceived larger political entities. However, the major part of Weil's book deals with the problems of the development of Aristotle's political thought, and in particular with the question of the date and order of his various political writings. He begins with a detailed analysis of the inconsistencies presented by the eight books of the *Politics* as we have them, and he points out, after examining the various interpretations which have been offered, that our knowledge of the composition of this work is still very fragmentary.

In undertaking his own interpretation, Weil draws inspiration from the work done on Aristotle's biological treatises since D'Arcy Thompson first noticed the correspondence between the place-names mentioned in *HA* and the areas covered by Aristotle's travels in Asia Minor and Macedonia. By comparison, the results which Weil achieves after a mystic examination of the *Politics* are perhaps rather disappointing. He uses two methods for dating the various books. First, he considers the

direct references or allusions to historical events, but this method is, of course, well tried and yields surprisingly few positive conclusions. One of the rare new suggestions which Weil makes is that the passage referring to the *πόλεμος ξενικός* in 1272^b 20 ff. is to be dated between 345 and 343 (he takes it to refer to Phalaecus' expedition, and argues that the perfect tense *διαβέβηκεν* implies that the war was still continuing at the time). But considering the strangeness of the expression and the large measure of uncertainty about the historical event to which it refers, this seems too positive a conclusion.

The second method which Weil uses to date parts of the *Politics* is more original. This consists in analysing the illustrations which Aristotle uses and tracing the growth of his research into the histories and constitutions of different states. Weil argues plausibly that Aristotle's interest in historical research began early, certainly before the period of the Lyceum, and he discusses at some length the date of the lost works of Aristotle bearing on historical or political subjects. He shows that in many instances there is no definite evidence that these were produced during Aristotle's second stay in Athens, and he is successful in assembling some positive evidence that some of them antedated that period (Jaeger had already admitted that the list of Pythian victors may have been drawn up 'towards the end of the Macedonian period'). Weil then analyses the different types of illustrations found in the *Politics* and makes out a convincing case that the differences between the references to barbarian *customs* in VII and VIII and the examples of barbarian *constitutions* in V cannot be explained solely in terms of the different subjects with which these books deal, but imply an important development in Aristotle's research on non-Greek states. Again, Weil argues that while Aristotle's knowledge and assessment of Sparta and Crete underwent no fundamental development, the various references to Carthage in II, III, V and VI suggest an evolution both in his judgment of that constitution, and in the information on which it was based. Weil also undertakes an exhaustive analysis of the references to other Greek states in the *Politics* to determine to what extent Aristotle was already drawing on his *Constitutions* (as is suggested by *EN* 1181^b 17). Here he is surely unwise to argue that particularly brief or obscure references imply that Aristotle's readers probably had other sources of information (in the form of a *Constitution*) in which they could find Aristotle's ideas more fully elaborated. Yet the general conclusion which Weil reaches at this point, that the *Politics* is not based on a complete collection of *Constitutions*, is unexceptionable and has, of course, long been widely accepted.

Finally, Weil considers Aristotle's theory of the state. He gives a clear and cogent account of the development of Aristotle's theories on the classification of constitutions, and on other less obvious topics, the evolution of the city from its primitive origins, and the cycle of changes by which one constitution turns into another. Lastly, he suggests

that while Aristotle began by distinguishing sharply between a *πόλις* and an *ἔθνος* and denying that the latter could have a *πολιτεία* (e.g. III 1276a 24 ff., VII 1326b 2 ff.), this view was later modified: in the 'realistic' books IV–VI examples are drawn from the constitutions of barbarian *ἔθνη*, and in VII ch. 7 Aristotle envisages the possibility of the Greeks being united in a single *πολιτεία*. Weil believes that contemporary events, combined with Aristotle's increasing historical research, led him to recognise that the classical *πόλις* is not the only conceivable political entity.

Weil's careful study of the illustrative material used in the *Politics* confirms, in general, Jaeger's thesis concerning the relative order of the books. His discussion of Aristotle's theory of the state has the merit of pointing out that this theory was both more flexible and more realistic than the dogmatic pronouncements in II and VII on the subject of the size of the *πόλις* would at first sight suggest.

G. E. R. LLOYD.

RANDALL (J. H.) *Jr. Aristotle*. New York: Columbia University Press (London: Oxford U.P.). 1960. Pp. xv + 309. £2.

This book might seem at first sight to be a general introduction to Aristotle's philosophy intended for the beginner; it surveys the whole of Aristotle's work from *Metaphysics* to *Rhetoric*, with one chapter or more to each subject, it gives a sketch of Aristotle's life and an account of the problem of his development, and it is designed for the Greekless reader. (Greek terms are used profusely, but always in transliteration.) But the reader soon discovers that this is not a straightforward exposition of Aristotle's thought; it is a re-interpretation by a scholar who approaches Aristotle with the problems of modern philosophy in mind.

Randall's intention, stated in the Foreword, is 'to point out some of the ways in which Aristotle's thought is relevant and suggestive for two of the most important present-day philosophical concerns, the analysis of language and the analysis of natural processes', and in practice Randall is largely concerned to expound Aristotle and interpret him in the light of later philosophers, both medieval and modern. This is a very enlightening procedure, especially for those trained as classical scholars rather than as philosophers, but it has two dangers: it sometimes smothers Aristotle in modern jargon, and it sometimes takes too much for granted. Often the statement of a problem in modern terms throws much light on Aristotle's approach to the same problem and helps one to understand what Aristotle was trying to do; an excellent example is the comparison with Spinoza (p. 119), which does much to explain the point of Aristotle's tortuous discussion of essence. But often Randall is far too allusive, so that the reader who does not already understand the problem gains nothing from the comparison, which he has to take on trust without seeing its point; for example, the mention of Hume

on p. 185 does not explain Hume's position, and consequently does not help one to understand Aristotle's. Nor do labels like 'formalistic naturalism' and 'structuralistic functionalism' (p. 295) add much to our understanding of Aristotle without a discussion of their meaning.

In expounding Aristotle Randall aims not so much at providing a detailed account as at revealing the general principles that underlie the whole of Aristotle's thought. This is an excellent aim, for it is all too easy to fill one's head with the details of Aristotle's system without looking beyond them. Randall discusses the nature of the problems that interest Aristotle, the way in which he tackles them, and the kind of answer that he gives to them. Another great merit of this treatment is that it does not divide Aristotle's thought rigidly into self-contained, mutually exclusive departments. Randall shows how the same principles operate in Aristotle's treatment of all subjects and how his ideas on different subjects interact. (This synoptic approach has one disadvantage: it sometimes leads to an annoying repetitiveness.) The two principles that he finds most important in Aristotle are his 'functionalism' and his 'contextualism', that is to say, his conviction that things must be understood in terms of their activity or function and in relation to their environment or context. These Randall considers more interesting than what he calls Aristotle's 'formalism', commonly regarded as his chief characteristic.

As is inevitable in a book of this size, many important problems which in fact are open to discussion are treated briefly and dogmatically (e.g. the development of Aristotle's view of metaphysics). Randall's view of the Unmoved Mover (pp. 137–44) is very unconvincing: having made the surprising statement that Aristotle 'came in his maturity to maintain that "pure forms" could not exist' (p. 97), Randall tries to explain away the Unmoved Mover as a 'Platonic myth' (p. 141) and argues that 'God is the form of the world's matter' (p. 143). It is difficult to accept his contention that the Unmoved Mover is the formal cause of motion (p. 135); his interpretation of *ἀρχή κινήσεως* in this context as 'principle of the intelligibility of motion' neglects the fact that *ἀρχή κινήσεως* or *μεταβολῆς* is one of Aristotle's usual terms for efficient cause. Nor is it true to say (p. 228) that final and efficient cause are never identical; *ἄνθρωπος γὰρ ἄνθρωπον γεννᾷ* (the dictum is used by Aristotle at *Physics* 198 a26 to make this very point, that final, formal, and efficient cause may be the same). But these are minor criticisms; viewed as a whole, this book is a valuable and stimulating contribution to the study of Aristotle.

H. J. EASTERLING.

ARISTOTLE. *De anima*. Ed. D. Ross. Oxford: the Clarendon Press. 1961. Pp. vii + 338. £2 10s. 0d.

There has been no English edition of Aristotle's *De Anima* since that of R. D. Hicks in 1907, and com-

mentary on this work has also been scarce during the last fifty years. A new edition is overdue, and the present one by Sir David Ross is welcome. It must, however, be confessed that the commentary is not nearly so full as that of Hicks, and for that reason alone it cannot completely take the place of the earlier work.

The present edition follows the familiar plan of Ross's other editions of Aristotle's works. There is an introduction dealing with the text and the dating of the work and also giving a discursive account of Aristotle's views on the soul. The text itself takes due account of other editions and includes a full *apparatus criticus*. There are quite a number of differences between the text adopted here and that given in the recent Oxford Classical Text, and it is clear that Ross has had a number of second thoughts. There follows the commentary, in which each section is preceded by a summary which is, in most cases, tantamount to a translation. There is a Greek index but no English index, and there is a very select bibliography.

A substantial proportion of the commentary is given over to textual matters. Ross's emendations seem on the whole sensible, if sometimes arguable, although it might be maintained that they make very little difference in the long run to our understanding of the work. He dates it during the period after 325/4; he does not here repeat the suggestion that Book III might have been written earlier than Book II, but states that the state of the text and the arrangement of Book III suggests that it may have been left by Aristotle in a relatively unfinished state. He also suggests that section 7 of this book may have been added by an editor.

Apart from textual matters, there is surprisingly little discussion of substantial issues in the commentary, although most of these issues are discussed more fully in the Introduction. The relegation of discussion to the Introduction has serious disadvantages for the book as a work of reference. These disadvantages are increased by a decided tendency to gloss over important issues. There is no discussion, for example, of the special meaning of *πρώτη ἐντελέχεια* in the definition of the soul, or of the infallibility of the special senses in connexion with their proper objects—to mention only two issues.

Ross's main interest is perhaps in the *νοῦς ποιητικός* of Book III, and he clearly sees a good deal of the discussion as leading towards this notion. He accepts without argument the 'transcendent' view of the last sentence of III, 5, identifying the detached active reason with ourselves. He also makes various suggestions about the functions performed by the active reason—suggestions which, as far as I am aware, have no foundation in Aristotle's remarks, and are, indeed, probably foreign to Aristotle's purpose. In all this the lack of reference to Hicks's extensive discussions is very evident.

Ross's treatment of the *κοινή αἴσθησις* is also open to criticism, although he adds nothing new here to what he has said on the subject elsewhere. He repeats

the view that it is not a sense (although what Aristotle says suggests a quite contrary view) and makes it responsible for a number of different functions. It is worth pointing out that Aristotle does not in the *De Anima* make the *κοινή αἴσθησις* responsible for any other function than the perception of the common sensibles, although he may do so elsewhere, e.g. in the *Parva Naturalia*. Ross is misleading on this point, and even more so when he makes it responsible for the perception of the incidental sensibles. As far as I can see, Aristotle never says this.

There is throughout the discussion a Cook-Wilsonian emphasis on *apprehension* which is not properly Aristotelian. Thus the analogy between sense-perception and reason offered at the beginning of III, 4 is, according to Ross, based on their both being forms of apprehension. The truth is surely that they are both, in an important but difficult sense, *ἀπαθείς*. The identity of knowledge and the known is similarly made a matter of the known being reflected exactly in the mind of the knower. I suggest that Aristotle's thesis ought to be taken more seriously and literally.

Despite these criticisms it is useful to have the edition, especially since Hicks's work (old as it is) is not easy to obtain. There are a few misprints. P. 33, n. 3 should presumably read '424a15'; on p. 167, 'b13-14' should read 'b3-4'; on p. 232 the note on 416b31 should read 'ἐν ---- λόγους'; the text of 417b14 does not agree with the suggestion given in the commentary: the reference in the commentary to 419b5 should separate the words 'ὁ μὲν' and 'δύναμις'; at 425a17 Ross excludes the first occurrence of 'κινήσει' but the summary reads as if it is included and the sentence has in any case a doubtful sense without it; finally on p. 296 the commentary on 430b6-30 in section 6 of Book III is included in the commentary on section 5 and before the commentary on the earlier parts of section 6.

D. W. HAMLYN.

ORIGENES. *Origenes der Neuplatoniker*. Ed. K.-O. Weber. (Zetemata, 27.) Munich: C. H. Beck. 1962. Pp. xvi + 164. DM 24.

The great majority of scholars who have concerned themselves with the question now agree that there were two Origenes, the Christian theologian and the pagan Platonist mentioned in Porphyry's *Life of Plotinus* chs. 3, 14 and 20 (the passage quoted from Longinus), whose opinions are alluded to several times by later Neoplatonists. Weber gives a useful survey of the evidence and the discussions on this point in his ch. II. The pagan Origen, when thus clearly distinguished from his great Christian contemporary, does not perhaps appear as a very important or influential philosopher. But he is worth studying because the wide divergence between his recorded opinions and those of his fellow-pupil of Ammonius, Plotinus, indicates the range and variety of third-century Platonism, about which we

know so little that we tend to think of Plotinus as more representative and less isolated and original than he really was. The most striking difference between the systems of the two pupils of Ammonius is that Plotinus made his first principle the One beyond intellect and being, but Origen, in the older Platonic manner, made his the primary intellect and being, and, apparently, set great store by the reconciliation of the theologies of Plato and Aristotle which this made possible. This does not make him at all an original thinker, but he is interesting as continuing in the age of Plotinus an older way of thinking which seems to have influenced some later Platonists.

Weber has therefore done a useful piece of work in collecting and commenting on the seventeen texts in later writers in which Origen the Platonist is mentioned (he calls them 'Fragments' in the misleading way which is, unfortunately, becoming common). His discussion of these passages is clear, systematic and interesting. But his work would have been much better (though, perhaps, not long enough to make a book) if he had not allowed his enthusiasm for his subject and a natural veneration for his teacher, Professor Langerbeck, to carry him a long way beyond the evidence. He follows Langerbeck (cf. *JHS* lxxvii, 1957, 67-74) in claiming to know far too much about Ammonius. I can still see no reason for abandoning the sober agnosticism of E. R. Dodds, who ends his careful examination of the evidence and the theories, including Langerbeck's, about the teacher of Origen and Plotinus with the words 'And so for me, as for M. Theiler, Ammonius is still, alas, "ein grosser Schatten" and nothing more'. (E. R. Dodds, *Numenius and Ammonius* iv 24-32, in *Entretiens Hardt V* (Les Sources de Plotin) Vandoeuvres, 1960). But for Weber Ammonius is the originator, and Origen the worthy continuator and champion, of a noble and orthodox natural theology which he feels it incumbent on him to defend passionately against the heretical aberrations of Plotinus. How far he is prepared to go, and by what methods, in filling in the gaps in our scanty knowledge of Origen's theology, can be seen by comparing the very curious summary on pp. 111-13 with the texts printed on pp. 3-12 (there seems to me to be nothing in between which justifies the attribution to Origen of some of the doctrines contained in the summary, particularly that God loves the world). And his discussion of the One of Plotinus (especially pp. 108-11) has a note of *odium theologicum* which is regrettable.

A. H. ARMSTRONG.

volumes. He lived to complete only this second volume; it is good to know that Harvard University Press is planning to continue the series so that although the completed work will be different from what Sarton would have made it, it will nevertheless, one hopes, do what he wished it to do: to illuminate 'the progress of mankind' by describing the growth of positive science and of scientific ideas.

The period with which this volume deals is, of course, one of the most fertile in the scientific history of mankind; at any rate, the first of the three centuries between Euclid and the beginning of the Christian era was not equalled for scientific brilliance before the seventeenth century. Euclid, Aristarchus, Archimedes, Apollonius, Eratosthenes, inaugurated a golden age in the history of mathematics; and if the next two centuries were perhaps an anticlimax in comparison, one must not forget names such as that of Hipparchus. And, of course, the growth of mathematics and the mathematical sciences was not an isolated phenomenon: medicine, too, had its golden age in Hellenistic times. That this was not an accident of history is well brought out by Sarton. He gives us a good résumé of the historical background of Alexandrian Hellenism and discusses such questions as that of oriental influence on Greek science and Greek influence on the non-Hellenic East. He thinks that we must not make too much of such possible influences, and he points out that religious beliefs, literary conceits and artistic motifs are more contagious than science, especially abstract science, and that superstitions such as astrology travel more lightly and therefore more easily than real science. In any case, the best that Egypt and Babylonia had had to offer had already been assimilated into Greek science at an earlier time. What is equally interesting is the comparative lack of evidence for large-scale transmission of scientific ideas in the opposite direction. There were exceptions such as the astronomer Seleucus who apparently explained Aristarchan astronomical views in Babylonia. But there seems to have been little knowledge of Greek astronomy in India, where Greek astronomical influences appear to be chiefly later than Ptolemy. Sarton thinks that the reason for this is that Greek emigrants were too few (?) in pre-Christian times, and that they were too little interested in science and scholarship to affect Eastern minds.

Sarton shows how considerable was the influence of institutional research (particularly in the Museum) upon the progress of science. He points to the enlightened patronage of the early Ptolemies, the complete freedom with which members of the Museum were allowed to pursue their investigations, the organisation of collective research without political or religious directives, the importance of the cosmopolitan atmosphere of Alexandria which enabled scientists to take advantage of work done before them and by their contemporaries elsewhere. Altogether Sarton seems to me to be right in stressing the importance of the Museum, and, of course, of the Library, on which he has an interesting discussion;

SARTON (G.) **A History of Science. Hellenistic Science and Culture in the Last Three Centuries B.C.** Cambridge, Mass: Harvard University Press (London: Oxford U.P.). 1959. Pp. xxvi + 554. 112 text figures. £3 3s. od.

The late Professor George Sarton had planned a monumental history of science in eight or nine

though one must not forget that there were other places where science and scholarship were pursued.

In a book so full of such varied material as this it is inevitable that some mistakes should insinuate themselves, and even more that some controversial issues should be treated in a way suggesting that they are really much less complicated than they are.

In discussing Euclid Sarton seems to suggest that the definitions of Book I are fundamental and sufficient for the whole of the *Elements*; this, of course, is not true. There are definitions in the other books too, as one would expect, particularly in those books that deal with fields other than plane geometry; where there is no list of definitions, as in VIII and IX and XII and XIII, the reason is that in both cases these books follow upon one that contains all the requisite definitions for the field in question, viz. VII for arithmetic, or XI for solid geometry.

Sarton has much to say about the importance of the fifth postulate (Book I) though he does not really explain what it is that makes it so important in the Euclidean scheme of things. He gives a very good account of the later history of the Euclidean tradition.

One would have liked a little more discussion of the historical problem why Aristarchus' astronomy was rejected in antiquity. In connexion with the estimate by Aristarchus of the angular diameter of the moon as 2° , Sarton ought perhaps to have mentioned the various attempts (e.g. by Tannery and Manitius) to reconcile this crude estimate with the much better one of $30'$ ascribed to Aristarchus by Archimedes in the *Sandreckoner*.

There is a very lucid account of Hipparchus' elaboration of tables of chords; a clear explanation of the relation of chords to sines; these would do honour to any textbook of mathematics. And earlier, in discussing the various numerical evaluations of Eratosthenes' result in measuring the circumference of the earth Sarton makes the important point that possible errors in the measurements do not matter very much. 'Eratosthenes' achievement lies in his method... correctness of his results was partly accidental for it was based upon very inadequate measurements.'

There are some annoying mistakes, or, at any rate, inconsistencies, possibly due to the fact that the author did not live to see the volume through the press. In the discussion of Eratosthenes' 'Sieve' we are given, on two consecutive pages, two different numbers as the largest primes discovered to this day; and of Archimedes we are told in one place that 'he is one of the ancestors of the infinitesimal calculus' while a few pages before this we read that 'it is foolish to speak of Archimedes as a forerunner of the inventors of the integral calculus'. The lack of final polish becomes evident also in occasional overwriting and in some odd repetitions. But there is much that is invaluable in this work. For Sarton the history of science was more than simply an account of discoveries. The tradition of ancient science is as

important to him and as interesting as its invention. He tries to explain not only the ancient achievements but also their transmission. Hence this book is full of the most valuable bibliographical information. It also has many beautiful illustrations of early printed editions. Specialists in the field may also find it a wholesome feature of this book that Sarton did not narrowly concentrate on any one field of science or indeed on science alone: the broad sweep of his interests and his wide sympathies matched the similar absence of narrow specialisation in not a few of the great men of the Hellenistic age whose work he described.

A. WASSERSTEIN.

SAMBURSKY (S.) *Physics of the Stoics*. London: Routledge and Kegan Paul. 1959. Pp. xi + 155. £1 3s. 6d.

The author of this book, who is Professor of the History and Philosophy of Science in the Hebrew University of Jerusalem, brings to his task the interests of the scholar and the equipment of the scientist. A physicist by training, he has made physical science in antiquity the special subject of his study. His preoccupations, however, fit into a framework less narrow than one might think. He is equally interested in the conceptual structure of scientific doctrines and in their historical filiations. In this book he gives a more detailed exposition and discussion of Stoic physics than was possible in the relevant chapters of his earlier work *The Physical World of the Greeks*.

Stoic physics is a field that has been somewhat neglected—not surprisingly, perhaps, since atomism has been in the foreground of scientific interest; Stoic physics is based on the continuum concept, a concept diametrically opposed to the fundamental doctrines of atomism. Sambursky points out that though physics is on the whole less important than logic in the teaching of the Stoics, 'they developed a highly consistent system of physical concepts and applied it to the whole body of their teaching'. The essential feature of the Stoic physical system, the dynamic concept of continuity, makes it one of the great original contributions to physical thought. Sambursky thinks that we find already in the older Stoa a first grasp of the modern mathematical notions of the function and the limit, and that this 'constitutes the first break through the barriers of the merely static contemplation of mathematical quantities'. (This claim is, perhaps, a little too large; Antiphon and Bryson in the fifth century, and Eudoxus in the fourth century, surely had something to do with the emergence of the limit concept?)

It is Sambursky's aim in this work to describe the main aspects of the Stoic continuum theory, to trace the origins of some of its notions in earlier science and philosophy; physical teachings of the Stoa which are of no relevance to the continuum theory are deliberately omitted or dealt with only superficially. Some of the features of the continuum concept are traced back to earlier doctrines such as the theory of total mixture of Anaxagoras, and the specific dynamic

function of air and fire to Heraclitus and Diogenes of Apollonia. But the main body of the continuum theory was developed by the older Stoics, particularly by Zeno and Chrysippus (who, it is not idle to remind ourselves, was a contemporary of Archimedes and Eratosthenes) in the fourth and third centuries. The most important later additions to the doctrine are due to Posidonius. After that the centre of interest shifts to ethics, and it appears that no further important contribution to physical thought was made by the later Stoa.

Sambursky usefully points out that, like the atomic theory, the Stoic continuum theory was essentially speculative, based on theoretical conceptions and independent of any recourse to systematic experimentation.

Atomic theory had been based on two fundamental entities: the atoms and the void; the continuum theory on unformed matter and on *pneuma*. The atoms were discrete and solid, each one a complete plenum, moving in the void. A complete mixture of atom with atom or of atom with void was impossible. The continuum doctrine on the other hand postulated a total mixture of *hyle* and *pneuma*. The role of the latter of producing cohesion in the physical world is traced by Sambursky to pre-Socratic sources (Anaximenes, the Pythagoreans, Empedocles). The *pneuma* pervading the whole universe makes it into a single cohesive unit 'and thus the *pneuma* becomes the first version of the aether with all the characteristic functions ascribed to it from the seventeenth century onwards'. By identifying the continuous and the cohesive the Stoics completed the transformation of the geometrical concept of continuity into the analogous physical one.

Besides being a binding force *pneuma* is the agent which generates all the physical qualities of matter. By their conception of the *pneuma* as the generator of the physical qualities the Stoics generalised their continuum theory into a field theory; the *pneuma* is the physical field which is the carrier of all specific properties of material bodies. The mixture of *pneuma* with inert matter imbues the latter with physical properties, whereas *pneuma* itself is a mixture of two components, fire and air. Hence the Stoics were occupied to a large extent with the problem of mixture. Sambursky offers an extraordinarily good and lucid discussion of the Stoic concepts of mixture and of ancient misunderstandings and criticisms of it. '... Taking a radical position with regard to continuity the Stoics conceived of mixture as a complete interpenetration of the components which exist simultaneously in the given proportions down to the most minute elements of volume ... every element of volume, however small, would be homogeneous with regard to the mixing of the components.' Ancient critics such as Alexander Aphrod. and others saw in the notion of total mixture an infringement of the principle that one body cannot occupy the place occupied by another.

In discussing the movement of *pneuma* in the body Sambursky makes the interesting point that the Stoics

probably conceived of this movement as being propagated in wave fashion, not as a translation of particles. It seems certain at any rate that movement of air and of sound were thought of in this way by some Stoics. A small point: 'Archytas used the term *oxys* (fast) in contradistinction to *barys* (slow) in his theory of sound to explain the generation of high-pitched and low-pitched notes by quick and slow motions.' This seems to me to be based on a slight misunderstanding of the text of Archytas (47 B.1. Diels). *Oxys* and *barys*, of course, do not mean fast and slow, but high-pitched and low-pitched; fastness and slowness are indeed used to explain the relative pitch in sounds, but the terms employed are *tachys* and *bradys*.

In a very good discussion of *τοική κίνησις* Sambursky gives reasons for being tempted to identify this concept with that of wave propagation in modern physics. But lucid and plausible as Sambursky is here and on many other similar points, one cannot help feeling that he is, perhaps, too often tempted to find parallels to and similarities with the concepts of much later periods. One wonders how useful this really is in understanding the historical significance of ancient doctrines. Is it even useful in understanding the doctrines themselves? It might be argued that by being too easily willing to compare ancient systems and doctrines with their modern counterparts we might be in danger of misunderstanding, not only their *historical role* but also their *conceptual framework*. On the whole Professor Sambursky has overcome this danger magnificently; but some of his readers whose grasp of the scientific concepts involved is not as firm as his, may be tempted into error. Sambursky's extrapolation from the Stoic notion of continuity to the modern concept of a field of force seems, well argued as it is, to be a case in point.

How useful is it to suggest, as Sambursky does, that the omnipresence of *pneuma* within matter and within the apparent emptiness of the space between bodies is the prototype of the concept of field of force as it was developed in nineteenth-century physics? Sambursky himself points to the significant difference that in modern mathematical physics the field concept is entirely stripped of any substantiality in the purely material sense of the word. Nevertheless, Sambursky puts an extremely interesting argument: the *pneuma* concept is the ancestor of aether; this, from the seventeenth century on, served as a medium to propagate motion, and to account for cohesion, and for 'non-mechanical' phenomena like electricity and magnetism. These notions were developed into the mathematical concept of the field and the forces in it.

Incidentally, is Sambursky right in illustrating his undoubtedly true thesis (that *pneuma* and aether were confused) by a quotation from Cicero, *de natura deorum* II. 66 'Air resembles aether and is closely connected with it'? (the 'connected' here is due to a play on words *coniux-coniunctio*; though attributed to the Stoics, this does not really say anything about the *pneuma*, at least not on the face of it). The

confusion between *aer* and *aether* and the juxtaposition of the two is, of course, older than the Stoics. Nevertheless, Sambursky may well be right in suggesting that it was the Stoic theory of *pneuma* which was responsible for the change in the usage of the term *aether* which became so important in the history of science from the beginning of the seventeenth century on. At any rate the similarities between the concept of *pneuma* and that of *aether* are striking; and Sambursky does not, of course, forget to discuss the differences. (In the same chapter there is another small point that may arouse some disagreement. In connexion with Stoic explanations of tides through the moon Sambursky says that 'Stoic theory for the first time implied a causal description of phenomena on a cosmic scale'. But extra-terrestrial causation of tides was thought of earlier. We are told that Aristotle and Heracleides thought the sun caused the tides; so certainly did Dicaearchus; and Pytheas thought of the moon in this connexion before Posidonius.)

In discussing Stoic notions of causality Sambursky traces these to the tradition established by the medical discipline of repeated observation of symptoms. He shows that the Stoic theory of causality was largely determined by the notion of continuity which dominated their physics and equally by their doctrine that everything capable of acting and of being acted upon must be a body. Physical events must be the result of direct contact of bodies (or of *pneuma*). Contiguity, therefore, becomes inseparable from causality; causes are bodies acting upon other bodies either directly or through the *pneuma*. The Stoics realised that in any given instance one had to reckon with a multiplicity of causes; but, given that multiplicity of causes, 'the Stoics postulate about all of them that whenever the same circumstances prevail with regard to the cause and the things affected by the cause, it is impossible that sometimes the result should be this and sometimes that; otherwise there would exist some uncaused motion' (Alexander). Precisely as there is no *creatio ex nihilo* there is no uncaused event, no spontaneity; 'chance' is defined as (and explained by) 'hidden causes'. (One might point here to a similarity with arguments put forward by some modern scientists who are sceptical about some of the alleged logical and philosophical consequences of the uncertainty principle in physics; they too talk about 'hidden variables'.) Thus the Stoics arrived at their conception of a chain of causes taking in the whole of space and time; and this is directly linked to their continuum doctrine: everything that happens is caused by one thing and causes another 'because there is nothing in the cosmos which is separated and divorced from all that happened before'. Hence the Stoic attitude to the Epicurean notion of the deviation of atoms. The account of the Stoic doctrine of causality includes a discussion of Stoic attempts to reconcile determinism and free will.

There follows an account of the Stoic doctrine of divination and induction. Here he discusses, *inter alia*, a passage from Sextus Empiricus (*Adv.*

Math. ix 132) *μαντική . . . ἐπιστήμη οὐσα θεωρητικὴ καὶ ἐξηγητικὴ τῶν ἐπὶ θεῶν ἀνθρώποις διδομένων σημείων*. (is this really Chrysippus anyway?). He translates (with Bury): 'the science which observes and interprets the signs. . .'. S. is right in describing this kind of *mantike* as based on observation and therefore akin to induction (see on this also Hopfner *s.v.* *Mantike* in Pauly-Wissowa, particularly col. 1276 ff.); but I am not convinced that the word *θεωρητικὴ* here is necessarily to be translated by 'based on observation' which Sambursky gives in agreement with Bury. Its connexions with episteme elsewhere tell against this translation and make it at least arguable that it may mean, more probably, 'speculative' rather than 'based on observation'. Compare Plato *def.* 414 b 3.

One may perhaps also disagree with Sambursky's contention that 'the whole problem of divination occupied the Stoics not so much for practical reasons but was of primarily theoretical or scientific interest to them, for the very reason that they had to accept the validity of divination in a deterministic world and at the same time saw in it a confirmation of determinism by inductive inference'. What seems established is that belief in divination fitted into the Stoic framework of determinism; but whether the analogy of inductive processes in science and in divination is really valid, whether, that is to say, we are really entitled to see in the Stoic belief in divination and in their arguments for it more than a merely schematic analogue to inductive reasoning in science is another matter. I must admit though that, as one reads and re-reads Sambursky's exposition of this question, one becomes less unconvinced as one goes on. There follows an extraordinarily interesting discussion of Stoic notions of the possible in relation to the contingent and the necessary, of how their notion of the possible is related to that of their predecessors, and of how it fits into a deterministic scheme of things. If the 'possible' means an *objective* contingency in a non-deterministic world, it obviously had to be banished from the Stoic universe. The Stoics therefore made the 'possible' into a *subjective* category basing it on human ignorance of the future; for those who are able to know the full causal nexus the 'possible' does not exist. This conception of the possible, according to Sambursky, on the one hand led to a deeper comprehension of causality, and on the other clarified for the first time the problem of disjunctive propositions containing statements subject to empirical verification (and here we come to questions of the greatest interest to students of Stoic logic).

In a particularly interesting discussion of Stoic approaches to a concept of function (he quotes here, *inter alia*, Sextus Empiricus, *Adv. Math.* ix 249 where the language is almost modern in describing a function with two variables) Sambursky argues that it was the lack of graphic representation and the failure of Greek mathematics to develop a proper algebraic notation that prevented a further advance of functional thinking. In this connexion he points out that the Stoics introduced a sort of physicalisation

of geometry by endowing geometrical figures with the elastic properties of material bodies. And with reference to Simplicius' criticism that Stoic mathematical concepts would destroy the essence of mathematics which is static and free from change, Sambursky remarks that it is precisely the non-static approach of the Stoics that brought them a considerable step nearer to the understanding of the variable and the function. Sambursky draws attention, most pertinently, to the fact that Stoic physics matured during the golden period of Hellenistic science; he points to Archimedes' method of investigating certain mathematical problems by mechanical means as being similar in trend to the Stoic 'physicalisation of mathematics'.

On the notion of the infinite: in this connexion he also discusses Greek methods of computing areas and volumes. He is perhaps a little too trusting in the literal accuracy of Themistius' report of Bryson's argument on the quadrature of the circle; that Bryson thought that the circle was equal to the arithmetic mean of the inscribed and the circumscribed polygon is not quite so certain as Sambursky seems to believe; indeed, it is conceivable that a different view of Bryson's argument (namely one based on assuming that he was talking sense) would make it possible to see in him a precursor of Stoic notions of a range of values varying *continuously* (seeing, in other words, the area of the circle as lying on a range between two values, that of the inscribed and that of the circumscribed polygons, one of which *increases through all intermediate values* towards the other). If this is a tenable view then we have, in Bryson's quadrature, an ancient prefiguration of Dedekind's Stetigkeitsprinzip, which would fit well into the prehistory of the Stoic notion of the continuum as traced out by Sambursky. (See on this Philoponus in *An. Post.* 112, 20-4; and *Phronesis* volume ix (1959) p. 98.)

In his discussion of Democritus' famous cone paradox Sambursky accepts the interpretation (of Heath and others) according to which this is no more than a paradox and a dilemma; he is obviously right in saying that 'there is no answer to Democritus' dilemma within the static concepts of atomic lengths'; but should that not suggest to us a completely different conclusion from that reached by Sambursky, who says: 'It is Chrysippus to whom the credit is due of having first grasped the concept of the limit?' I should like to make two points here:

(1) It seems clear to me that Democritus' 'dilemma' was in fact put forward as a *reductio ad absurdum* argument. If so, it follows that Democritus meant to suggest that the notion of atomic lengths in mathematics as analogous to discrete atomic units in physics had to be abandoned. That such an intention should be ascribed to the atomist Democritus is not as fanciful as it may sound; see on this Simplicius (in *Phys.* 81.34-82.6) and Scholia in *Arist.* p. 469 b. 14. Brandis.

(2) Is it really right to say that Chrysippus is the first to grasp the concept of the limit? Antiphon

(and perhaps Bryson) long before Chrysippus talked in terms that entitle them to at least some credit in the history of this most fruitful mathematical concept.

Plutarch, *de Comm. Not.* 1078 e may be thought to show that the Stoics 'discarded the conception of a distinct surface of a body'; though I have my doubts about it; I rather suspect that what is reported there as Stoic doctrine is no more than that there is no greatest and no smallest body and that all bodies are infinitely divisible: but, however that may be, even on Sambursky's interpretation, there is no warrant *here* to say that the Stoics replaced it (the discarded conception of the distinct surface of a body) by an infinite sequence of boundaries defining the surfaces of inscribed and circumscribed figures which converge from both sides to the figure in question and thus define it as a dynamic entity. On the other hand, Sambursky is clearly right in saying that the Stoics laid the first foundations of the theory of sets; he points out that the main characteristic of the infinite set—the fact that it contains subsets which are equivalent to the whole—was known to the Stoics and formulated by them (*cf.* *Plut. de. Comm. Not.* 1079a). Though, in this passage too, one might see a greater dependence on Pre-Socratic thought than Sambursky is inclined to do; he discusses Anaxagoras fragment 3 (Diels) (if I understand him correctly) as not speaking about a *mathematical* concept of infinite divisibility at all; yet surely what we have there is in fact material for a sort of substitute definition of 'infinity' such as underlies the common use of the concept in Greek mathematics. Thus a Greek, instead of saying 'there is an infinity of x's' would say 'the number of x's is greater than any assigned number' and instead of saying 'the difference between a and b can be made infinitely small', would say 'the difference between a and b can be made smaller than any assigned quantity'. This comes to the same as Anaxagoras' formulation: 'In the case of the small there is no smallest—there is always something smaller; . . . in the case of the great [there is no greatest, for] there is always something greater.'

In his discussion of Stoic explanations of the state of rest of the earth in the centre of the world Sambursky writes that whereas in earlier cosmologies the earth's state of rest was explained by considerations of symmetry, in Stoic cosmology 'geometrical symmetry as a cause of rest was replaced by the picture of a symmetrical action of forces upon the spherically shaped earth'.

How true is this? Sambursky quotes a passage (Achill. *Isagoge* IV) which contains, almost as an afterthought, an example of a dynamic equilibrium of outside forces. But is this enough to establish this as the authentic Stoic cosmology? Most of the other passages bearing on this (collected by von Arnim ii 173 ff.) suggest more conventional explanations of terrestrial stability at the centre of the world and where there is a question of an equilibrium it is one of elements *inside* the cosmos. In quoting this last passage from Achilles, Sambursky most usefully points out that the word *ισοστάτης* used here appears also

in the third proposition of Archimedes' first book *On Floating Bodies*; the parallel is very suggestive indeed; and Sambursky is surely right in thinking that Chrysippus may have had in mind that the cosmos is mixed of heavy and light elements in a proportion which balances their 'upward' and 'downward' tendencies, and that this is analogous to the suspended floating in a fluid of a body whose specific weight is equal to that of the fluid. (The equation of 'downward' or 'below' and 'at or towards the centre' is older than the Stoics; and so is the conception of the centripetal tendency of 'heavy' bodies.)

In a relatively short review it has not been possible to do justice to the real qualities of this work: its lucidity and its thoroughness are beyond praise; where the reader may be inclined to disagree with the author he will do so almost always because he has been stimulated by Sambursky's argument to think about the problem afresh. No scholar interested in Stoicism or in the history of scientific ideas in antiquity will be able to neglect this most valuable work.

A. WASSERSTEIN.

GUTHRIE (W. K. C.) **The Religion and Mythology of the Greeks.** Cambridge Ancient History (Revised edition) Vol. II, Chapter XL. Cambridge: University Press. 1961. Pp. 55. 6s.; \$1.25.

In this fascicle from the new Cambridge Ancient History, which has been issued separately for the convenience of scholars, Professor Guthrie discusses Greek religion and mythology against the Minoan-Mycenaean background and their subsequent history in Homer and Hesiod. The chapter is divided into six sections, the first of which deals with the formation of Greek religion. Despite the obvious distinction between heavenly and chthonian cults it would be a mistake in G.'s view to 'assume that the historical Greeks were composed of two, and only two sharply contrasted racial elements'. These merely represent the 'warp and woof of the fabric upon which, in later centuries, was embroidered the intricate and many-coloured pattern of Greek religion'.

The second and third sections reveal what is known about the Minoan-Mycenaean religion and its relationship to the Greek without attempting to conceal any of the difficulties. The decipherment of Linear B, which for G. is 'in principle accomplished', has added little to our knowledge, which still depends mainly upon individual interpretations of archaeological material whose purport is still often far from clear. Nevertheless, the evidence for the existence of a bull-cult in Minoan Crete and the worship of both male and female deities of fertility seems overwhelming. It does not, however, follow because they were influenced by their art that the Mycenaeans slavishly adopted the Minoan religion. Such 'adjectival' titles as Pasiphae, Ariadne, Phaedra or Glaucus seem to mark the effect of Greek polytheism on the original deities of Crete. On the other hand, the survival of the Minoan palace-cult has been virtually proved at Eleusis, Athens and elsewhere.

Though noting the resemblances between Cronus and Marduk or Kumarbi in the section devoted to theogonical myths G. reminds us that such 'tales learned from the Orient may only be the mould of expression in which they (sc. the Greeks) cast memories of their own composite history'.

Section V deals with the Homeric religion. Though 'not very lofty' in some respects the conception of the gods as guardians in battle or embodiments of conscience was of cardinal importance in a warrior society concerned wholly with the glory of the here and now, and not with the after-life.

The final section describes the gods of Olympus in brief but masterly fashion. The views given are mostly orthodox, perhaps a trifle too much so in the case of Poseidon, whose origins have never been satisfactorily determined. That Apollo was a Dorian god all may not agree, but it is true that 'there is a strangeness, or foreignness about him' which is curiously at odds with the normal view of him as the 'embodiment of the Hellenic spirit'.

The chapter ends with a useful biography and its print and format, as might be expected, are all that could be desired. The difficulty of giving a balanced view of a subject which notoriously bristles with difficulties is well known. That Professor Guthrie has succeeded in doing so is a tribute both to the breadth and depth of his scholarship as well as to the sanity of his judgments.

JOHN POLLARD.

COMPERNOLLE (R. VAN). **Étude de chronologie et d'historiographie siciliotes. Recherches sur le système chronologique des sources de Thucydide concernant la fondation des colonies siciliotes.** [Études de philologie, d'archéologie et d'histoire ancienne publiées par l'Institut Belge de Rome, tome V]. Brussels-Rome. 1960. Pp. xx + 603. 2 maps. B.Fr. 320.

The reader's first impression is that this is an immensely prolix book. The 'Prolegomena', which quantitatively outweigh the legomena, include 75 pages on the Mermaid kings of Lydia and 95 on traditions relating to the foundation-date of Carthage, with incidental discussion of the route taken by the Samians to Zankle and the date of Marathon. The exposition is full and leisurely, abounding in recapitulation and even in repetition (e.g. p. 306 n. 1~ p. 307, p. 335 n. 3~ 324 ff.). Yet as the architecture of the book emerges, it compels admiration. Observation of the opportunities for digression which are not taken reveals the disciplined sense of relevance which underlies the Prolegomena. The fullness of the exposition is never marred by rhetoric or shoddy reasoning; it is inexhaustibly sensible and candid, and so lucid that there is no point at which any reasonably attentive reader can lose the thread of the argument; and that, in a book on such a subject and on such a scale, is a remarkable achievement.

The author, however, shares with many Continental scholars excessive scruples in doxography and a reluctance to discriminate in bibliography. Pp. 466 ff. are a horrifying example—matters may soon reach a point at which it will be thought improper to make an historical statement without listing all the scholars who have not disagreed with it—and it is surely unnecessary to cast a discussion of the chronology of the Deinomenidai in the form of an argument against Pareti (pp. 261–317) once the basis of Pareti's case has been neatly removed (p. 268) by the correct interpretation of Diod. xi 38.7 in the light of Diodoros' usage.

Van Compernelle's thesis is that Thucydides' dates for the Sicilian colonies—derived from Antiochos, since there are positive arguments against every other putative source—rest solely (with the exception of the Naxos–Syracuse interval) on calculations which converted genealogies into years by using a single generation-value, 35 years. This value is plausible a priori, given the theory of hebdomads propounded by Solon (fr. 19, cf. Hdt. i 32.2), and van Compernelle makes a strong case for its use in some dates first known to us from authors later than Thucydides, e.g.

- I. 546/5: Fall of Sardis (pp. 68 f., 103 f.).
 + 3 Begged by Apollo from the Fates (Hdt. i 91.3).
 (a) + $4\frac{1}{2} \times 35 = 707/6$: Accession of Gyges (708/7–705/4) according to Euphorion of Chalkis fr. 1.
 (b) + $4\frac{1}{2} \times 33\frac{1}{2} = 699/8$: Accession of Gyges according to DH *Pomp.* 3.14 al.
 II. 476/5: Sybarite remnant helped by Syracuse (Diod. xi 48.4).
 (a) + 35 = 511/0: Destruction of Sybaris (Diod. x. 90.3).
 + $6 \times 35 = 721/0$: Foundation of Sybaris according to [Skymnos] 359 f.
 (b) + $7 \times 33\frac{1}{2} = 709/8$: Foundation of Sybaris according to Jerome.

Of Thucydides' Sicilian figures, two (70 and 245, vi 4.2, 5.2) are multiples of 35, and van Compernelle (pp. 421 ff.) explains the remainder in the same terms by utilising some terminal dates which have not hitherto figured in discussions of this problem, e.g. Gelon's and Theron's victories at Olympia (488/7 and 476/5 respectively) and the refoundation of Kamarina by Hippokrates in 493/2 (pp. 303 f.). His explanations rest on the assumptions that 483 was the date of the destruction of Megara and that Antiochos regarded Megara as founded in the year after Leontini. The former assumption is entirely defensible, and the latter is not made arbitrarily in order to prop up a single argument but in order to

introduce, at one stroke, coherence into the whole complex of Thucydides' figures.

Coincidental resemblances to calculation by generations can, of course, occur—Timaios' date for Carthage, $20 \times 33\frac{1}{2}$ years before its destruction, is an amusing example—but no rational being can dismiss van Compernelle's thesis as being founded solely on a run of coincidences. The question is rather whether the generation-value 35 accounts for *everything*, and here one must make reservations. Van Compernelle explains the foundation-date of Gela as 479/8 (the last year of Gelon's rule at Syracuse) + 6×35 ; this seems an odd terminus, especially as the foundation of Syracuse is calculated from Gelon's Olympic victory, won before he became tyrant of Syracuse. Is there really no room anywhere for at least one calculation which took as its terminus not an early fifth-century event but a famous name in a sixth-century list of eponymous magistrates?

Van Compernelle's talents are most in evidence in the study of historians and chronographers, and his *stemma* of variant traditions are extremely illuminating. He is a little less at home in the interpretation of poetry and literary criticism; he speaks on pp. 336 f. as if Pindar in *P.* 2 referred plainly to Anaxilas (his bibliography here contains no reference to Bowra, *Problems in Greek Poetry*, ch. V) and he goes astray (in good company) in his treatment (pp. 324 ff.) of Σ Pi. *P.* 3, who in fact presents a slightly muddled abbreviation of the argument: 'σπερδαίους in line 73 is not a poetic plural, but implies two Pythian contests, viz. the 26th and 27th; moreover, line 70 refers to Hiero as king, and Hiero became king' (Σ meant: 'was king') 'in the 76th Olympiad, within which the 28th Pythiad fell; anyway, however long after Hiero's accession *P.* 3 was written, it was after the 27th Pythiad, because of σπερδαίους'. Whenever van Compernelle's argument requires an excursion from historiography into history, he shows admirable common sense and a meticulous eye for detail. Yet one is aware at times of a certain lack of historical curiosity. The records of Assurbanipal's campaigns are naturally discussed in connexion with Gyges, but not the Babylonian Chronicle (cf. Sidney Smith, *Isaiah Chapters XL–LV*, pp. 35 f.) in connexion with the fall of Sardis. The problem of the relation between Antiochos and oral tradition is not really faced; the archaeological evidence which bears upon Thuc. vi 2–5 is treated rather similarly; and it is assumed (p. 506) that when dates are reached by genealogical calculation the longer the period, the bigger the error. This last assumption is not necessarily true, in so far as errors may cancel out one another; I cannot help observing that if I gave Antiochos the two data (i) that I was appointed to a chair in 1955, and (ii) that one of my grandfathers alleged that one of his grandfathers fought at Waterloo, Antiochos would arrive at the correct date of Waterloo. He was, I think, less fortunate about Selinus, but that in itself tells us nothing about his margin of error elsewhere.

K. J. DOVER.

HADAS (M.) **Hellenistic Culture: Fusion and Diffusion.** New York: Columbia University Press (London: Oxford U.P.). 1959. Pp. vii + 324. £1 15s. 0d.

This is a well-produced volume for the enjoyment of which it is not necessary to believe everything the author says. It is the work of a fertile and alert mind, writing pleasantly for an educated general public. The title does not perhaps sufficiently prepare the reader for the author's special interest, which lies in the interaction between Greek and Roman ideas on the one side and Oriental, especially Jewish, ideas on the other. It is a recurrent implication that historians are seeing the hellenistic age out of focus if they allow themselves to think of the Jewish world as separate from the Greek and Roman; on the contrary, it is an integral part of the scene, being itself receptive of Greek and Roman influences and in turn exercising a potent influence on surrounding culture. The relation is fully reciprocal, and we are therefore not to imagine that it was conversion to Christianity which first made the Greeks and Romans familiar with the Old Testament. Accordingly, hellenistic Judaism is never far from the centre of Professor Hadas' stage as the major historical phenomenon that draws his concentrated attention.

Although it must be frankly stated that not a few things in this book will provoke dissent, it should be openly recognised from the start that on a *priori* grounds this general thesis deserves a fair hearing and has the right to command sympathy. Both the familiar anti-semitic accusations of Jewish *ἀνανθρώπια* and the rigid rabbinic exhortations to strict observance and social separation tend to create a powerful but illusory impression that with few exceptions the Jews never really escaped from the ghetto and were as resistant towards the surrounding world as a contemptuous society was to them. But the traditional picture has lately been suffering damage, especially from E. R. Goodenough's *Jewish Symbols in the Greco-Roman Period* (1953 ff.); whatever view be taken of certain theses there advanced, Goodenough has surely established that the illiberal, academic Judaism of the Rabbis was not 'normative' and that Diaspora Judaism could diverge very considerably in a hellenised direction. Professor Hadas' experience of the Jewish community's role in American society may no doubt have had some influence on his approach to these questions; but he is probably right in seeing at least a measure of analogy.

The main thesis, then, is that the Mediterranean was one world, not two or three, and that the Jews were more hellenised and Greeks and Romans more affected by Judaism than is usually supposed. So Job shows the influence of tragedy; Ecclesiastes is touched by Epicureanism and Stoicism; the Testaments of the XII Patriarchs are influenced by Euripides, the Song of Songs by Greek lyric poetry, Judith by erotica, Jonah by Stoic universalism, the Dead Sea sect by Neopythagoreanism. And so on. The case is stated at its maximum; but at least Hadas drives one back to the texts to ask if it is indeed so, and

sometimes one is inclined to think that there may well be something in what he says.

More problematic is the question of Jewish influences on Greeks and Romans. Among educated persons Judaism was generally felt to be non-U, an extremely peculiar religion that could only be reasonably defended as ancestral custom for the race; and the level at which Jewish influence is directly attested ranges from the magical papyri to Numenius of Apamea, perhaps to Porphyry. This book, however, seeks to establish influence and positive appreciation at a much higher level: in Zeno the Phoenician, in Posidonius of Apamea, and in those Gadarene pearls, Menippus, Meleager, and Philodemus. Hadas also invites us to see the direct influence of the Jewish Sibylline Oracles in Tibullus ii 5, in Horace's Epode 16, and in the fourth eclogue. He even toys with speculations, based on common literary motifs, that Horace knew the Old Testament, having had a Jewish mother or a proselyte father! Here one cannot suppress a feeling that it was unwise of Professor Hadas to overstate so interesting a case.

On pp. 144-6 there is a discussion of the Areopagus speech ascribed to St Paul (Acts xvii. 22-3). That this speech has a 'literary' flavour is widely recognised; but it is a pity that Professor Hadas smiles a little too uncritically upon the ingenious thesis of Norden (*Agnostos Theos*, 1913) that the speech is not an original part of the text of Acts but is dependent on an incident in the career of Apollonius of Tyana described by his disciple Damis, a source much invoked by Philostratus in the third century. In brilliance and erudition Norden's argument is breathtaking, but the critical faculty revives when one begins to look up the references. According to Hadas' summary, on his visit to Athens Apollonius 'uses an inscription on an altar to teach that the proper worship of god is spiritual'. This statement cannot be supported by Philostratus, *V. Apoll. Tyan.* iv 19 (citing Damis' verbose summary of Apollonius' speech exhorting the Athenians to spiritual religion). Norden forced the reference to the altar inscription out of *V. Apoll.* vi 3, where Apollonius, now in a boat in the Nile, congratulates a young Egyptian, who has successfully resisted his stepmother's affection, for not having neglected Aphrodite as the Athenian Hippolytus did: *σωφρονέστερον γὰρ τὸ περὶ πάντων θεῶν εὐλόγειν καὶ τὰτα Ἀθήνησιν, οὗ καὶ ἀγνώστον δαιμόνων βρομοὶ ἰδρυνται*. There is no reason whatever to think Norden right in supposing that this allusion to the Athenian altars occurred in Damis' report of Apollonius' speech at Athens. Literary commonplaces do not prove dependence, and when analysed in a cold light Norden's spellbinding case comes to no more than pleasing conjecture.

All in all, this book offers rather too many hostages to criticism, and one comes to react cautiously on reaching the words 'There is a strong possibility that . . .'. But provided that the reader obeys Dr Routh's injunction to Dean Burgon and verifies his references, there is no reason why enjoyment should be diminished.

There are a few slips: p. 86 line 19, for Cyrene read Ptolemais; p. 127 line 8, for 207 read 217; p. 299 n. 20, for III C read III A; p. 310 n. 11, read Rohde.

H. CHADWICK.

PRITCHETT (W. K.) **Marathon.** (Univ. of California Publications in Classical Archaeology, vol. 4, no. 2.) Berkeley and Los Angeles: University of California Press. 1960. Pp. 137-90. Plates 6-17. 1 map. Price not stated.

In this essay Pritchett's aim is to determine the Athenian and Persian dispositions at the start of the battle of Marathon and to trace the course of the fighting. He is interested first and foremost in topographical questions and has not only gone over the terrain himself but has also obtained advice from archaeologists and geologists. Inevitably, however, he has had to pursue his enquiries in other directions as well. He discusses the organisation of the Athenian regiments and the history of infantry tactics; and he deals with the text and interpretation of the so-called Marathon epigrams and also with the note on *χωρίς ἱππεύς* in the Souda. His treatment is thus fairly comprehensive, and in each section his presentation of evidence is painstakingly detailed and clear.

Pritchett's labours have not, it must be said, led him to any novel conclusions. His account of the battle is in most essentials the same as was current in textbooks in the late nineteenth century. This is because he follows Herodotus, eking out his narrative with references to Plutarch, Pausanias and the Souda, and disagrees strongly with German historians who rely on the Ephorus tradition found in Nepos. He also favours Leake's assessment of the tactical situation and rejects the suggestions put forward by modern theorists such as General Maurice. Nevertheless, Pritchett makes a valuable contribution to the solution of the Marathon problem in two ways. He disposes effectively of irrelevant considerations which have long occupied the minds of scholars, and he correlates both the ancient authorities and recent speculations with his own accurate knowledge of the battle-field.

It is important to know that Herodotus' *ὡς ἀποθύμεοντο αἱ φελαί* has nothing to do with the ordering of complete regiments in line of battle, and that it really refers to the parade which each regiment held of its own men before moving into position. Similarly it is well to be assured that the first Marathon epigram should be associated not with Marathon but with Salamis.

Pritchett deplores the inadequacy of Soteriades' publications but accepts his identification of the shrine of Heracles with the church of H. Demetrios. He disagrees with Soteriades, however, in preferring a site near H. Demetrios and Vraná for the ancient deme of Marathon. Having made up his mind on these locations, he shows ingenuity in calculating the positions of the two armies. The Athenian line, he argues, extended either from Mt Kotroni to Mt Agrieli or from H. Demetrios to Brexisa; in either

case it was about 1500 m. long; and it may have been four deep in the centre and eight deep on both flanks. The Persians were drawn up a mile away, also on a front of 1500 m.; and their centre lay near to the Soros, which was built where the Athenians suffered most heavily. With How, Pritchett regards the Athenian charge as a move intended to counter the effect of Persian archery and to engage the entire Persian line simultaneously; with Schachermayr he accepts *χωρίς ἱππεύς* as indicating that the Persian cavalry was withdrawn from the field during the night before the battle.

In the absence of a certain location for old Marathon (not to mention Probalinthus), Pritchett probably has done as much as can be done to clarify the opening phase of the battle. His account of the Persian withdrawal through the northern marsh to a point of embarkation at Schoiniá also seems to be correct in the main. The trouble is that the ancient literary evidence does not enable us to follow the action throughout, and for want of good authorities Pritchett sometimes over-argues his case. For example, he presses into service Ar., *Wasps* 1081 f. *ξὺν δόρει, ξὺν ἀσπίδι . . . στάς ἀνὴρ παρ' ἀνδρὶ* and so turns the battle into an infantry set-piece like Plataea. This conception seems to be at variance with his own notion that the Athenian charge quickly resulted in the rout of the Persian wings and in the rapid envelopment of their centre. In any case Aristophanes' words merely echo the hoplite tradition that had been in vogue since the time of Tyrtaeus and are surely not intended to describe the fighting at Marathon precisely.

The map facing p. 137 has no scale and does not contain all the place-names mentioned in the text of the essay. Readers should use one of the published maps to which Pritchett refers.

A. J. BEATTIE.

FONTANA (M. J.) **Le lotte per la successione di Alessandro Magno dal 323 al 315.** [Atti dell' accademia di scienze lettere e arti di Palermo, ser. 4, vol. xviii, 1957/58, 2.] Palermo: the Author. 1960. Pp. 239. Price not stated.

Italian scholars in recent years have found a special interest in the history of the Greek World after Alexander, and Miss Fontana has produced in this volume of the short studies published by Palermo University the most elaborate work to date on the struggles between the leading figures of the Greek World from Alexander's death to the victory of Cassander over Olympias in 315 B.C., when, as she says (p. 119), 'le lotte per la successione' ended and 'le lotte per la determinazione delle sfere di potenza e di influenza' began.

Miss Fontana rightly concentrates her attention on the causes and effects of the agreements and disagreements between these leading figures; her study certainly re-emphasises their dominant role. While more might be said, chiefly perhaps to lament our ignorance, of relations between the leading figures and the populations of the areas which they

from time to time controlled (though the Macedonian army and homeland do emerge in places as actors in the drama), her method has the advantage of saving her from becoming too involved in the details of military campaigns. Further, Miss Fontana, whose earlier published work has included articles on Diodorus Siculus 17, has confined her detailed analysis of the literary sources, on which her historical arguments are, at times closely, based, to an appendix of 87 pages (her most important conclusion here is that Diodorus Siculus 18.5-20 can be regarded as a summary, sometimes badly compiled but always direct, of the work of Hieronymus of Cardia).

But, in spite of these judicious exclusions, it is difficult, in the early chapters especially, to see the wood for the trees, because Miss Fontana has to deal with some much discussed problems, such as the relative positions of Philip Arrhidaeus and the young Alexander, the distribution of offices made at Babylon after Alexander's death and the ambitions of Perdiccas. She rejects the idea that Philip Arrhidaeus was incapacitated by idiocy (an exaggeration by tendentious sources—p. 33) and concludes that he was recognised as sole king by the Macedonian army in 323 with Alexander's then unborn and therefore still hypothetical son as no more than his heir; and that the 'prostatia' entrusted to Craterus at Babylon was not therefore a regency or guardianship of Philip, but a guardianship of his kingship, a conclusion which is the same as that of Tarn in *CAH* v 461, whose views in that work are not noticed by Miss Fontana, a surprising omission in her wealth of references to modern literature. As to Perdiccas, her conclusion is favourable: that he has suffered from hostile sources, because Hieronymus was a supporter of Antigonos, and that so far from following a course that would lead him ultimately to the throne, he was growing steadily weaker and was forced first to make concessions over the marriage of Eurydice and Philip and then to a showdown with Antigonos, whose unpunished intransigence was likely to lead to his own ruin; Antigonos was in fact the most responsible for the outbreak of civil war, acting in his own interests, though he did persuade Antipater and Craterus that the interests of the king were at stake.

Such problems as these are too well worked over for all Miss Fontana's conclusions to be likely to be unchallenged. But, when the general picture becomes clear in the concluding chapter, the characters and actions of Perdiccas, Antigonos and Polyperchon in particular emerge with a consistency that makes hers at least a plausible reconstruction of their struggle.

Three and a half pages of corrections give a bad impression of the printing, but most of these are concerned with the conventions, not the substance, of footnote references; there are, however, still some small errors uncorrected, one even in the list of corrections.

T. T. B. RYDER.

AFRICA (T. W.) **Phylarchus and the Spartan Revolution.** (Univ. of California Publications in History, 68.) Berkeley: the University of California Press. 1961. Pp. x + 92. Price not stated.

To judge from what is known of his work from Polybius and Plutarch, Phylarchus was one of the most spirited of ancient historians, both in social and political interpretation and in literary style. The attempt of Agis IV and Cleomenes III to revive the 'Lycurgan' traditions and restore the military power of Sparta in the Peloponnese, against the interests of the Spartan oligarchy at home and of the Achaean League and Macedon abroad, was controversial and dramatic in its day, and it raised revolutionary issues that are still with us. The theme can have lost nothing in Phylarchus' telling; his evidence keeps its general significance in the present study.

Africa opens with a lively reconstruction of the historian's life and attitudes: Phylarchus was erudite and imaginative, satirical and moralising, setting social decadence against the primitive virtues. In doing this, we may add, he was true to the character of Hellenistic historiography. How far had he an ideological approach to Spartan reform, if the Stoic Sphaerus was closely associated with Cleomenes? Africa rejects arguments from 'Stoic socialism', rightly, and would look rather to Cynic influence on Phylarchus. Yet, we may note, if a movement appealing to traditional standards of conduct in Sparta needed intellectual support, it might well use the basic philosophical doctrines of Stoicism. In fact, as Africa remarks, 'practical action stemmed from interest and necessity', and he treats the propagandist element in Phylarchus' history in the light of Polybius' criticism. Few will quarrel with his conclusion that the pro-Spartan comes out of the comparison at least as well as the Achaean, both incorrigibly attached to their opposing points of view.

Polybius attacked Phylarchus for writing 'tragic history', and Eduard Schwartz evolved from this a 'Peripatetic' theory of historiography. Recent criticism has aimed at breaking down Schwartz's argument, partly by referring to Aristotle's contrast between 'history' and 'tragedy', partly by showing the dramatic elements in all Greek historical writing, even among 'part-time practitioners of tragic history'; and Africa is content to illustrate the dramatic elements in Phylarchus' account. But the Aristotelian canons of tragedy, including the 'catharsis' of emotions through the direct impact of 'horror and pity', could be applied in history to the systematic dramatising of tragic scenes. There is still a case for studying Phylarchus' presentation of dramatic episodes as a whole; note now C. O. Brink, 'Tragic History and Aristotle's School', *Proc. Camb. Phil. Soc.*, 1960, p. 14.

Finally, since Phylarchus helped to give form to the traditional Spartan 'mirage', Africa examines the place of myth in his work and relates his conception to the actual conditions in Hellenistic Sparta. It

would have helped to do this earlier in the discussion and make more analytical use of the results. For instance, how far should Phylarchus' moralising be considered as fair historical comment upon the hypocrisy of Sparta's condition before Agis and Cleomenes? We have been shown Phylarchus in universal terms: we still need to focus our attention more sharply on the immediate Spartan setting of his interpretation. This readable and stimulating study will enable us to do so with fuller sympathy.

A. H. McDONALD.

MERRITT (B. D.) *The Athenian Year*. (Sather Classical Lectures, 32.) Berkeley and Los Angeles: University of California Press (London: Cambridge U.P.). 1961. Pp. viii + 262. £2 8s. od.

All Meritt's gifts of exposition cannot have made these Sather lectures easy to follow, and they are not easy reading either. But I have found them rewarding; Meritt's readiness to revise his own views, his candour, and his pertinacity have seldom been better displayed.

In essence, this book is an extended review of Pritchett and Neugebauer, *Calendars of Athens*; that book or, perhaps better, Pritchett, *BCH* lxxxi 269-301, should be digested before reading it. Three of their main contentions were that astronomical theory had no place in the workings of the Athenian calendar, that the archon indulged in extensive intercalations of days into the festival calendar which dated by months, and that the lengths of prytanies within a year were kept as regular and equal as possible. In so far as the first of these was a point against M., he has accepted it; for the second, he accepts the phenomenon, but does not allow it as free play as P. and N.; the third he rejects.

First, he clears up some preliminary points. (1) It was the view of P. and N. that the basic Athenian festival calendar was established by lunar observation. It seems to me that M. disproves this conclusively. He incidentally shows that the observations reported by Timocharis and Hipparchus which have always been taken as evidence for the Athenian calendar are nothing of the kind, but are expressed in terms of an ideal astronomical calendar. M.'s view, resting partly on Geminus viii 52, is that the Athenians found a regular alternation of 29- and 30-day months quite accurate enough for their purposes, though they may have put it straight occasionally by observation. (2) An observation theory requires that, in a 29-day month, when backward count is employed in the last decade, the day which is omitted must be *δευτέρα* (*ἡβρόντος* or *μετ' εικάδας*) and not *ἐνάτη*. M., by using the Aristophanes Scholia rather than a vague statement by Proclus, shows that the opposite was true of Athens. This again seems to me certain. But I think M. is too tolerant of the possibility that forward count was ever used in the last decade. Here I would side with P. (cf. *CP* liv 155-7) and

take the view that all possible explanations, including stone-cutter's error, should be employed rather than assume forward count in the few remaining cases where it is even a possibility.

This clears the ground for the main point which still seems open. P. and N., working from *Ath. Pol.* 43.2, have as a firm postulate regularity and equality in the length of prytanies. (Equality for this purpose includes the inevitable discrepancy between, e.g., 36- and 35-day prytanies in a 354-day year of 10 prytanies). Rather than have irregularities here, they will assume irregularities in the festival calendar caused by considerable intercalation or inaccurate observation. M. prefers to work from alternation of 29- and 30-day months, which he applies with flexibility, and is prepared to tolerate irregularities in the prytany calendar. The only method of judging between them is to work through every year for which there is calendar evidence, and this takes up the greater part of both books. It still seems hard to arrive at a decision. Individual solutions can be judged by their possession of the mathematical virtue of elegance rather than by anything else. Since M. has more flexible postulates, he will inevitably be able to produce plausible solutions for more cases. P. can still claim that there is no directly attested case of a prytany longer than the hypotheses predict. There are, however, certainly some very strong pointers to inequalities in the prytanies, and, even when the prytanies are equal in length, the most elegant solutions often place the prytanies which are one day longer in what P. and N. think the wrong place in the year. Agreement still seems far off, and it is a matter of regret that P.'s first reaction (*BCH* lxxxv 23-8) has been to defend the lost cause of lunar observation rather than to salute M.'s conversion to a scheme where both prytany and festival calendars are flexible, a scheme which he has always regarded as a possible alternative to his own.

There is much more in the book. M., I think successfully, defends his view of the varying length of the fifth-century conciliar year. More important for the general reader, he redefines his views on the possibility of assigning Julian equivalents to Attic dates. His position now is that near accuracy is possible between 422 and 411 and 337 and 318, and I think his confidence is justified (contrast P., *BCH* lxxxi, 293-300). There has indeed been a good deal of error committed on the basis of precise chronology; a recent example could be cited where four pieces of direct evidence were discarded rather than question a Julian date given by Dinsmoor. But M. convincingly defends his dating of Antiphon VI to 419 against P.'s criticisms.

There is an unlucky chapter on 'Coins and the Calendar' where various conclusions are drawn from advance news of Miss Thompson's work on Athenian New Style coinage. This seemed to require several specific second-century years to be intercalary, two pairs of consecutive intercalary years, and an example of forward count. Miss Thompson's views are now available in full, and I have tried to show

elsewhere that they should not be accepted. There will be help from this source, but not yet.

M. also provides us with an up-to-date Hellenistic archon-list, about which I am mildly unhappy, because it looks a good deal more solid than it is. What we really need is someone with Daux's caution to provide us with a *Chronologie Athénienne*, where what is solid and what is not could be clearly seen. To take some examples, I do not share M.'s confidence that inventories fit in with secretary-cycles, his current view of the secretary of Philoneos' year is highly improbable, 160-140 is guesswork. M. rarely deceives himself, but it is only too easy to deceive others.

A book with so much detail inevitably raises minor factual comment. Pp. 17-18: why quote Maimonides when the Mishnah (Rosh Ha-Shanah, chs. 1-2) is 1000 years earlier and more accessible? P. 72 n. 4: Michel 832 is a more accessible reference. Pp. 72-3: 347-6 was intercalary, and 345-4 an ordinary year of 355 days (*BSA* 1 25-6). P. 84: on the face of it, *IG* ii² 358 should come from the period of the 12 tribes. P. 86: the restoration in *IG* ii² 368.1 should not have been accepted. The rho in *IG* 1.9 is doubtful and would in any case be the first letter of the patronymic. P. 90: surely the point of the restoration ἐν Ἀριστοφάνει in *IG* ii² 348.1 is that it is only 15 letters and is not spread out over the line?

D. M. LEWIS.

KENNA (V. E. G.) **Cretan Seals: with a Catalogue of the Minoan Gems in the Ashmolean Museum.** Oxford: the Clarendon Press. 1960. Pp. xiii + 163. 23 plates. 172 text figures. £5 5s. od.

The collection of Minoan gems in the Ashmolean is probably the most important outside Crete. The bulk of it consists of seals collected by Sir Arthur Evans, many of them during his early travels in Crete before 1900 while the island was still under Turkish rule. This first methodical catalogue of the collection, well arranged, excellently described, with full references and apt comments, is very much to be welcomed. It is preceded by a general survey of Minoan seals and their relationship to the early seals of Egypt and the Near East.

Here the author's views are always interesting, even if somewhat Delphic at times. This was perhaps inevitable in view of the many uncertainties surrounding the exact chronological horizons in which the seals and their impressions in clay have been found, particularly in the case of the earlier periods, Early Minoan and Middle Minoan.

A theme which is basic to the book is the amuletic (due to shape or substance) and talismanic (due to signs or symbols added) character of early seals. An intriguing explanation is thereby offered for the extraordinarily fine and naturalistic quality of Minoan gems of the best period, from the end of Middle Minoan and the early part of Late Minoan times. This is thought to be due to an emancipation

from the restrictions imposed (on material, shape, treatment and choice of subject) by amuletic or talismanic considerations. The development of the specialised 'talismanic' class of seals, noted by Evans, towards the end of the Middle Minoan period, is then explained as a reaction against this emancipation, people being afraid that the new gems in the fine naturalistic style might have lost their amuletic and talismanic virtues.

The limited distribution (largely confined to the north and centre of the island) of the early three-sided prisms is noted, and the suggestion made that they were perhaps essentially amulets. These stones were assigned by Evans to Early Minoan I, but the author is probably right in agreeing with Matz (pp. 19 f.) that none of the seals known from Crete are as early as E.M. I, and seals may not have been made in Crete until E.M. II.

The attribution of seals to E.M. and the early part of M.M. is bedevilled, as it was when Evans wrote the *Palace of Minos*, by the flimsy character of the evidence. A few seals come from excavations of settlements, but most are from communal tombs which continued in use over long periods of time. Moreover, there is no reason why the seals from such communal tombs should not be later in date than the latest of the clay vases found in them. The vases inside such tombs are often comparatively few (indeed a large tomb like that of Kalathiana in the Mesara, with many seals and much gold jewellery, produced no clay vases at all), while the burials may be counted in hundreds. It seems clear therefore that clay vases were not put with every burial, and some evidence suggests that the custom of placing vases with burials inside the actual tombs was an early one which tended to fall into abeyance in mature M.M. times. Thus the famous equation of the imported Babylonian cylinder and Egyptian scarabs from Platanos Tholos B with the latest pottery found inside the tomb has no compelling grounds, as the seals may be quite a bit later than the few clay vases with them inside the tomb.

But there are now large closed deposits of seals or their impressions, which, when the evidence from them has been digested and their date established beyond doubt, will be of decisive importance for the study of early Cretan seals. These are:

(1) The Lerna sealings (M. Heath, *Hesperia* 27 (1958), p. 81 f.), which, in spite of the fact that they were found at an Early Helladic coastal site in the Argolid, in the opinion of the reviewer at least (although the idea is rejected by their publisher) are likely to be impressions of Cretan seals, assignable perhaps to M.M. I A/B or even II A: in any case earlier, but not much earlier, than,

(2) The Phaistos deposit (D. Levi, *Annuario* 35-6 (N.S. 19-20) (1957-8), 7 f.), associated with fine decorated pottery assignable to M.M. II B on the system of Evans;

(3) The seal-maker's workshop at Mallia associated (according to the preliminary report in *BCH* 81 (1957), p. 693) with pottery assignable to M.M. I,

which may mean contemporary with either M.M. I or II at Knossos.

For the later periods (end of M.M. and early L.M.) the situation is less obscure. The author is surely right in preferring with Matz the earlier opinion of Evans (e.g. *Scripta Minoa* I (1911), p. 21) dating the Hieroglyphic Deposit to M.M. III A not M.M. II B. It should be later than the Phaistos deposit ((2) above) of M.M. II B, but earlier than the M.M. III B Temple Repositories. There is an interesting analysis of the great L.M. I B deposits of sealings from Zakro and A. Triadha; and the Vaphio gems, many of them Minoan imports, are discussed at length. A number of seals of all shapes and materials from L.M. I B deposits recently found on the north side of the Royal Road at Knossos should further help to clarify knowledge of Minoan seals during this important period.

There is a useful discussion of the great deposits of the 'Last Palace' of L.M. II at Knossos. The deposits are inter-linked, and are indissolubly connected with the Linear B tablets; although the warning of Evans must be kept in mind that strays of the Reoccupation period of L.M. III B may have intruded themselves among these deposits (*PM Index Vol.*, p. 183). J. Boardman has suggested grounds for thinking that the debris of the Lapidary's (seal-maker's) workshop, assigned by Evans and K. to L.M. III, is really of L.M. II date. K. is surely right (p. 65, n. 9) in rescuing for L.M. II these three fine gems assigned by Pendlebury (and one of them by Evans himself) to L.M. III.

Two criticisms: The illustrations are life size, which in the case of many gems is too small to allow adequate study, and a magnifying-glass is no help as it would be with the originals. At the same time the text of the first part of the book is unnecessarily difficult to use because the Ashmolean seals are described by their museum numbers and not by the numbers of the published Catalogue. There is no simple table of correlations, and the labour spent in converting the numbers in order to refer to the Catalogue is disproportionate. Seals from other collections apart from the Ashmolean are often cited by museum numbers without references to publications; doubtless because the seals are unpublished, but it means that a thorough study of the book is impossible until the appearance of the promised corpus of Minoan and Mycenaean seals (*Jdl* 1959, p. 288 f.).

Some minor points. P. 27, n. 7. K. may unduly minimise the original dependence of early Cretan seals upon those of Syria and North Mesopotamia.

Appendix I, p. 28. The derivation of the lentoid shape of seal from a fish vertebra is ingenious, but seems unnecessary.

Horses on early Cretan seals! e.g. p. 94, No. 50 in the Ashmolean and p. 28, n. 5, Nos. 444-5 in Herakleion Museum. Revolutionary, but nothing is impossible in the light of the twelfth-dynasty horse found at Buhen in Upper Egypt. But perhaps safer with Evans to see an ass on Ashmolean No. 50.

P. 34, n. 7. The tubular drill was surely used for making concentric circles in twelfth-dynasty Egypt (e.g. scarabs in the University College collection, London). This technique and style of design may well have reached Crete from Egypt along with scarabs at that time. When more is known about the beginnings of spiral decoration in Crete it may be found that this also reached Crete from Egypt and not vice versa as is usually thought.

In connexion with early Cretan cylinder seals the silver cylinder (was it really an import?) and the 'carved cylindrical bead of green steatite' from Mochlos deserve notice (Seager, *Mochlos*, p. 111 and 75, xx 8).

Since the publication of the book M. Gill has discovered new grounds for doubting the authenticity of the Ayia Pelagia cylinder (*Inst. of Classical Studies Bulletin* 8 (1961), pp. 15-22). Another seal alleged to be from A. Pelagia (p. 129, No. 285), the sole example of a long amygdaloid assigned to L.M. II, looks unconvincing to the reviewer.

P. 49 f. Perhaps the Knossos Harbour Town sealings were stolen from the Zakro deposit, as Pope suggests (*BSA* 55 (1960), p. 205 n. 10).

The author does not seem to agree with the criteria advanced by H. Biesantz in *Kretisch-Mykenische Siegelbilder* (1954) for distinguishing between Late Bronze Age gems of Cretan and Mainland origin. But it would be interesting to know more of his views about the gems from the Shaftgraves at Mycenae, to what extent they are the work of Mainland or Cretan craftsmen. On this and many other aspects of Aegean Bronze Age gems the author's opinions, based upon a wide knowledge and intimate study of the material, will be much welcomed.

M. S. F. HOOD.

BOARDMAN (J.) **The Cretan Collection in Oxford: the Dictaeon Cave and Iron Age Crete.** Oxford: the Clarendon Press. 1961. Pp. xi + 180. 1 map. 48 plates. 57 text figures. £5 5s. od.

Owing to the generosity of Sir Arthur Evans, the collection of Cretan antiquities in the Ashmolean is the richest outside Crete. In this important publication, many of its most notable pieces are presented for the first time. Of the Minoan material, only the votives from the Dictaeon and Patso caves are included. The Hellenic period is represented by a few choice pieces from the Idaean cave, and a large corpus of miscellaneous finds from other sites on the island. Everything is set out in an exemplary manner, with full description and illustration, and penetrating commentary; where relevant, *comparanda* in Heraklion are added to the illustrations. The final chapter is a terse but comprehensive summary of Cretan art and archaeology from the tenth to the sixth century: this is the first general survey since P. Demargne's *La Crète Dédalique* (1947) of a subject that needs frequent reappraisal, owing to the huge

quantity of new material that enriches the museums of Crete every year. The Oxford collection could hardly have found a more skilled exponent than Mr John Boardman, who has been intimately associated with Cretan antiquities both as an excavator at Knossos and as a Keeper in the Ashmolean.

The first 24 plates illustrate the full range of the Dictaeon offerings, which fall into three main periods: MM III-LM I, LM III, and eighth-seventh centuries. All three are presumably represented in the rich series of bronze figurines. Worshippers begin in the first period, but may well continue into the second: figurines with bent knees, once regarded as LM III, are rightly down-dated on the strength of their resemblance to Geometric work on the mainland. Of particular interest is the crawling baby, no. 22, with its obvious relevance to the Dictaeon legend: this is recognised as Minoan, since it is so like the well-known ivory from Palaikastro. From the Idaean cave comes the most splendid offering of all, the bronze head-vase in a strong Oriental style: Mr Boardman reasonably suggests a date of c. 650, coinciding with the end of the series of Idaean shields to which it is closely related.

The final résumé begins with the Cretan Dark Age: here some of the general statements need modification. P. 129: it now transpires that Phaistos, as well as Knossos, remained inhabited throughout. P. 130: Attic Protogeometric vases were imported to Knossos not at the end, but at the beginning of the local Protogeometric series: thereafter mainland influence waned, until the arrival of a Mature Geometric style, also Attic in inspiration. P. 131: in supposing that this style was introduced through Cycladic middlemen, the author may be overstating the case: at Fortetsa one of the earliest MG painted pithoi (no. 454) is Attic work, and perhaps made for export. In tracing the sources of Oriental influence, he rightly attributes many of the new phenomena to Phoenicia and Palestine (p. 151): yet need we travel as far as Philistia to find the model for the quaint birds on Knossian polychrome pithoi, so clearly reminiscent of the Myc. III C Close style? But these are only minor points: the main strength of the chapter lies in the exceptionally clear account of Orientalising art in Crete. Mr Boardman makes a useful distinction between the more or less hellenised metalwork of the Knossos area, and the wild, untamed Oriental style of the Ida shields: both series run concurrently until the middle of the seventh century, when they give way to the more sober canons of Dedic art.

Mr Boardman must be congratulated on producing a valuable book out of such diverse, and often intractable, material. The patience with which he has studied every object, however humble, will earn him the gratitude of all those who dig in Crete: his final summary, fully documented and lucidly presented, will be welcomed by all general students of Hellenic art and archaeology.

J. N. COLDSTREAM.

BROCK (J. K.) **Fortetsa. Early Greek Tombs Near Knossos.** [British School at Athens, Supplementary Paper No. 2.] Cambridge: University Press. 1957. Pp. xvii + 224, with 174 plates and 2 maps. £6 16s. 6d.

It is always a difficult task to edit and publish the researches of someone who has died, and the author in this instance was faced with a Herculean task beyond the ordinary. Two directors of the school had died in quick succession, leaving not only their excavations unpublished but also the problems unsolved of the classification and chronology of Cretan pottery between the collapse of the Minoan civilisation and the murky rise of archaic art in Crete. Brock was only present at the excavation of eight of these tombs, but he was able to profit by the additional experience and the magnificent photography of the succeeding Director, Mr Gerard Young.

Many of the plates are far from clear and occasionally the printer may be to blame, but usually (as for example on plate 67) the obscurities are due (as I can testify from personal knowledge of these vases) simply to the bad preservation of the surface or the presence of extraneous matter that no acid would remove without also removing the decoration.

Brock's tomb lists are magnificent. The book is solid work for the specialist, not bed-side reading, but the short general description of each tomb is readable enough, and the small print catalogues of finds are all that could be desired.

The final chapter on 'Chronology and Conclusions' is very valuable for students of Greek history and ceramics in general, and I do not think Brock's chronological series (Sub-Minoan 1020-970, Early Protogeometric 970-920, Middle Protogeometric 920-870, Late Protogeometric 870-850, Protogeometric B 850-820, Early Geometric 820-800, Mature Geometric 800-770, Late Geometric 770-735, Early Orientalising 735-680 and Late Orientalising 680-630 B.C.) will require much revision by later research, though the term Protogeometric B seems a little odd when there is no such thing as Protogeometric A.

The historical inferences very cautiously drawn by the author are important. 'The complete elimination of the remains of an earlier generation must surely point to invasion and conquest by a foreign race' (though *larnax* fragments showed that some of the tombs had been dug in Minoan times) . . . 'I think one may fairly say that there is an affinity between the traditionally Dorian regions of Crete, Thera, Caria and the Dodecanese. But the outstanding conclusion from the archaeological data is the preponderant influence of Attica during the tenth and ninth centuries.' Corinthian and Cypriot influences are noted but time and expense prevented the author from discussing Cretan styles other than that of Knossos.

The author mentions but does not attempt to solve the puzzle of the cultural eclipse that descended on Knossos towards the end of the seventh century B.C.

The printing and production of the work is very good.

R. W. HUTCHINSON.

MYLONAS (G. E.) **Aghios Kosmas. An Early Bronze Age Settlement and Cemetery in Attica.** With an appendix on the Early Helladic skulls by J. L. Angel. Princeton: University Press. 1959. (London: Oxford U.P.) Pp. xviii + 191. 182 figures. 64 drawings. 160s.

This important prehistoric settlement on a low peninsula jutting into the sea near Athens Airport was excavated by Mylonas in 1930-31, with a final season of work after the war in 1951. The excavations of 1930-31 were described at some length in *AJA* 38 (1934), 258 f., those of 1951 in *AE* 52 (1955), 117 f. Final publication was delayed in the hope, never realised, of finding the Mycenaean cemetery.

The settlement did not offer a great depth of stratified deposits. But four distinct building periods were detected, two Early Helladic (Aghios Kosmas I-II), and two, much eroded, Mycenaean (Aghios Kosmas III-IV). AK II was destroyed by fire at the very end of the Early Helladic period. Mycenaean occupation (AK III) dates from at least as early as Late Helladic II (c. 1500 B.C.). During the intervening Middle Helladic period the evidence suggests a gap in occupation, surprising on a site so apt for prehistoric settlement.

The final destruction of the Mycenaean settlement is assigned by M. to Late Helladic III C. But one at least of the vases from it (fig. 138, no. 66) admittedly looks III B, and the designs on the shoulders of the stirrup vases figs. 137, no. 20, 139, no. 61, correspond to III B varieties of Furumark's 'Flower' (*The Mycenaean Pottery*, fig. 45 Mot. 18). Similarly the shapes of the vases from the Mycenaean houses S and T (figs. 136-8) seem more akin to Furumark's III B than to his III C.

P. 61. The 'larnakes', whose bases with remains of burials were found below the floor of house S (AK IV), were surely oval bath tubs like fig. 171 from inside the same house. Such bath tubs were commonly used for burials in Late Bronze Age Crete, and sometimes on the mainland; but true box-shaped larnakes with legs have not yet been attested on the mainland, apart from the unique stone example from Cephallenia (Marinatos, *AE* 1933, 77 f., 94 f.).

Two stone objects of great interest from the point of view of chronology were recovered from the Early Helladic settlements: a foot amulet, and a bottle-shaped seal, both paralleled from the earlier Bronze Age (Minoan) horizon in Crete. The seal, originally published as from the fill of a 'bothros' of AK I (*AJA* 38 (1934), 267), is now assigned to the packing of the earliest floor of AK II immediately above the 'bothros' ('on the lowest floor' (p. 157) is clearly a mistake (see pp. 152, 159)). The importance of these objects for the relative chronology of the Aegean Early Bronze Age would be greater if the Cretan parallels cited by Mylonas could be assigned

more precisely to their place within the Minoan system.

Interesting, and also important from a chronological point of view, are the Cycladic elements in the Early Helladic of Aghios Kosmas. These are most prominent in the cemeteries (tombs, marble figurines, and clay vases including 'frying pans', comparable with those known from the Cycladic islands). M. indeed infers an original colony of people from the Cyclades trading in obsidian, and eventually fusing with the Early Helladic population. Obsidian, shown by analysis to come from Melos (A. N. Georgiadhis, *Praktika Akad. Athinon*, 31 (1956)), was found in unusually large quantities at Aghios Kosmas, and is still abundant on the site.

The Early Helladic tombs, of which M. explored some forty in two distinct cemeteries, are of great interest, and worthy of the exhaustive description given them. Unfortunately almost every trace of them has been obliterated since the excavations. Most had walls built with upright slabs or with small stones, or with a combination of slabs and stones; and they were roofed with slabs. Although small in size they were clearly family graves. M.'s conclusion that in many cases, if not invariably, burials were inserted through the roofs, and not through the doors, seems inescapable. M. shows reason to suppose that, during the latest phase in the use of the cemeteries, the tombs were used as ossuaries; the bodies being buried in shallow graves beside them until the flesh had decayed. It seems to have been normal to place funerary offerings outside the tombs, rather than inside them with the bodies, a practice also apparent in the case of Middle Minoan communal tombs in Crete. Differences in the character of the pottery recovered from the settlement and the cemeteries (e.g. inferior fabric of vases from the cemeteries, and greater frequency of certain shapes like 'frying pans' there) is explained on the theory that most of the cemetery vases were made especially for funerary use. Not the least valuable aspect of M.'s study of the tombs is very full reference to the observations of Tsountas, who excavated many hundreds of prehistoric graves in the Cyclades at the end of the last century.

Grape seeds found in a pithos which has a spout near the base may indicate wine production in Early Helladic times. In the Late Helladic settlement there was abundant evidence for the extraction of purple dye from *murex* shells, but none in the Early Helladic. M. suggests that the decorative motifs, and even the shapes, of many Early Helladic and Early Cycladic vases were originally inspired by marine creatures such as sea urchins, star fish, and shells.

A few criticisms. There is no plan showing the relationship between the main excavations in the settlement and outlying areas of houses, e.g. the Mycenaean house T, and those on the reef U, V, W; and nothing to indicate the position of the cemeteries. Similarly such important objects as the Early Helladic seal-stone (fig. 166, no. 13) deserved drawings as well

as photographs: the design on the bottom of the foot amulet (*ibid.*, no. 14) is described, but not illustrated; and no section or side view is given of the interesting Early Helladic mortar (p. 145, fig. 169, no. 48).

The appendix by J. L. Angel on the Early Helladic skeletal remains is full of suggestive ideas.

M. S. F. HOOD.

MOON (B. E.) **Mycenaean Civilization, Publications 1956-60: a Second Bibliography.** (Bulletin of the Institute of Classical Studies, Supplement 12.) London: Institute of Classical Studies. 1961. Pp. xxv + 130. £1.

This second bibliography (the first appeared as *BICS* Supp. 3 (1957)) covers publications from 1956 to the end of 1960. It is, however, much more than a mere continuation; for whereas the first covered Mycenaean civilisation in the ordinary sense, this (despite the title) includes Minoan, and also lists work on Minoan and Mycenaean language and epigraphy. In these two fields publications are listed from 1936 onwards (unless included in the first bibliography), though no attempt has been made at complete coverage of language and epigraphy prior to 1953. This last restriction is reasonable enough: it means in effect that we start from Ventris and Chadwick's 'Evidence' in *JHS* (1953), with a selection of fifty or so items published before the decipherment.

The main author-list is supplemented as before by subject and topographical lists; and these naturally include many new headings to suit the new material: nineteen were needed for language and epigraphy alone. It is noticeable too that the list of periodicals represented is much increased; and titles of research dissertations (from the ASLIB index) are usefully included.

The work is all the more welcome in this amplified scope. As before it appears very careful and thorough in execution, and faults would be hard to find. So far I have only noticed one misprint, and that not misleading. Once again our thanks are due to Miss Moon, this time for even more comprehensive help than before.

F. H. STUBBINGS.

HARDEN (D.) **The Phoenicians.** (Ancient Peoples and Places, 26.) London: Thames and Hudson. 1962. Pp. 336. 82 text figures. 115 illustrations in plates. £1 10s. 0d.

The Phoenicians are the most elusive of ancient peoples. Only for Carthage is it possible to trace a consecutive history and to hope for a reasonably full archaeological record. The archaeology and history of the Phoenicians at home remain obscure, and since Herodotus, who was in a better position to judge than we are, commentators and scholars have glibly attributed to them anything vaguely eastern which owned no other home. Mr Harden is one of the very few scholars who have pursued them soberly and steadily over many years, and the Ancient Peoples and Places editor has done well to

make him set down his considered opinions on this thorny subject.

A reviewer for Hellenists can only pick out special features. The Phoenician part in the Greek orientalist movement is still hard to define mainly because the homeland Phoenician material is still so scanty. Harden strains the evidence to say that 'much of the material of early geometric style found in Rhodes is more akin to the Phoenician stock-in-trade than to the Greek'. For Phoenicians in the west the literary evidence is perhaps more plentiful but no more reliable than it is for the Greek Dark Ages. Harden believes that the pattern of Phoenician settlement there was established by 800, but has to admit that there is no archaeological evidence for it. The earliest deposit at Carthage, in the Tanit sanctuary, is dated by its Greek pottery to the second half of the eighth century and nothing else there can be assured any earlier date. The presence of Greek pottery, its origins (? Euboean rather than 'island' or Corinthian), and the Greek (? therefore pre-Phoenician) place names in the area have still to be explained fully. The Nora stone (evidence for Phoenicians in Sardinia) is put at the end of the eighth century. The Carmona ivories (part of the evidence for Phoenicians in Spain) are taken for Carthaginian, and dated to the sixth century. An example from Samos (*AM* lxxiv Beil. 86.1) in a pre-640/30 deposit, is from the same workshop. Harden suggests that it was Mycenaean experience of the west, passed on after the moves of the twelfth century, that set the Phoenicians off in that direction. It is not clear to the reviewer why the Phoenician objects which reached Etruria were 'certainly not carried by Greek ships'. There is a fascinating discussion of early Phoenician exploration round Africa and of Hanno's voyage. In the account of Phoenician art it was perhaps a pity to admit so much of Cyprus and North Syria, since this only serves to blur the picture yet further. The jasper scarabs of Sardinia are, some of them, not only Greek in style but Greek in subject, and show that Greeks could work for or beside Phoenicians as they did for Etruscans.

The chapters are not arranged in chronological sequence, and the author was right to isolate Carthage and the considerations of Phoenician life, religion, commerce, etc. A most useful bibliography compensates for lack of footnotes, and there is an excellently detailed index. The pictures include many unpublished pieces and are well produced. This is very good value for money.

JOHN BOARDMAN.

BARNETT (R. D.) and FALKNER (M.) **The Sculptures of Aššur-nasir-apli II (883-859 B.C.), Tiglath-pileser III (745-727 B.C.), Esarhaddon (681-669 B.C.) from the Central and South-West Palaces at Nimrud.** London: the British Museum. 1962. Pp. xxvi + 46. 127 plates. 6 text figures. £8 8s. 0d.

This is a work which contains some interest for Hellenists. It consists of a short introduction by

Dr Barnett, a catalogue by Professor Falkner of every relief of Tiglath-Pileser III now known, either in *corpo* or only from the drawings of Layard and others, and excellent reproductions, the drawings and their originals being placed, where possible, side by side. Since some of the reliefs were incorporated in the later 'South-West Palace' at Nimrud, the other sculptures, earlier and later, from the same building are included for completeness. A great deal of work has gone into the compilation, for the reliefs (often previously unpublished) are distributed over eighteen museums in three continents. Further, Barnett has continued the work of C. J. Gadd in arranging the slabs in their correct order, and in identifying, with some success, the events and locations shown.

This last process, however, naturally tends towards controversy. One of the criteria most used by Barnett (and by Falkner, who offers a typology of her own) is the shape of helmet worn. Barnett cites a number of representations, found in various places, as parallels: several of them have been generally understood to portray Greeks. Thus it will come as a shock to many to be told that the archers on the Fortetsa bronze girdle are Urartians defending a fortress (p. xx, n. 4). Again, the well-known painted plaques from Pazarli are cited as evidence for Phrygian armour (p. xxiii): it is surely far more likely that they are simply modelled on East Greek art, as the shield-blazons and the helmets themselves suggest: the 'high buskins' look more like a misunderstanding of the Greek greave. This particular identification is put to unfortunate use, since Barnett concludes that a fortress shown on one of Tiglath-Pileser's reliefs, in which there are warriors with helmet-crests similar to the Pazarli ones, is to be located in or near Phrygia: whereas the warriors in question, as is twice observed by Falkner (pp. 15, 40), are really attackers who have forced an entry, and are therefore Assyrians or their auxiliaries. Helmets thus give no clue as to the venue, which Falkner (p. 42) would be inclined on other grounds to place in Media. In fact this type of crest is common in Assyrian and Greek art, and known from an actual find in Greece—the Geometric helmet from Argos, for which incidentally pl. lxiii provides a strikingly close parallel in the shape of crest and helmet, confirming their Near Eastern origin. Further, the figure on a new ivory from Gordion is said to wear an Oriental cap with 'Greek cheek-pieces' (p. xix), whereas he most obviously resembles contemporary Greek warriors with Corinthian helmet, forward-curving crest and shield decorated in concentric circles (as the lead figurines, *Artemis Orthia*, pl. 183).

But in general its completeness, homogeneity and above all its manageable size will make this an invaluable reference book. One is left with a feeling of thankfulness for Layard's industry and, in the main, his accuracy, though excessive trust is sometimes placed in this last. Two of Barnett's helmet-types (his figures 2,1 and 10) probably arise from

Layard's occasional errors in copying, which can be appreciated from the juxtaposition of his drawings with the modern photographs.

A. M. SNODGRASS.

Olympia. vii. Bericht über die Ausgrabungen, von E. Kunze. Frühjahr 1956 bis 1958. Berlin: W. de Gruyter. 1961. Pp. v + 223. 88 plates. 112 text figures. DM 52.

Olympiabericht VII in every way maintains the high standard of its predecessors, and we may be thankful that the seemingly endless riches of this site are in such hands. Nearly three-quarters of the text is again from the pen of Dr Kunze: the emphasis is largely on the bronzes, but a Laconian cup, four inscriptions and a virtually new treasury are also here published.

Dr Kunze begins with a brief account of the campaigns of 1956-8, which produced the most spectacular finds from Pheidias' workshop, and in which the final excavation of the Stadium was undertaken: but the later sections of the report cover objects found as recently as the autumn of 1960, a praiseworthy if at times confusing policy. Something over 3 million cubic feet of earth have already been moved from the Stadium, but the recovery of the bronzes, some of them in superb condition, goes on with the same skill and devotion. It is, moreover, now abundantly clear that there were large-scale repairs to the Temple of Zeus in the mid-fourth century.

A. Mallwitz reconstructs a treasury from half a dozen blocks found by Dörpfeld and mislaid for nearly 80 years, together with several new fragments found built into Roman walls. This fascinating exercise gives an illustration of Doric heterodoxy (the façade metopes must have been narrower than the side ones), but in the final section, where the fragments are allocated to 'Schatzhaus II' which is in turn identified as the Syracusan treasury of Gelon, the need for a plan is felt.

The kernel of the report is a 70-page study of helmets by Kunze, who this time is concerned with two varieties of the most important type, the Corinthian. His knowledge and understanding of the subject are unsurpassed. In 1936 E. Kukahn, in the standard work on the subject, gave 5 and 27 examples of the two sub-forms in question: Kunze now catalogues 15 and 74 respectively, most of them products of his own excavation and research. The first variety is characterised by two parallel 'crest-ridges' running fore and aft over the crown: Kunze is later able to give, on a new figurine of a goddess (p. 160), the first known representation of the type. The group begins early and Kunze's dating, between 700 and 640, is the only possible one: but it is rather heterogeneous, and despite Kunze's verdict (p. 76) I still feel that two examples with long, raking shape (his nos. 1 and 11) are suspiciously close to the Cretan variety. It will be interesting, when Kunze turns to the early plain helmets, to see the chronological conclusions: some are of even more primitive

shape, and they include the helmet in Cadiz found in the River Guadalete.

The other group is larger and later, the first 'classic' form of the Corinthian helmet. By analysis of the incised decoration that was now regular, Kunze places the group in the period 650-570 B.C. Two further examples seem, from photographs only, to belong to this class: one in Aberdeen is, exceptionally, undecorated (*Antiquaries' Journal* xxxi (1951), 68, pl. 13), the other was lent by Messrs. Spink & Son to the Royal Academy for an exhibition in 1946 (Chittenden and Seltman, *Greek Art*, 38, no. 176, pl. 34). To the representations (p. 115) one might add a solitary Protoattic sherd, Kraiker, *Aegina*, pl. 42, no. 585 (extreme left), with the characteristic jutting nose-piece and decorated rim. At the end of this section Kunze provides the one thing which is often lacking at Olympia—stratigraphic evidence. A series of wells, sunk in the north bank of the Stadium and used only for a short time, produced helmets in association with pottery: his chronology is thereby confirmed. Kunze even finds time (p. 118) to revive the desultory controversy over cymbals and shield-bosses, though in the reviewer's opinion he has joined the wrong side. The value of Kunze's helmet-studies is such that soon it will be possible to place any example in an absolutely dated series: just as he himself can now refer (p. 22) to a 'geometrische Kegelhelm' without more ado.

He next publishes the inscribed conical helmet, a trophy from the Persian wars, whose discovery aroused much interest. It is a rare piece, not only for its historical import and remarkable preservation, but as an isolated survival of an older Oriental type. There is only circumstantial evidence that it comes from Marathon: it is in any case curious that it should have been thrown down and buried by the mid-fifth century.

Kunze's next contribution is a study of a dozen figurines, some of them superlative in quality, others (like the warrior in pls. 72-3) highly effective. They range from a primitive Zeus with uplifted hands which Kunze places in the first half of the ninth century, to another Zeus (? with thunderbolt) not long before 500. Some of Miss Benton's remarks in her review of *Forschungen III* (*AJA* 63 (1959), 94-5) would perhaps bear repeating here: not least the reservations on using Attic Geometric figures to date what are often Peloponnesian bronzes. Nor will everyone accept Kahane's chronology for the vases themselves, which is still maintained.

Laconia looms large in this volume. Kunze identifies his two latest figurines as Spartan work: Willemssen toys with Laconia as the source of a fine tripod-leg incised with unusual mythological scenes, but wisely prefers Corinth; and Fr. Goette publishes an inscribed cup from the Naucratis Painter's workshop, which carries on his manner into the 'Droop cup' shape of Laconian IV.

Finally, four new inscriptions are published: the memorial of a treaty between the Sybarites and the apparently unknown 'Serdaioi', remarkable as the

first known epigraphic monument of Sybaris: fragments of two stelai which can be associated with the short-lived Arcadian League; and a second century victory-inscription of the wrestler Leon, a hitherto unknown Periodonikes. Not content with his earlier efforts, Kunze himself publishes three of these.

The presentation and illustration are as fine as ever: of the occasional misprints, it is worth noting that on p. 175, l. 24, 'Athen 2633' should read 'Athen 6233'.

A. M. SNODGRASS.

DELOS. *Exploration archéologique*. Fasc. xxv.

Les palestres, par J. Delorme. Paris: E. de Boccard. 1961. Pp. 183. 29 plates. 32 text figures. NFr. 90.

The author pays tribute to Ch. Avezou and Ch. Picard, who investigated the sites in 1910-13, the latter being the discoverer of the 'Palestre du Lac'. Remains of the so-called Palestre du Granit had long attracted the notice of early travellers, who observed its six standing columns and recorded the tradition that this was the gymnasium of the island (*cf.* App. B).

The work is divided into two main sections. The first deals with the P. du G., its general characteristics, description and present state, details of elevation, identification and chronology; the second deals with the P. du L. similarly, except that in the history of the monument there is available epigraphical evidence for the periods of Delian independence, and the subsequent Athenian domination, regarding the *παλαίστρα* (*cf.* App. A). A final chapter of the second section deals with the wall of Triarius which, built in 69 B.C., partly preserved and partly ruined the palaestras. There is a full apparatus of photographs, also plans and drawings, many of the latter due to the architects J. Dubuisson and Y. Fomine.

The P. du L. lies immediately north of the Sacred Lake, the other is separated from it, obliquely, by the width of a street. Both are complex sites in the sense that they bear evidence of many changes of plan. Both essentially consist of a central court (hypæthral) surrounded by colonnades backed by rooms of varying dimensions. At some period a large reservoir or tank was constructed in either court, divided into sections and roofed.

Though the Palestre du Granit is so-called, the chief materials are gneiss and poros. The ground floor is occupied by eighteen rooms in three wings on the east, south and west sides, the north being reserved for a double portico. Within the entrance 'vestibule' were found three granite steps beginning the (vanished) staircase to the upper storey. In a room of the south wing were the latrines and hard by in the south-west corner, lacking communication with the exterior, D. identified two rooms as the *λουτήριον*. No finds *in situ*, epigraphical or otherwise, prove that this building was a palaestra, but D. relies on the comparative method to establish its use.

The P. du L. in its present, or final, state appears in the form of an imperfect quadrilateral; it is rather

smaller in area than the other palaestra. The court was originally bounded by porticoes on at least three sides, but now there is evidence of only one. Of the rooms making up the wings, that of the north-east angle is outstanding because of the excellent preservation of its walls and its 'installations hydrotherapiques'; it was the bathroom. Further south along the east side are two exedras, their walls pierced with niches, at the base of one of which is a large block of Tenos marble bearing a dedication to Hermes made by Eurymantes, son of Athenis, winner in the lampadephoria (early third century); near another niche is inserted a block bearing an inscription in honour of the gymnasiarch Pausanias (158/7). At one end of the west wing were found latrines; at its opposite end a semi-rotunda or exedra forms a curious excrescence. About one-third of the area is occupied by the court, wherein is the reservoir, of unplumbed depth, divided into two compartments. Subsoil of wings and court alike contained foundations of older buildings from which the history of the palaestra is tenuously deduced for the period earlier than the accounts of the Hieropoei; for D. has no doubt that the P. du L. is the *παλαίστρα* so often mentioned in them. Certainly the dedications found in the east wing indicate a place devoted to physical education. The changes wrought in the Athenian period, in the later second century, affected all the wings and swept away the large exedra; there appeared bathroom, latrines, and even a *concamerata sudatio*, 'dont jusqu'ici on n'avait jamais retrouvé de trace dans un gymnase hellénique' (p. 145)—cf. Vitruv. v 10, 3.

The plan of the early, amphictionic palaestra on this site is difficult to trace from archaic substructures. The west wall may be as old as the fifth century. Certainly the area north of the Sacred Lake was inhabited in very early times.

The last Appendix (D) deals with the objections of J. Tréheux regarding the meaning in inscriptions of *κάτω παλαίστρα*, which D. says means ground-floor of the P. du L., and of *ἀρχαία παλαίστρα*, mentioned once in 218.

The photographs are mostly sharp and clear; one (fig. 131) shows remains of elegant paving, which D. could not bring himself to destroy. I have noted only trivial printing errors: p. 39, l. 4 *read* *chambres*, p. 44, l. 3, lack of a spacing, p. 119, l. 13, wrong spacing.

W. A. LAIDLAW.

Corinth. Results of Excavations Conducted by the American School. Vol. 1, part v. The South-East Building. The Twin Basilicas. The Mosaic House. By S. S. Weinberg. Princeton: the American School of Classical Studies at Athens. 1960. Pp. xviii + 128. 10 plans. 57 plates. 29 text figures. \$12.50.

This impressive and finely-produced volume marks another big stage in the exploration of the Roman market-place at Corinth. With the Julian and South Basilicas and the 'south-east' building, the

whole of the exploration of this area, which has gone on intermittently for forty years, will have been completed, with the exception of a small space above the market-place, known as the West Shops.

The Julian and south basilicas were identical buildings, and the careful excavation which Professor Weinberg describes in exact detail have enabled their plan, purpose and history to be recovered with a considerable degree of certainty. Both basilicas originated in the early years of the reign of Claudius. Their purpose seems to have been, to quote their excavator, to 'serve as entrances to the Agora, as places of gathering and of retreat from inclement weather, but principally as large commercial halls for the display and storage of wares'. They could also have served as the seat of magistrates (p. 109). They remained apparently in use until 375, when the Julian basilica was levelled by a great earthquake which destroyed most of the buildings in the Roman agora. The south basilica, however, survived another twenty years, when it succumbed with the rest of Roman Corinth to Alaric's destruction in 395. Unlike the Athenian Agora, that at Corinth showed no sign of barbarian visitation in the latter half of the third century.

Of the two other buildings investigated, one, the south-east building, may have been a *tabularium*. It was a symmetrical structure, divided into two parts which were united by a colonnade facing the Agora. The suggestion that one part may have been a library and the other a depository for archives seems reasonable. It had a long history, continuing to be used for some purpose at least down to the reign of Justinian. The second building abutted the south basilica and was a large and well-appointed private house which produced a very fine series of Dionysian and other mosaics, probably of early third-century date.

The report goes into great detail regarding the major discoveries. A feature is the massive research into recent excavations in the Mediterranean in order to try to establish analogies to the mosaics found in the Mosaic House and south-east building. One may not feel at home with this method, in view of the long life of many common patterns, but the mine of references to similar mosaics in other parts of the Roman world is not the least useful part of this report. The standard of photography and draughtsmanship is exemplary. One might perhaps regret the lack of a stratigraphic section of the site, and one assumes that the cataloguing of small finds and pottery is being reserved for another volume. All in all, the achievement of the American School on the Agora at Corinth approaches the standard of their work at Athens, and few praises can be higher than that.

W. H. C. FREND.

Lindos. Fouilles et recherches 1902-14, par C. BLINKENBERG et K. F. KINCH, et 1952, par E. DYGGVE. **Fouilles de l'Acropole, 3. Le sanctuaire d'Athana Lindia et l'architecture lindienne,** par E. DYGGVE, avec un

catalogue des sculptures trouvées sur l'acropole, par V. POULSEN. 2 vols. Copenhagen: G. E. C. Gad/Berlin: W. de Gruyter. 1960. Pp. 576. 1 folding map. Numerous plans, plates and text figures. D.kr. 300./DM 171.

Lindos, on the east coast of Rhodes, was one of the three original Rhodian cities, and its famous temple of Athena remained a great shrine in the Hellenistic and Roman periods. K. F. Kinch, who excavated this sanctuary between 1902 and 1905, in fact discovered an architectural ensemble of the early Hellenistic age, the most creative in Rhodian history. German scholars, who often overrate Pergamum, should remember Rodenwaldt's words, rightly quoted by Dyggve on p. 9: 'Die Blüte der pergamenischen Kunst verdankt der Insel Rhodos ihren Namen in der Kunstgeschichte.' Actually, these buildings are too pure and too ably planned to have much in common with Pergamum.

Many mischances, including successive wars and the death of Kinch in 1921, postponed publication until 1960. Fortunately, H. Rasmussen had made a very careful plan of the site (PLATE IIA here), and the excavators' notebooks enabled Dyggve to publish it. The buildings themselves have suffered from neglect and spoliation in places (e.g. the Boukopian) off the tourist track, and from 'restoration' of the show-spots. This is not the only *ensemble* to be 'restored' before it was even published. Dyggve's painstaking exposure of the 'anastylotes' mistakes should be advertised widely to officials and fashion-journalists. Among the errors, one must record (1) that the wrong *poros* was used for the new parts—p. 39; (2) that the ancient approach has been wrecked by the modern stairs—p. 58; (3) that the junctions of the temple's cella-walls have been confused—p. 88; (4) that the original masons' numbers for stones and courses have also been confused—p. 91; (5) that six column-drums, originally set at the same level, as is proved by ancient cuttings in them for a cross-bar, have been set at different heights—p. 102; and (6) that a capital of the propylon has been used on the 'restored' stoa below it. Moreover, exposure of the temple's decayed abaci has destroyed every trace of their remarkable moulded crowns—p. 103.

Beginning with the temple, on the crown of the Acropolis, Dyggve works outwards, or northwards. Like all the buildings, the surprisingly small temple, only some 22 metres long, depended for its effect on a stucco-finish. The earlier coat has fourth-century profiles, the later an early Imperial character. The plan was tetrastyle amphiprostyle, the order Doric and the siting perilously eccentric, over a holy grotto. Dyggve heightens the columns to about six lower diameters. This is the contemporary proportion, and was later exceeded on the stoa below, as we know from a stretch of it always preserved (*cf.* PLATE VIC). To crown the other buildings, the temple must have columns at least as high as this. Temple and Stoa both had columns with an *arris*, not a flute, in the centre of each shaft on

elevation. The architrave was strangely low, and the corner triglyph actually projected—surprising and undatable solecisms. The annulets 'broke line' with the echinus, and the abacus had a moulded crown. There were no orthostates. Dyggve dates the temple after the fire of 342 B.C., known from the Lindian Chronicle.

The propylon covered a remarkable sixth-century staircase, 7 metres wide. It masked the entire temple court, in which Dyggve rightly places an altar, and was a decastyle stoa with two projecting tetrastyle wings. A terrace filled the whole area between the projections, and was approached by an enormous staircase of its own width (20 metres). It is pleasant to see this anticipation of the great Roman stairway to the Athenian Propylaea, recently destroyed. The whole Propylon was about 120 feet wide, with an order about three-quarters of the temple's. Behind it, a lower internal peristyle accommodated itself to the temple's eccentric siting. The details in the Propylon are more correct than in the temple. For instance, its architrave is relatively higher, and the annulets do not break line with the echinus. So Blinkenberg put it in the fifth century. Dyggve dates it after the temple and therefore about 300 B.C., because its truncated inner court presupposes the temple's pre-existence. However, the temple, which had to occupy its site for ritual reasons, might, on the present evidence, have been rebuilt in the third century.

Before and below the Propylon was an even larger building, the Stoa, with an order more like the temple's (Dyggve, p. 183), even down to the *arrises* in the centres of the elevations. It was 87 metres wide, with deeper wings than those of the Propylon, and had in all 42 columns on the front. The eight in the centre formed a mere screen across the lower end of the stairs up to the Propylon. According to Dyggve's text (not borne out by his plates), they slightly curtailed the stairs—which shows that the Stoa is the later building. There were some strange *antae*, half-columns attached to the normal thickened wall-ends. The architrave, too, projected beyond the wall-courses of the two end-walls of the stoa—another rare solecism, perhaps showing an Ionic tendency. From the moulded details, Shoe dated this stoa to the later fourth century. But Dyggve considers its materials and mouldings inferior to those of the Propylon, and therefore later—say, late third century. Besides, the lower stretches of the column-shafts are planed, not fluted, so are hardly much earlier than 200. All the same, many details puzzle. Why, for instance, do the flutes, properly segmental halfway up the shaft, become so flat (PLATE VI, D) below the echinus? This seems to defy most Doric precedent. But Dyggve has done all that he can to date Temple, Propylon and Stoa, in that order, between 350 and 200 B.C. Finally, towards 100 B.C. the Lindians built out the Stoa's forecourt above a row of arched cisterns, which reminds us of a Republican Roman substructure.

Brief chapters follow, on a small Corinthian temple facing this outwork and of late Imperial date, on exedrae used in minor cults, on the numerous cisterns (the only direct source of water here) and on the roof-tiles found about the site, among which Dyggve identifies a pantile from the Temple, 57 cms. wide, just half the distance (116 cms.) between the axes of any two triglyphs and perhaps representing two Lindian feet. The Theatre south-west of the Acropolis, is less interesting than the large tetrastoon (44 by 31 metres) beside it, obviously meant to supplement it for certain purposes—Dyggve suggests for the more crowded festivals. Chapter 12 describes three holy places at Lindos, the grotto below the temple, the Bonkopion (a rude temple with pronaos and opisthodomos) and a small Heroon, a very truncated version of the type best known from Daochos' monument at Delphi. In the Necropolis, the two chief monuments are a round mausoleum, with a corbelled burial-chamber of sophisticated construction (the key-stones seem to me to have a Roman ingenuity), and the Archokrateion, a rock-hewn tomb consisting of a columnar façade, open in the three central bays to reveal a rectangular chapel with surrounding 'loculi'. It is securely dated by the persons commemorated to the years around 200 B.C., and is memorable for a unique attic, once beautifully decorated, nodding forwards over the entablature and designed to support four cylindrical altars. Here we have an originality rare in Hellenistic work. The appendix is a catalogue by V. Poulsen of sculptures connected with Lindos. They include fragments of well-girdled archaic Korai, a typical mid-Hellenistic statue with over-crossed drapery, a Praxitelean female head, the head and torso of colossal Athenas, a fine horse's head and a pretty head of Athena in relief.

Besides describing Lindos so fully, the two volumes are also valuable for vindicating the accuracy of Vitruvius. Several walls offer prototypes of his 'emplekton' masonry (pp. 41 and 393). Again, the Tetrastoon by the Theatre obviously served, among other things, those purposes which he required of stoas near theatres, and recalls his example, the fifth-century Odeum at Athens (Vitruvius V 9, 1). The polychromy of this building was partly preserved, and included the blue triglyphs which he postulates in iv 2. ii.

Dyggve himself, though an admirable researcher and compiler, comes less well out of his architectural discussions. He tends to overstress his own past field-work, and to ignore relevant material outside it. Why, for instance, should the late Imperial temple, of 6 by 11 columns, be Diocletianic, just because it is nearly the same size as one he has seen at Split? On the other hand, he does not compare the moulded crowns of the main temple's abaci with the similar examples in the Treasury of Cyrene at Delphi. Yet this also contained antae with attached half-columns, like those in the Lindian Stoa. Is it possible that the front of the Lindian Temple (not its rear) had similar antae, and that the column's

unusual abaci were meant to harmonise with the anta-capitals? There was at least one smaller capital of the 'Cyrenaic' type at Lindos (PLATE VII, C), which Dyggve dismisses, assigning instead a flattened Ionic capital to the capital of the Stoa. This he calls 'Macedonian' (p. 233)—I do not know why, since it is found not only at Calydon but at Canosa, in lower Italy, in a third-century tomb published by Nachod in *RM* 1914.

On p. 516, seemingly in deference to von Gerkan, he thinks that axial planning was important in Hellenistic Priene, as it assuredly was not. He also accepts (p. 175) von Gerkan's restorations of various Ionian altars, and compares them with the façade of the Lindian Propylon, though he honestly admits that its date is far removed from that which von Gerkan gives the altars. He supposes that Pausanias, in vi 4, ii, distinguishes the axial planning of Ionian agorai from the non-axial layout at Elis. But Pausanias merely says that the buildings were connected at Ionia, isolated at Elis. Dyggve cannot have read Wycherley's excellent discussions. Similarly, he cannot have seen my own discussion of the modillions inside the Tower of the Winds, which he compares (pp. 320 ff.) with some on a monument at Lindos (PLATE VII, G). At Lindos, as they should, they surmount dentils and a squat cyma-recta frieze, of typically Hellenistic proportions: at Athens, uniquely, they come below the dentils—facts ignored by Dyggve in his far-fetched discussion. The inner façade of the Lindian Propylon has a squatter order than the outer, just as in the Athenian, and for the same reason—that the inner façade is higher up, on the crown of the hill (PLATE V, L). Dyggve inappositely cites Vitruvius on Rhodian peristyles, which are merely the inside courts of private houses, where one side is higher than the rest. I cannot follow Dyggve and see the influence of stage-buildings on almost any long Hellenistic façade. Finally, why call the rock-hewn seating of the Theatre 'maeniana' (p. 402)? Maeniana were galleries raised on colonnades (Vitruvius v 1, ii).

This imperfect knowledge vitiates, I think, most judgments in Dyggve's last chapter. The majestic and intricate design of Praeneste surely owes hardly anything to Hellenistic precedent. Pergamum may owe much to Rhodes; but Roman architecture, despite Dyggve, owes very little. The charge from an asymmetric to an axial planning of towns becomes possible only with the general adoption of Corinthian in the Augustan Age.

A few minor comments. The akroterion described on pp. 354-6 seems too small and probably too old for the temple of c. 325 B.C. The painted wings could hardly be so late. The Gorgon-antefixes of p. 365 were dated by Kinch in the earlier fifth century. The bare forehead between the locks runs up into a point, and is thought by Gjerstad to make this type of Gorgon rather later. However, this is an Early Classical form of coiffure (cf. Schuchhardt in the *Festschrift* for Karl Weickert). Pp. 393-4 say that Vitruvius called 'emplekton' a technique 'sans

mortier'. Fig. xi 24 conflicts with Fig. xi 10 and the text. Did single blocks make up stretches of combined architrave and frieze, or not? Fig. xi 22 surely represents not a 'Kymation dorique à feuilles' but a Lesbian Leaf. P. 525: should one talk about Templars in Rhodes, or Hospitaliers?

Misprints already unnoticed on p. 577 are few. On p. 84, in lines 4 and 7, for 'Pl. IV, B' read 'Pl. IV, C'. P. 117, at top: 1.20 is not the length of one triglyph *minus* one metope, but *plus* one metope. P. 145, Fig. iv 18, 5: the Temple of the Athenians on Delos is neither Ionic nor third century. P. 234, v 11: an 'n' has dropped out. P. 496, second paragraph, last line: delete 'et'.

The system of numbering figures and plates, though unusual, is easily mastered and foolproof. But I wish that more scales appeared on the drawings. It is much harder to work with sheets merely labelled 1: 100 or 1: 200. Nor can one always trust the accuracy of the printer's reductions.

HUGH PLOMMER.

ANDERSON (J. K.) **Ancient Greek Horsemanship.**

Berkeley and Los Angeles: University of California Press (London: Cambridge U.P.). 1961. Pp. xv + 329. 39 plates. £3 10s. od.

This is a pleasantly written, well-produced book with many excellent illustrations. Mr Anderson has technical knowledge and practical experience, and I hoped that he would solve all my problems for me, but some still remain. The explanation of the mule's nose band (42, pl. 2b), clears up many difficulties, but ch. III is all too short. It confirms me in my belief that A. Roes's 'solar horse', *Geometric Art*, fig. 43, is a race-horse, wearing a halter fastened to his noseband, and is not eating a snake as Mrs Roes has it. I doubt though whether all baggage animals always wore so elaborate a harness, and whether walking-sticks were never used for guidance in ancient Greece. I used mine thus in 1956 in Arcadia. Mr Anderson says that it is not common in Greece today to ride a horse on a halter alone. Some Greeks just loop a rope round the creature's neck on a slip-knot. It fell to me to help to rescue a pony so harnessed from the swollen Olynthos river: the more we pulled the tighter went the loop, and the less interest was taken by the pony in his rescue. It would be strange if this harness were a modern invention.

Besides being a horseman, Mr Anderson is a far-travelled and a widely-reading historian; in this book his parish extends all the way from Britain to the Far East. Some of his less skilled readers might have preferred him to have stayed in Greece to describe Greek horsemanship. It would have been easier if he had referred to his illustrations at the beginning instead of at the end of his descriptions.

Mr Anderson is cautious (66) about Geometric bits but he seems to think that they existed. Indeed how could anyone guide four horses from a vehicle with the stability of a coal-scuttle, without one? However, Blegen has put the matter beyond doubt

by publishing fragments of two light snaffle bits found in the Agora, in an undisturbed grave XXVII, containing the earliest kind of Attic Geometric pottery (*Hesp.* xxi 281). The fact that these bits were not cast, like the Mycenaean bronze bits, but made of twisted iron wire may explain why no others of this period have survived. Bronze was no doubt almost unobtainable at Athens at that period. Contrast Olynthos x, pl. clxii. A cheek-piece here, no. 2555, reminds us of Corinth in the seventh century. Cf. the horses on Payne *PV*, pl. 22.1.

Women have some few advantages: Mr Anderson never saw a fierce Girton don convert an ankle-length, girt chiton into knee-length in one second. There is no evidence that Attic chitons were sewn or confined except by girdle and fibulae. Orythia can stride away from Boreas. Our author is no doubt correct in saying (113) that Attic women did not ride horses, but it was not their clothes that kept them grounded.

There is much entertaining light reading in the book, but I must warn readers that ch. X, Advanced Equitation, is difficult. 'High School' is not a happy translation of *haute école*, especially in America.

Among the many good things in this book is the Appendix, a good translation of Xenophon's *Περὶ ἵππων*. I hope it will also be published as a separate pamphlet.

SYLVIA BENTON.

MYLONAS (G. E.) **Eleusis and the Eleusinian Mysteries.** Princeton: University Press (London: Routledge and Kegan Paul). 1962. Pp. xx + 346. 88 illustrations. £3.

It would be pleasant to give this book an unequivocally warm welcome for one can only echo M.'s belief that 'there is urgent need of a general account of the Sanctuary and the Eleusinian cult for the student and the interested non-specialist, scholar and layman alike'. M. should have written a good book. His enthusiasm is tempered with scepticism, and he has known the site for over thirty years. The result, however, is unequal and disappointing.

Fortunately, he does know the site, and about half the book is devoted to its building history. Here, the evidence is clearly set out, well supported by photographs and plans by Travlos. The most controversial question is that of the age of the cult. M. thinks that the tradition of Bronze Age origins is valid, and finds the Bronze Age temple in the building known as Megaron B. He thinks this too small for a palace; that it is far larger than anything else which could be described as a Late Bronze Age shrine does not bother him. M.'s faith in tradition is well known, and a reference to *JHS* lxxix 217 saves me from having to say what I think about his treatment of the Homeric Hymn and the Atthis. Back on his home ground, he frankly admits that there is no Mycenaean evidence for the Demeter cult.

Once past this hurdle, we are on firmer ground. There is a careful discussion of the evidence for an eighth-century apsidal temple, though no recogni-

tion that its existence spoils the view that the Hymn must be referring to Megaron B. The view that the Anaktoron was very small is convincingly argued. The account of the later history of the Telesterion is clear and helpful; it never had a second storey. On the wider history of the site, M. will be found a useful guide. He and his pictures are a pleasant alternative to the smoke and dust of the cement works which make Eleusis one of the least enticing sites in Greece.

There follows a chapter on Art and Eleusis, and here our troubles really begin. To begin with, it at times degenerates into a Guide to the Eleusis Museum, containing much superfluous matter. But those who go to the Eleusis Museum may notice some curious omissions. They will be struck by the wealth of votive plaques. Some go back to the seventh century and have tripods on them, and have parallels from the Eleusinion at Athens (*BSA* xlix 197-200). Why did Eleusinian worshippers feel the need to dedicate tripod-plaques? 'Tripod' makes no appearance in the index. There is the fine b.f. plaque by Euphiletos (*ABV*, p. 352). Surely that has some relevance to the dress of the officiants, a subject on which M. has much to say (they had no rich robes until Aeschylus showed the way). It is not in M. In fact the only plaque discussed in M. is the Ninnion plaque. If it gets nine pages, its companion-piece (Nilsson, *Gesch.*, pl. 41 left) surely deserves more than a bare mention. The Eleusis visitor will also note fragments from a b.f. loutrophoros (I think, *ABV*, *Swing Painter* no. 97) with a procession of basket-carriers on the neck, and, on the shoulder, a procession of men carrying ears of corn led by a flautist and lyre-player. He may well think it relevant to the cult; he will not find it in M. In any case, M. adopts a view which means that there is no real point in discussing any artistic representations. What is secret cannot have been revealed; therefore, anything depicted in art cannot have formed part of the Mysteries. Not of the Mystery itself, agreed, but was there no overlap between anything said and done at the Mysteries and more general, more public legend?

This theme is developed in a section on secrecy which leads to an account of the public aspects of the cult. M. seems interested in this topic, but his account of the functionaries of the cult and the public proceedings cannot be seriously regarded as an up-to-date guide. The ominous signs are in the footnotes; inscriptions long since safely enshrined in the *Editio Minor* are referred to by their appearances in the old *Corpus* or periodical publications. M. has just not done the work himself, and clearly thinks that inscriptions are things that people called epigraphists work on. Not surprisingly, he has missed a good deal. Our principal authority for the public history of the cult is *IG* i² 6; it receives no systematic discussion. *IG* i² 313. 150-65 has an account of expenses on the Mysteries in 408; it is not used. *Hesperia* iv 21 gives us a unique list of Eleusinian deities receiving sacrifice c. 400; not here. It is hardly surprising that much of what we are told is

wrong. 'After (the fourth) century (the Kerykes) are no longer mentioned.' Cf. p. 161, just for a start.

The foundations have been inadequately laid, and there is no point in pursuing M. into the core of the Mystery. Here there have been much worse accounts, but those who start with the presupposition that the secret must have been kept need not be surprised if their investigations lead them to the conclusion that the secret has been kept. The Fathers come in for some violent treatment, and M. ends in a curious combination of piety and agnosticism, having written a book of considerable use, which could have been a great deal better.

D. M. LEWIS.

GOMBRICH (E. H.) **Art and Illusion: a Study in the Psychology of Pictorial Representation.** (A. W. Mellon Lectures, 1956.) London: Phaidon Press. 1960. Pp. xxxi + 466. 320 illustrations. £3 10s. od.

In the History of Art, as in most other fields of scholarship, modern specialisation is such that one would have thought it hardly possible for one man to write a work covering the whole subject, from Palaeolithic cave-paintings to the movements of today. If done at all, one would expect to find it the work of professional book-maker with no specialised knowledge rather than of a serious scholar. A few years ago Professor Gombrich proved such preconceptions wrong with his *Story of Art*. He has now followed up that remarkable achievement with *Art and Illusion*, a formidable and fascinating attempt to penetrate deep into the intentions and processes of art: the relation between what man sees in the world about him and what he makes of it in a work of art—or rather, perhaps, between what he makes and what he sees; for it becomes Gombrich's contention that the artistic process is one of 'making and matching'. In the beginning the artist is not an imitator but a creator. Only later, in certain times and places, does it become the aim to bring art as close as may be to nature; and then the artist sets what he has made, or what his masters have made, against what he sees, and so the next time can bring the likeness a little closer. The first and most extraordinary of such *rapprochements* of art and nature is that brought about in ancient Greece; and the chapter *Reflections on the Greek Revolution* is not only the one of most concern to readers of this Journal, but a key one in the book. Here Gombrich starts from Loewy's account in *The Rendering of Nature in Early Greek Art* which, as he justly says, remains unsurpassed as 'a description of what happened'; but he points out (surely also justly) that Loewy gives really no explanation—that the 'conceptual' approach which the Greeks learned to slough is that normally prevalent, and that its abandonment was not simply the natural concomitant of developing civilisation, since many highly complex civilisations had flourished before without feeling the need for this change, and the artists of Byzantium later did

their best to go back from it. In a valuable passage Gombrich suggests that the conceptual arts of early civilisations are more profitably compared to cartography than to children's work, their conventions being adopted primarily for clarity and easy legibility of the message. He is inclined to link the development of naturalism in Greek art with the idea of 'fiction'—the interest in story-telling, the creation of an imaginary world, known to be imaginary but accepted as having its own reality, which begins with Homer. At one point he states his position thus: 'Professor Hanfmann . . . succinctly sums up the prevailing view: "When classical sculptors and painters discovered a convincing method of representing the human body, they set up a chain reaction which transformed the character of Greek narration." . . . I feel prompted to put forward the opposite hypothesis: when classical sculptors and painters discovered the character of Greek narration, they set up a chain reaction which transformed the methods of representing the human body—and indeed more than that,—i.e. the world of space and light in which man has his being. I find myself in whole-hearted agreement with this reversal, and with the following passage which stems from it: 'By taking the history of the freestanding figure more or less in isolation we arouse the impression of the Sleeping Beauty, but we miss the life-giving kiss. Is it not much more likely that the discoveries which infused life into the freestanding single statue were first made in narrative contexts that demanded a convincing re-creation of a situation—for instance, in the narrative groups of pediments with their dramatic evocation of mythical episodes?' I have been able to touch only on one part of a highly complex work, of which all parts are closely interwoven, and in which the author shows himself at home in psychology and in the physical problems of perception as well as in the arts of all ages.

MARTIN ROBERTSON.

Lecce. Museo Provinciale 'S. Castromediano'.

Vasi dello stile di Gnathia. Vasi a vernice nera. By M. BERNARDINI. Lecce: Museo Provinciale 'Sigismondo Castromediano'. 1961. Pp. 30. 68 plates. L. 8,000.

This volume, which corresponds closely in style and appearance to a fascicle of *CVA* but is published independently, contains the Gnathian and plain black vases in the Lecce Museum, adding to the few better-known pieces already included by Romanelli in Lecce *CVA* 1 (iv Ds pls. 1-5) a wealth of new material of the greatest value and interest to students of South Italian pottery in the Late Classical and Early Hellenistic periods. Comparatively little study has yet been devoted to later Apulian red-figure and even less to the non-figured ware, but the material here made available, together with that published in some of the recent fascicles of *CVA* (notably Naples 3, Capua 3, Karlsruhe 2, Altenburg 3) will go a long way towards providing a sufficient basis for a more detailed

classification of Gnathian and plain black vases than has yet been attempted.

Bernardini prefaces his volume with a very brief introduction; perhaps even too brief, since, in view of the present limited state of our knowledge of Gnathian, a fuller survey of the fabric would have been most welcome, especially in the light shed by the Lecce vases upon proveniences and chronology. The author plausibly suggests, on the evidence of the discovery at Rocavecchia of a kiln containing many ribbed oenochoi (*Scavi di Rocavecchia dal 1928 al 1944*, p. 10, figs. 12-13), that since such vases must have been made at various local sites in Apulia, as well as at Taranto, it is likely that the same is also true of Gnathian; indeed, the original exclusive association of the term with Egnazia has long since been abandoned, and 'Gnathian' has now become the accepted designation for that large class of vases, decorated in white and various shades of red and yellow paint applied to the black glaze, which are found not only throughout Apulia but in Campania and Sicily as well.

The author makes almost no reference to chronology, but it is now clear that the third-century date originally proposed for these vases is far too late and that they must begin not long before the middle of the fourth century (*cf.* Scheurleer, *AA* 1936, 285 ff.; Webster, *JHS* 1951, 225 ff.; Trendall, *VIE* ii 212 ff.; Forti, *Rend. Nap.* 33, 1958, 224 ff.), as may be seen from the parallels between the figures on some Gnathian vases and those in added white frequently found in the heroön scenes on Apulian r.f., or between the female heads and heads of Nikai (e.g. pl. 23, 1-5, pl. 30, 5-6, pl. 31, 1-2, pl. 48, 3, 4 and 6) and those which figure on the necks of Apulian r.f. volute-kraters and other vases (*cf.* Cambitoglou, *JHS* 1954, 111 ff.). The many Gnathian vases found in recent years at Rocavecchia confirm this dating, since they have mostly come from tombs which may be dated between 350 and 300 B.C. by the other vases they contained, and it is a pity that Bernardini in republishing them here does not relate them more fully to their contexts, which may be studied in *NSe* 1934 and 1957 and in *Studi Salentini* i, 1956, 33 ff. The latest Gnathian vases, especially those with ribbed bodies, which become popular towards the end of the fourth century (*cf.* *CVA* Capua 3, IV Eg, Text pp. 7-8, pl. 3; Forti, *Rend. Nap.* 33, 1958, 226), continue into the third century though not perhaps for as long as used to be supposed.

Some progress has lately been made towards the classification of Gnathian on stylistic grounds, notably by the late Anna Rocco (*Mem. Napoli* 6, 1942, 233 ff.), by Lidia Forti (*Rend. Nap.* 32, 1957, 57 ff.), and particularly by Webster (*JHS* 1951, 224) who grouped a number of Gnathian mask-vases according to their decoration (e.g. Konnakis, Red and White, Red Spray, Dotted Spray and Yellow Spray Groups), a system which may readily be extended to other vases and might usefully have been adopted in the present work (e.g. Konnakis Group—1292-5, 1301; Red and White—1014, 1016, 1018-19, 1131, 1151,

1064-6, 3787; Red spray—1034, 1045-7, 4892, 5077; Dotted spray—1181; Yellow spray—3802). Bernardini, however, does put together numbers of vases on grounds of similarity of shape, decoration or style and these form useful nuclei for sub-groups, within the larger groups mentioned above, bringing the time nearer to hand for a more extensive identification of individual painters.

The reproductions are in general good, though at times the variations in scale can be misleading: for example, on pl. 5 the two kraters in the centre (nos. 3 and 4) look to be of the same size but in fact are not (25 and 35 cm. respectively) and again on pl. 7, where incidentally nos. 2 and 3 have been transposed, the actual heights (37, 22, 45 and 38 cm.) are very different from what they appear to be when the four vases are illustrated together. References on the plates to the museum inventory numbers would have been a convenience.

The subjects represented on Gnathian vases are of limited range and, as the author points out, mostly associated with funeral rites or the cult of Dionysos. The mask-vases are of particular interest, and as Webster's *Monuments Illustrating Old and Middle Comedy* appeared too late to be used, it may be as well to give here the references to the Lecce vases listed in it:

pl. 6.4 (1034) = Webster, p. 52, GV 3b; pl. 10.4 (4892) = GV 19b; pl. 10.7 (5077) = GV 18b; pl. 13.3 (3789) = GV 10a; pl. 37.4 (1181) = GV 17i; pl. 37.5 (3802) = GV 16j; pl. 41.1 (5058) = GV 20a; pl. 42.3 (1872) = GV 28d.

In his subsequent article in *Lustrum* 1961/6, p. 29, Webster notes the following additions to his catalogue: pl. 58.10 (1631) to his GV 6; pl. 10.6 (1072) to GV 18; pl. 10.3 (1073) to GV 19; pl. 42.2 (1820) to GV 27 and pl. 42.4 (1873) to GV 28. He adds that the mask on pl. 41.3 (1814) appears to be unique in representing the young man with a roll of hair.

Webster's reference to 1818 (GV 22a) must be a mistake, since that vase is decorated with a dove, and it should be to 1870 (pl. 42.1). It may be noted in passing that the reference in the text (p. 8) to Webster's publication of 5077 in *Antike Kunst* 3, 1960, should be to pl. 9.3. The following other slips in the text might also be misleading:

- p. 7 (pl. 8.5, no. 1022): two diameters are given, of which the latter is correct.
- p. 10 (pl. 16.2, no. 4732): the height should be 0.075.
- p. 18 (pl. 38.6): the number should be 1132 instead of 1131.
- p. 21 (pl. 44), no. 2 has been omitted from the text.
- p. 21 (pl. 45.3-4), the number should be 1292 instead of 1293.

It should also be noted that in the references to Romanelli and Bernardini *Il Museo Castromediano di Lecce* the figure numbers are in fact the museum inventory numbers of the vases there illustrated.

Dr Bernardini, whose publications of Rocavecchia,

Lupiae and Rudiae have done so much to advance our knowledge of the archaeology of the Salento, has again earned our gratitude for a volume which, if somewhat severe in content for the general reader, yet offers a most rewarding field of study for the specialist.

A. D. TRENDALL.

HAUSMANN (U.) **Griechische Weihreliefs.** Berlin: De Gruyter. 1960. Pp. 113. 60 text figures. DM 18.50.

Hausmann presents a series of essays, rather than a detailed monograph, on a group of monuments which generally attract attention one by one and not as a genre—votive reliefs. He discusses the purpose of such offerings and the spirit in which they were given. He goes on to describe the development of the series and the main types, and he dwells here and there on the difficulties presented by particular pieces. The origins and form of most votive reliefs (a broad plaque set in an architectural frame) are related to similar monuments in other materials and to grave stelae. Attention is paid to the problems of the Laconian and the later 'Totenmahl' reliefs showing offerings to the heroised dead. Other classes popular in the classical period are reviewed—those showing the god, the deities representing states at the heads of decrees, mythological scenes, scenes of healing, etc. The author's own interest is reflected in the discussion of Asklepios reliefs and the problems of the god's relationship to Zeus Meilichios, especially in the Hellenistic period. Finally, in a study of the famous Apotheosis of Homer relief in London he argues that the statue of a poet which appears at one side of the composition should represent an epic poet of the Alexandrine school—most probably Apollonius Rhodius—and that the relief may have been commissioned by one of his pupils. Fig. 4 is the first published photograph of one of the famous sixth-century wooden plaques from Pitsa.

JOHN BOARDMAN.

Studi miscellanei, 2 (1959/60): Bocci (P.) *Ricerche sulla ceramica cicladica.* Rome: L'Erma di Bretschneider. 1962. Pp. vii + 22. 12 plates. L. 3,500.

Dr Bocci has re-examined three major classes of Cycladic pottery, and her conclusions are most easily summarised by reference to R. M. Cook's concordance of the other classifications in *Greek Painted Pottery* 344. Her 'Parian' is more or less that of Buschor and Brock. The Leiden vase and its companions are included, and with them the London griffin jug, despite its few points for comparison. She adds to these the vase in the Bibl. Nat. (de Ridder 25) which already appeared in her Parian group 3 and belongs better there. It must still surely be open to question whether the big group of vases found on Thera (her groups 2, 3 and part of 4; with the Walters Art Gallery vase and *JHS* xxii (1902) 73, fig. 3) and represented by only one fragment on

Delos (Ab20, a stranger in the group) and nothing on Paros really belongs with the 'Parian' series which is so well represented on Delos and Paros. Delos Group C is also added, the Protome Group, which Brock and others take for Naxian. The 'Siphnian' is as Buschor's, plus the new finds in Kimolos, and with Buschor she sees in Delos Group D the possible successors to the subgeometric vases, although there are no real points for comparison. The 'Naxian' is much as Brock's, less the Protome Group. The Naxian geometric includes the Delos Groups Ac, Bb, Bc (some of the smaller Bb vases surely need not belong) with large vases attributed by Kontoleon. The distinctive type might be looked for farther afield, as at Eretria, *BSA* xlvii pl. 2.A8-10, B4; 3.A8. With the orientalising Naxian vases are the Delos Group Bc and the Bellerophon plate from Thasos. It is debatable whether the attribution of this group, with its strong affinities to 'Melian', is compatible with the agreed Naxian origin of the Heraldic Group and the Aphrodite vase from Naxos (and the new fragments *BCH* lxxxv 851; *Ergon* 1961, 200 fig. 211). An early example (Bc 18) might rather qualify as successor to the 'Siphnian' subgeometric Ad group (*cf.* especially Ad 4).

The plates include a rich corpus of the subsidiary decorative motifs used in each group and good photographs of the London griffin jug and the Leiden and Stockholm vases. It is not clear how far this study is based on inspection of the vases themselves, but in the reviewer's opinion the distinctive fabrics of some of the groups are important guides, here largely ignored. The classification of Cycladic vases remains a problem. Further study must first ignore prejudices about 'Parian' and 'Naxian', must concentrate on the discernible smaller groups and determine their absolute and relative chronology with reference to more than Early Protoattic (which is discussed at length; and see now Brann in *Athenian Agora* viii 21 f.), and must, I fear, await full reports of the many unpublished excavations in the islands.

JOHN BOARDMAN.

BUSCHOR (E.) *Altsamische Standbilder, iv-v*. 2 vols. Berlin: Gebr. Mann. 1960-61. Pp. 61-72, 73-99. Plates 233-310, 311-405. Each DM 18.

This brilliant re-assembly of the archaic sculpture of Samos is now almost complete, and we are given a synoptic view of the art of a great Ionic centre no less vivid than that which we already had for Athens in the same period, through the eyes of Payne and Young. In the quarter-century since the appearance of vols. i-iii, B. has added a wealth of fresh material: wholly new dedications recently unearthed; new fragments to join old; works previously known but unpublished in this connexion, some Samian by provenance, others by style. Everything that can be called sculpture is included, from the miniature to the colossal. Vol. iv resumes the study of the

kouroi, vol. v that of the korai. (Vol. vi will contain the remaining miscellanea.)

The most exciting of the kouroi is a kneeling figurine of ivory (figs. 238-48), beautifully preserved apart from the loss of the inlaid parts. The high standard of the carving is matched by the extremely skilful use of the grain of the material to help suggest the anatomical structure. The date is c. 625. The kouroi evidently formed part of an applied decoration, B. suggests for a piece of furniture. Ohly, however, now argues attractively that the kouroi formed one 'horn' of a lyre (*AM* lxxiv (1959) 48-56).

Earlier still, B. says before 650, is a *perirhanterion* supported by three korai grasping the leashes of two lions at their feet (figs. 317 f., 321-3). The base, lions, and headless korai, are all that survive—in Berlin, 'from Samos'. Similar groups are known elsewhere, but all are half a century later or more. We may now conjecture that the origin of the type was in Samos. It inspired the more elaborate dedication of Kolaos c. 638 (*Hdt.* 4.152), which may in turn have inspired the kneeling figurine already mentioned.

From c. 630-25 we already had the shoulder-fragment of a daedalic kore (figs. 72 f., 328). Of some epigraphic importance, if it truly belongs, is a new piece inscribed [*av*]εθηκεν (figs. 329 f.). The foot-fragment of similar style (figs. 75, 319 f.) is now ascribed to a second statue. A part of yet a third daedalic kore is illustrated (figs. 325-7). All three are of Naxian marble.

The generation c. 600 is represented by a number of new pieces of colossal kouroi (figs. 253-9, *cf.* 1-4), all belonging to Richter's 'Sounion Group'.

To the well-known kore of Cheramyes (figs. 86-9, 340), are added two new offerings, at least one of the same dedicant. This is a similar but rather later (c. 560-555) kore in Berlin, from the Heraion, inscribed not 'to Hera' but 'to the goddess', whom B. identifies as Aphrodite on the ground that the kore carries a hare (figs. 341-4). [*Chera*]myes also dedicated a kouros (B. suggests to Hermes), of which only a part of one leg survives (figs. 262 f.). The identity of the dedicant is perhaps not certain: Hexamyes (*cf.* Thales' father) would do equally well, and many in Samos will have borne such Anatolian names. (For Aphrodite and Hermes at the Heraion, see *AM* lxxii (1957) 77-86.)

Somewhat later, c. 550, is the group of six statues on a single two-step base, by Geneleos. (For the reading of the dedicant's name, see now L. H. Jeffery, *Local Scripts*, 329 n. 3.) Hitherto three headless named statues have been known, and fragments of a fourth (figs. 90-101). Now we have Philippe's younger sister Ornithe, in Berlin. Her name, like that of Philippe, appears vertically on her skirt where it is drawn to the side. Scale apart, she is almost a replica of Philippe; and she fills the next socket in the base (figs. 345-50).

From c. 550-540 we have some new pieces of the 'second East Colossos' (figs. 270-4, *cf.* 25-8). For the missing head, in Istanbul 'from Rhodes', see

Richter, *Kouroi*² 127, figs. 369 f.: B. ap. Richter, *AJA* lvi (1962) 102. Of similar date is another head, two-thirds life-size (figs. 363-70), similar to a head in London from Ephesos (fig. 393), but much better. To the same decade belong two bronze statuettes from the Heraion, both in Berlin: a kore (figs. 353-6), of Cheramyes' pattern; and a kouros (figs. 275-7). The same period provides a fascinating illustration of the keenness of B.'s eye. He had compared the upper arm of a kore (fig. 124) with a figure in Berlin, from Asia Minor (fig. 125), and suggested that the latter was itself Samian. Now the rest of the kore can be seen (figs. 357-9, 361): it is almost identical with the piece in Berlin, except that the folds of the dress are a little softer.

From c. 540, and throughout the reign of Polykrates, there is a manifest decline in the size and quality of offerings at the Heraion. The korai are undistinguished, chiefly represented by one in Berlin (figs. 373-5), and two from Myli (figs. 136, 384; 385-92). For new kouroi, we have only statuettes in bronze: a pair of twins, c. 540 (figs. 285-92); and another in Berlin (figs. 295-300), an East Greek cousin of Euthydikos' kore at Athens. There are others less good. For the last quarter of the century there is, as before, a noticeable dearth of material. But B. detects a Samian hand in fragments of a kouros of c. 520 from the wall of Themistokles at Athens (figs. 307-10).

The new material illustrated in these two volumes fills out the picture given by the previous three, of the development of a distinctive school of artists over a period of a century and more, c. 650-520, from the orientalising period to the later archaic. We see that Samos was a centre, and a most important one, of a style common to her neighbours, especially Ephesos and Miletos. The corollary, against which B. is perhaps insufficiently guarded, is that it is not quite safe to declare that every work of 'Samian' style must be Samian.

A word about the form B. chooses for his text. It is that of a series of illustrated lectures, excitingly readable, and highly personal. There are disadvantages: the form offers no room to distinguish in the text what is original, what derivative. And there are no footnotes. B. compares with the group by Geneleos the kore with a fowl from Miletos (fig. 352): it would have been appropriate to refer in a footnote to the suggestion of R. M. Cook and A. G. Woodhead that the inscription on 'Chares' from Didyma is by the same hand as those of Geneleos on works of very similar style (*BSA* xlvii (1952) 163, n. 26a). References to some previous publications, however, will be found in the list of illustrations. A bibliography for the whole work might usefully accompany vol. vi.

The production is good; but where direct comparison is possible, the new plates are not always as good as the old: cf. figs. 72 and 328. Perhaps when vol. vi is published we may look forward to a re-issue of i-iii, invaluable and unobtainable.

Buschor is now dead. But archaeologists will

delight to read and re-read *Altsamische Standbilder*, not least those of us who remember with gratitude the kindness of this agile old man, springing from stone to stone as he showed off his beloved Heraion. And all who read his book will envy his individual blend of imagination and artistic sensibility, with sound archaeological scholarship rarely surpassed.

JOHN P. BARRON.

Yale Classical Studies. Ed. H. M. Hubbell and W. S. Anderson. Vol. xvi. (Davison (J. M.) Attic Geometric Workshops.) New Haven and London: Yale University Press. 1961. Pp. xi + 161. 144 figures. £3.

The search for individual Geometric vase painters began over thirty years ago, but hitherto the attention of specialists has been exclusively directed to the upper and lower limits of the Attic figured style. At or near the beginning, the great grave-markers from the Dipylon have been treated in turn by Mrs Nottbohm, Villard and Kunze, but never as exhaustively as they deserve: at the end of Geometric, J. M. Cook has illumined the road leading from the latest figured work to the earliest Protoattic. In the present monograph, Miss Davison not only expands our knowledge of these two fields, but explores the *terra incognita* in between. She attempts to isolate the output of individual 'workshops': she then notes their stylistic connexions with each other, and with earlier and later work: finally, out of her analysis there emerges the first comprehensive picture of the Attic Geometric figured style. Two important conclusions are convincingly established, with far-reaching consequences for any future study:

(a) A continuous Classical Tradition can be traced throughout the whole development of the style, from the Dipylon Master down to the Workshop of Athens 894, and thence to the Early Protoattic Analatos Painter:

(b) This development, corresponding to Kahane's *reif-* and *spätgeometrisch*, occupied no more than about half a century. (In some recent Agora publications, e.g. *Hesperia* 30, 1961, 95, the two phases have been merged in an undivided 'Late' period: but Kahane's dichotomy is well worth preserving, especially in discussions involving relative chronology. In this review, in order to avoid confusion, the phases will be distinguished as L(ate) G(eometric) I, and LG II.)

After defining her terms, the author deals briefly with previously assembled groups, arranging them in chronological order, and expanding them where opportunity offers. The chapter on 'The Dipylon Group' (LG I) breaks little new ground, and tacitly accepts several attributions to the Dipylon Master that are open to question (e.g. figs. 5 and 8, which hardly deserve more than 'workshop' status). There are signs of confusion in the lists of the Louvre krater fragments, which need to be revised in the light of Kunze's review, *AJA* 1957, 306 ff. Her 'Kunze painter' is born out of a misunderstanding, for here she has joined what Kunze had already sundered:

much work remains to be done in distinguishing the Dipylon Master's closest associates, who sometimes collaborated with each other on the same vase (pp. 30 ff.). The kraters of Villard's group look like later work from the same circle, rather than products of an independent workshop: on the other hand, the Hirschfeld krater and its group lie well outside the Classical Tradition, introducing a wooden and lifeless style of their own. The two vases listed under the Hirschfeld 'workshop' (p. 142) cannot stand: the pitcher belongs rather to the orbit of Athens 897, at the very end of Geometric, while the kantharos is surely Boeotian.

In the next chapter, 'The Early Orientalising Group' is a misleading title for the groups collected by J. M. Cook, which are still predominantly Geometric. Useful additions are made to the prolific workshops of Athens 894 and Athens 897, which at the end of LG II represent the forces of progress and reaction respectively. Within the latter group, the author has good grounds for distinguishing an 'Empedocles' hand: but his amphorae (e.g. fig. 44) seem to me earlier, not later (cf. p. 46) than the works of his colleague, who painted Athens 897: the Empedocles dogs and horses are clearly derived from earlier models (figs. 84, 47, both early LG II).

The most important of the 'New Workshops' are those which span the unexplored period between the end of the grave-markers (LG I) and the beginning of Cook's groups (late LG II). In the Classical tradition of figured drawing, the Sub-Dipylon Group (an ugly, but necessary name) provides a convincing link (especially figs. 94-5, 98) between the later output of the Dipylon Workshop (which must surely include the Baring amphora, fig. 93) and the more cursive manner of the Philadelphia Painter and Athens 894. Elsewhere, there is little or no continuity, except in the gradual deterioration of linear ornament—a topic which lies outside the scope of this monograph, but is nevertheless essential to any treatment of Late Geometric as a whole. In isolating the Birdseed Painter, Miss Davison has discovered the most influential maker of pitchers, a shape whose acme falls within this intermediate period (cf. Cook, *BSA* 1947, 153): another pitcher specialist, of narrower scope, is her Swan Painter. Other groups assembled in this chapter belong rather to the earlier and later periods already explored: thus the Lambros Painter falls into an advanced stage of LG I, and likewise the 'Oinochoe Group' are among the minor works of the Dipylon Workshop: the Hydria and Stathatou hands are both inseparable from the Workshop of Athens 894 (late LG II). The Stathatou Painter, one of the author's most attractive discoveries, may well have included the Analatos Painter among his more talented pupils. There remain the Knickerbocker, Tapestry and Burly 'Workshops', which, in the opinion of the reviewer, have too little homogeneity to prevent them from falling apart in due course.

There follows a full discussion of Interrelationships

and Relative Chronology, whose conclusions are summed up in a diagram on p. 123. If we confine the entries to the figured style, they might be slightly amended, simplified, and dated as follows:

	<i>In the Classical Tradition:</i>	<i>Outside the Classical Tradition:</i>
c. 760		
A	The Dipylon Master close associates.	No consistent figured style.
LG I		
B	The Villard Group. The Baring Amphora.	The Hirschfeld Work- shop.
c. 735		
A	The Sub-Dipylon group.	The Birdseed Work- shop.
LG II		
B	The Philadelphia Painter. Workshop of Athens 894.	The Lion and Benaki Painters. Workshop of Athens 897.
c. 710		
EPA	Analatos Painter.	? Oxford or 'N' Painter.

Finally, the Absolute Chronology. In restricting the figured style to c. 50 years, the author is supported by much internal evidence from the vases themselves, and by other factors that lie outside her field: for example, the frequent mixture of styles in individual grave groups, and the comparative chronology of Athens and Corinth. But in compressing Middle Geometric into an equally short span, she is on much weaker ground: here the grave groups are far more homogeneous, and one must also consider the two sherds, belonging to the beginning of Middle Geometric, found in Megiddo IVb/Va (*Berytus* 1955, pl. IV), a level closed in c. 850 at the latest estimate (*Iraq* 1959, 85). However, her treatment of earlier Geometric chronology in no way invalidates her main theses, which are well and clearly argued. Such an important monograph deserved an index: the illustrations often require a magnifying glass, but nevertheless form a useful corpus.

J. N. COLDSTREAM.

Corpus vasorum antiquorum. Deutschland, Band 20. München, Museum antiker Kleinkunst, Band 5. Ed. R. Lullies. Munich: C. H. Beck. 1961. Pp. 49 + 53 plates. Price not stated.

An admirable record of the Attic red-figured neck-amphorae, hydriai, stamnoi in the Munich collection. The photographs are very good; but in reviewing previous works I have often noted that photographs do not always give the brown inner markings to which the vase-painter attached great importance; and here it is clear that for much of the finer detail in the hydriai by Phintias and Hypsis one must have recourse to Reichhold's drawings. In the reproduction of the stamnos by the Harrow

Painter (pl. 240, 5) the brown lines are brought out well, but for those in the Berlin Painter's stamnos one still depends on Reichhold and *Der Berliner Maler*. For one figure, Lullies and Hirmer pl. 63 is in this point more successful.

Dr Lullies' descriptions, ample but concise, contain many good observations. On some of the vases there is new matter in the second edition of *ARV*, and in the remarks that follow I do not repeat, as a rule, what may be found there.

Neck-amphorae. Pl. 209, 1-2 is dated 'about 480', but must be distinctly later than other vases that are given the same general date. Cleaned, the lyre-player on pl. 211, 9 is now seen to be bearded, and takes his place among those discussed in CB. ii pp. 55-61. In shape, the vase is so like those decorated by the Berlin Painter that the Eucharides Painter must here have collaborated with the same potter. The juxtaposition of two other neck-amphorae on pl. 213 shows the potter connexion between two members of the Mannerist Group.

Hydriai. Lullies well notes the influence of Psiax on the early hydria pl. 219, 3-4. Pl. 223: the boy Tlempolemos is no doubt the same as is commemorated, a few years later, on a new cup by the Euergetes Painter (*ARV*² no. 52 bis) and a cup, contemporary with it, in Orvieto (*St. etr.* 30 pl. 14, b). Pl. 230, 3-4: what the fourth woman has on her head is not a wineskin but a holdall of the sort described in CB. ii, p. 79, and represented on a Locrian plaque published by Mrs Zancani Montuoro in *Arch. Class.* 12 pl. 8. Pl. 231: I now take this to be certainly by the Chicago Painter. Pl. 232, 2: Birmingham 1618.85 is a small hydria specially close to this. I confess that I cannot find any 'sepulchral' significance either here or in other places where the editor is inclined to find it (p. 27, above, left; p. 37, foot). Pl. 232, 4: for shape and patterns, compare Berkeley 8.3373 (*CV* pl. 46, 2): I see nothing un-Attic in Munich 2437. Pl. 232, 6: compare the Mykonos hydria Dugas *Delos* xxi pl. 31, 78. Pl. 236, 7: for the shape, and for the drawing of the egg-patterns, compare Cambridge 1.02 (*CV* pl. 34, 2); the style of the figure-work is different; the Munich vase is not far from the Washing Painter.

Stamnoi. Pl. 238: true that the combatants are not certainly Achilles and Hector: but comparison with the Berlin Painter's volute-krater in the British Museum, and with a stamnos by the Providence Painter in Barcelona (García y Bellido pl. 106) makes it likely. The fragments pl. 245, 1-2 had seemed to me to be by the Aegisthus Painter (as Lullies notes), but later I saw that they belonged to a stamnos by the Copenhagen Painter in the Brooklyn Museum (*ARV*² p. 258 and addenda): the error is not disgraceful, since the Aegisthus Painter was a follower of the Copenhagen. Pl. 251, 3-4: the cakes must be *παραμίδες*, with seeds or raisins in them. The volume concludes with a new acquisition, a handsome stamnos, very like the Herakliskos vase in

the Louvre, by the Berlin Painter in his later and more conventional period.

J. D. BEAZLEY.

Corpus vasorum antiquorum. Österreich.

Wien, Kunsthistorisches Museum, 2. Rotfigurige attische Vorratsgefäße, bearbeitet von F. Eichler. Vienna: Anton Schroll. 1959. Pp. 37. Plates 51-100. Price not stated.

The first fascicule of the Kunsthistorisches Museum at Vienna contained the red-figure drinking-vessels and pyxides. The present one has half the large pots in the same technique (amphorae, stamnoi, pelikai, column-craters) with a few addenda to the first. The remaining types of crater and the hydriai are left for a third volume. There is one masterpiece, the famous pelike with the death of Aegisthus, and many works of interest or charm, including the Pan Painter's most delightful if not his grandest or most exquisite vase; and Eichler (who acknowledges the help of Rudolf Noll) has again produced an invaluable publication. Good photographs are especially welcome of the strange bilingual with Dike and Adikia (pl. 51), the neck-amphora with athletes (pl. 53), and the Aegisthus pelike with its 'ghost' (pls. 68 f.). Pl. 53: it remains strange that Klein read the inscriptions as a signature of Epiktetos, and that it has since been recognised that Epiktetos was the Kleophrades Painter's name; for this vase, listed by Beazley and Eichler as in his manner, could very well be from his own young hand. One wonders if the illegible letters are not in fact remains of a signature which could just be made out in Klein's day. Pl. 67: is it certain that the goddess lecturing Poseidon is Amphitrite? The picture could illustrate the scene between Hera and Poseidon in *Iliad* 8, 198-212. Pl. 68 f.: Splendid photographs, and account, of this splendid vase. An unusually noisy scene: four of the five figures have their mouths open. In the 'ghost' Eichler sees the hoof as cloven and interprets the figure as Artemis. In the photograph I cannot see the cleft, nor can I restore the group convincingly; in fact I am loth to relinquish my interpretation as Polyxena turning while she runs before Troilus' horses. Pl. 75, 1-2: turning to this hasty little piece from the big careful signed pelike one understands Eichler's doubt about Beazley's attribution to Hermonax; but looking closer, surely the heads are very like, especially the charming boy's on the reverse to the sphinx's on the signed vase. Pl. 92, 3: on these transvestite scenes, see also now Beazley in Caskey and Beazley ii 55 ff., the Vienna vase p. 60, no. 22. Pl. 94, 2: one would like to find an interpretation for this very curious scene. The 'cap' hanging on the wall here and in the reverse picture is surely rather sponge and strigil, implying perhaps that the building on the obverse is a palaestra. Similarly, is not the 'conical hat' in pl. 98, 2 and 4 a schematic version of a pair of halteres? (as shown, for instance, half a century earlier on the cup, Blümel, *Sport der Hellenen*, 62,

no. 88). These late column-craters are rather daunting *en masse*; but next time it will be bell-craters. Meanwhile it is a relief to reach the gay little tail-piece of the Sotadean rhyton.

MARTIN ROBERTSON.

HIGGINS (R. A.) **Greek and Roman Jewellery.**

London: Methuen. 1961. Pp. xlvii + 236. 68 plates (including 4 in colour). 33 text figures. £3 10s. 0d.

The most recent of Messrs Methuen's archaeological volumes, a study of Greek and Roman jewellery by Mr R. A. Higgins of the British Museum, is in the best tradition of this distinguished series; its own bibliography shows that it was badly needed. H. introduces his subject with five chapters describing and illustrating the techniques of the ancient jeweller, and gives a very useful summary of the materials employed and the sources from which they came. This very readable section is indispensable to anyone with more than a superficial interest in jeweller's work. We find that the evidence is against the use of the relatively common stone jeweller's moulds for casting in precious metal; they were employed, it seems, for making wax models in the *cire perdue* process, and moulding glass, lead or bronze. In discussing enamelling, possibly a Minoan or Mycenaean invention at the end of the fifteenth century B.C., H. proposes that both the well-known Curium sceptre (whose context should be dated to the eleventh, not the twelfth century) and the Kouklia cloisonné rings were made in Cyprus. They are at least as likely to have been brought to the island by refugee princely families who settled in the island with their followers throughout the twelfth century B.C.

The main part of the book consists of eleven chapters wherein successive phases of jewellers' work are described in detail. H. has interpreted his title so that it embraces the whole of the Aegean Bronze Age and Etruscan Italy as well as the periods more conventionally covered by 'Greek and Roman'. His thorough survey of Minoan and Mycenaean jewellery is particularly useful and welcome; surely the most beautiful jewel described is the Middle Minoan hornet pendant from Chrysolakkos at Mallia. The Aegina Treasure (which H. had previously taught us to see as Middle Minoan) plays a substantial role. These chapters are followed by thirty very valuable pages of combined bibliography and site-lists, repeating conveniently the chronological arrangement of the previous chapters, where reference can be found to most of the material relevant to H.'s theme. Late Cypriot jewellery could have been better represented here by the Swedish (*SCE I*) and French (*Missions en Chypre and Enkomi-Alasia*) tombs at Enkomi, rather than the badly dug and poorly documented Old Tombs of the Turner Trust excavation.

Each chapter follows a similar pattern. After a general introduction in which the relevant parts of the archaeological background are briefly but

skilfully sketched (Etruria is particularly well handled) the main characteristics of the jewellery are summarised, attention being drawn where appropriate to the appearance of new techniques and new forms. Chapter 11—'The Period of Oriental Influence'—very sensibly is further divided into five sections—Attica and neighbourhood; the Peloponnese; the Islands; Eastern Greece; the West—to take adequate account of the marked regional differences which existed during the period. The introductory matter is followed in each chapter by a description of the several categories of jewellery—'Funerary bands', 'Necklaces', 'Clothing ornaments' and the like. It is no fault of H. if this makes for dry reading; the value lies in its completeness (the list of Mycenaean relief beads, pp. 77 ff., is a case in point). It is an inevitable disadvantage of this treatment that assemblies have to appear piecemeal under different headings; one regrets that H. did not have space now and then to refurbish the dead—at least the royal dead of the Shaft Graves—in their full splendour. In the same connexion there are only the briefest references to the evidence of contemporary representations in sculpture and vase-painting.

The scale of illustration is generous; nearly 250 objects (over half of them to be seen in the British Museum) are figured in the collotype plates (of which the only bad one is fig. 40, the often-illustrated Regolini-Galassi fibula), many others in the adequately drawn text-figures. As so often with jewellery, the colour plates are a little disappointing; compare especially the difference in definition between the two appearances of the Aegina Treasure pendant, pl. B.1 (colour) and pl. 3 B (collotype).

A few very minor points may be raised. H. makes no reference to the gold and amber disk from the Temple Tomb at Knossos (surely a jewel?), with its alleged parallel in a Wessex Culture grave at Manton, Wilts. Late Helladic gold hair-spirals have also been found with burials at Kirrha (*Kirrha*, pl. 60). The very stylised bulls' heads in a Rhodian necklace (*Clara Rhodos* i 64, fig. 65) should be added to the list of Late Bronze Age relief beads. Though Late Cypriot 'diadems' certainly have nothing to do with those in the Shaft Graves, they could possibly be connected with the rare Mycenaean gold embossed strips which presumably embellished clothing. These are exemplified by Mycenae Tomb 515, no. 80a (*Arch.* 82, pl. 32). The gold lions from Mycenaean Naxos—? clothing attachments—probably came too late for inclusion. Though none of the gold arched fibulae from Cyprus has a good context, their equivalents in bronze seem to be no earlier than the middle of the eleventh century (p. 86). A Cypriot origin for the twelfth century Gypsades T. VII pins (p. 86) is unlikely; dress pins are hardly found in Cyprus after c. 1400 B.C. until their re-introduction late in the twelfth century. The Nymphaeum earrings in Oxford (p. 122 and pl. 24 G) are griffins, not cocks.

This is an important and most useful book, upon

which author and publisher are to be warmly congratulated.
H. W. CATLING.

THOMPSON (M.) **The New Style Silver Coinage of Athens. Text and Plates.** (Numismatic studies, 10.) New York: the American Numismatic Society. 1961. Pp. vii + 747. 202 (+ 2 folding) plates. \$50.

The coinage of Hellenistic Athens is one of the most extensive and complex in the whole range of Greek numismatics: many previous writers have studied the subject partially—notably there is the detailed work of Kambanis and the useful interim synthesis by Bellinger—but Miss Thompson's work gives us for the first time a really comprehensive treatment of the whole material. The main series of coins is illustrated on 149 plates, with further plates of ancient imitations and of hoard-groups: even so there was not room for every reverse die, though every obverse (showing a head of Athena reflecting Pheidias' Parthenos) is given. Two very useful fold-out plates give a grouping of obverse dies at reduced scale to elucidate the stylistic sequence of the early and the late stages where we remain largely dependent on stylistic criteria for the arrangement. The middle section is the most solidly based, and contains the greatest number of cases where an obverse die is carried over from one issue to the next: out of a total of 110 issues for the whole series, such obverse die-linkages now involve no less than 39 issues. Whatever problems remain, the rigorous organisation of this vast material represents a great and lasting achievement for Miss Thompson, and a clear gain to knowledge.

The numerous details present on the reverse side of the coins receive detailed discussion. The names of two and often three 'mint-magistrates' which appear total over 600: the numerous prosopographical problems are fully dealt with, and it is suggested that the persons in question cannot in fact be connected with any known Athenian magistracy and did not, as in the case of the Roman *tresviri monetales*, constitute a college of officials but were rather holders of a liturgy, as contributors to the cost of the issue, accordingly entitled to have their names displayed on the coins. The first two names change annually (the exception being Euboulides-Agathokles replaced by Zoilos-Euandros half-way through 142/1 B.C.), though some men seem to have served in more than one year: the third 'magistrates', however, are much more numerous, and their rotation does not seem to follow any regular system, though occasionally changing by the month. In addition there is a symbol or subsidiary device evidently chosen by the first 'magistrate' (p. 602). There is a letter denoting the month: in one case (p. 130) intercalary months are indicated for adjacent years, though some caution is necessary over this evidence, important for the question of the Athenian calendar, for the adjacent years do not have a die-link which alone would finally prove their con-

tiguity. Finally, there is a pair of letters serving some purpose in checking the coins before issue (a table of these 'control combinations' is given on pp. 614-17): it is suggested that these letters had some connexion with particular mines or particular lots of bullion, and it is noted (p. 620) that the greatest variety of such letters occurs at the time of the greatest diversity of metallic content. The latter has been established for a number of coins by the 'neutron activation' method (first described in *Archaeometry* i 1958) which involves making the coins temporarily radioactive in an atomic pile, the subsequent readings of radiations giving an accurate determination of the presence of small traces in the silver of other metals, e.g. gold and copper (tables on pp. 626-9).

The foregoing is a very bald summary of some of the many interesting pages of discussion. It is inevitable that problems remain, and controversy will probably centre on the question of the absolute chronology, and of certain prosopographical questions which are inextricably bound up with it. The 110 annual issues are here ranged neatly between 196 B.C. and 87 B.C., from the 'freedom of Greece' proclaimed by Flamininus to the conquest by Sulla: it is not easy to be sure whether it is coincidence that we have so close a fit—for any new issues subsequently discovered could not be accommodated. Admittedly, over most of the series, there are plenty of specimens per issue and new issues are unlikely to turn up, but no less than six issues of the late period are attested by a single coin, and this makes one rather apprehensive as to whether the possibility of new issues of the late period turning up can be safely ruled out. Then there is the question of the issue in the names of 'King Mithradates-Aristion'—hitherto it had been taken for granted that this was fixed to 88-87 B.C., when Aristion was holding Athens for Mithradates VI. Miss Thompson shows, however, that the relative placing of this issue must be with issues which, on her chronology, belong to a time more than thirty years earlier: she accordingly interprets the royal name as that of Mithradates V, and the issue (comprising also a little gold and a lot of bronze) as representing a benefaction by Mithradates V to Athens, in line with other known benefactions by this king to, e.g., Delos, and as having no connexion with the defence of Athens against Sulla. Opponents of this theory will find difficulty in the association of Aristion with Mithradates V, though Miss Thompson thinks that this could still be the same Aristion earlier in life, already dedicated to the pro-Pontic cause: his name, along with the 'Pontic' emblem of a drinking Pegasus, also occurs on another issue here placed as early as 129/8 B.C. The drinking Pegasus does not, however, appear on the Pontic coinage until 96 B.C., in the time of Mithradates VI, so that if the association is to stand are we to say perhaps that the emblem was originally not Pontic but that of Aristion himself, and was perhaps taken over from him later by Mithradates VI? A disquieting fact is that coin-hoards, at least *prima facie*, would appear to support

the later date (88-7 B.C.) for the Mithradates-Aristion issue: thus in the Piraeus, Dipylon and Abruzzi hoards, each of which is dated by the presence of actual Pontic coins of Mithradates VI, of 90-88 B.C., the representation of Athenian coins stops short just at the point at which the Mithradates-Aristion issue fits in.

However, there are other important factors which seem after all to support Miss Thompson's chronology. Foremost, a coin of the Demeas-Kallikratides issue (dated by Miss Thompson to 107/8 B.C.) which certainly seems to belong to a later stage in the series than that of Mithradates-Aristion, was re-struck by Aesillas, a Roman official in Macedonia (pl. 138, no. 1233 b): Aesillas is known only from his coins, which have been dated 93-88 B.C., but one of them is die-linked to an issue of Sura, who has been very cogently identified with L. Brettius Sura, legate of C. Sentius in 88-7 B.C. Thus the Demeas-Kallikratides issue at Athens must be before 93-88 B.C. Consequently, to get the Mithradates-Aristion issue back to 88/7 we should have to adopt one of the following alternatives: (a) to alter the sequence of the later Athenian series pretty drastically, which there seems to be little chance of doing: the later and rather sparse issues with which Demeas-Kallikratides clearly belongs cannot, so far as I can see, possibly be re-positioned earlier in the series than the group containing Mithradates-Aristion. (b) To interpret the overstriking of the Demeas coin in the opposite sense, viz. that the Athenian types were struck over the Macedonian: this too appears utterly improbable. (c) To shift Aesillas and Sura far enough to bring down the dates of the whole Athenian series—though this seems unlikely prosopographically, and indeed a newly-reported hoard from Macedonia (Varoucha in *BCH* 1960, 494) contained Roman coins down to 76 B.C. and included an Aesillas piece, which tends to show that the accepted dating of the latter must be substantially correct.

Miss Thompson's dating also fits in better with the large issue of Athenian type convincingly attributed to Sulla, on which two monograms are cogently interpreted as 'Markou Tamiou' (p. 434), viz. M. Lucullus, Sulla's quaestor: these (pls. 143-9) are very close in style to another, certainly Sullan, issue (pl. 149: with two trophies as on Sulla's Roman gold pieces), but so different from the normal Athenian series that it is virtually impossible to imagine the latter continuing after them. Moreover, the 'monograms' issue has corresponding bronze coins of Athenian type, which seems to confirm that the Sullan issue was indeed minted at Athens.

The new chronology, then, though at first sight disturbing, cannot easily be faulted: it remains only to be seen whether fresh evidence, especially in the realm of the bronze coinages (discussed briefly on pp. 525 ff.), can eventually contribute further elucidation.

G. K. JENKINS.

Sylloge nummorum Graecorum. [U.S.A.]
The Burton Y. Berry Collection. Part 1,
Macedonia to Attica. New York: the
 American Numismatic Society. 1961. Pp. 33.
 28 plates. \$15.

The publication of the present volume marks an auspicious occasion in that it is the first contribution made to the *Sylloge* series by the American Numismatic Society. The Burton Y. Berry collection, assembled over a period of twenty-five years, contains some 1500 coins from a limited area of the Greek world. Part I (1-746) records issues from Macedonia to Attica and a further part (Megara-Egypt) will complete the catalogue. Coins of Elis, Athens, Aspendus, the Chalcidian league and regnal issues of Macedonia, Thrace and Syria are well represented. Individual specimens are noteworthy for their fine quality.

The commentary, a model of conciseness, is the work of Miss Margaret Thompson, assisted by Dr Ross Holloway. The text follows the general plan of the original *Sylloge* volumes in giving only the minimum of essential information (see *JHS* lxxxi, 1961, p. 229). References, therefore, are restricted to definitive publications; E. J. P. Raven, *Numismatic Chronicle* 1950, i-ii 1-22 'The Amphictionic Coinage of Delphi 336-334 B.C.' might have been added to the text of 584-5. Miss Thompson wisely, in a work of this nature, avoids any elaborate discussion of major problems of attribution; these are clearly the province of the writer of the specialised study, or monograph.

The collection includes a fine series of gold staters of Philip II (pl. 4, 80-97) and pls. 6-7 are exclusively devoted to gold issues. A tetradrachm from the Chalcidian league (25) with a new reverse type but shared obverse die (Robinson and Clement, *Excavations at Olynthus* IX, no. 136) shows the potential importance of single issues in die study; it makes necessary some readjustment of the sequence of magistrates (Archidamus-Ariston). An unpublished stater (54) is perhaps of Thraco-Macedonian mint. There are also new dies of Alexander I (67) and of Perdiccas II (69) not recorded in Raymond, *Macedonian Regnal Coinage to 413 B.C.* The long series of Athenian coins includes many of the New Style arranged according to the author's revised chronology in *The New Style Silver Coinage of Athens* (*ANS Numismatic Studies* X). The dating of these issues to the period 196/5 - 88/7 B.C. has been questioned, but discussion of the inherent problems may more appropriately be deferred to the review of the monograph itself. The catalogue also records a rare decadrachm (295)—only two other genuine examples are known—the obverse type of which is described as Alexander, or Taxiles, on horseback attacking Porus? (see Head, *Historia Nummorum*², p. 833 and Walker, *BM Guide to the Principal Coins of the Greeks*², pl. 27, 4: the present coin is apparently from the same dies). Other rarities include a second-century tetradrachm of Eretria (625) and a tetradrachm with reverse facing owl (701)—cf. *BMC Palestine*, pl. xlii I.

Finally there is an Athenian decadrachm (641) in relatively poor condition.

The plates are mostly of high quality and of particular interest because the coins have been directly photographed. Not only does the definition of detail bear favourable comparison with that present in illustrations from casts, but the increase in character is welcome (cf. pl. 5).

The American Numismatic Society, to whom the collection has been given, and Miss Thompson both deserve congratulation on the publication of what it is hoped may be the first of a flourishing series of *Sylloge* volumes.

J. F. HEALY.

BRUUN (P.) *Studies in Constantinian Chronology*. (American Numismatic Society, numismatic notes and monographs, 146.) New York: the American Numismatic Society. 1961. Pp. xi + 116. 8 plates. \$5.

This monograph is based on a survey of the gold coinage of Constantine of the years 306-337. It demonstrates beyond any possible doubt that the issue of gold was normally concentrated at one mint, following the itinerary of the Emperor ('the mints remained stationary whereas the organisers or controllers were mobile'). Bruun presents as his main results, however, certain revisions in the chronology of the period. The most striking of them are that Constantine's Italian campaign was in the Spring of 311, not in 312, and that the *Bellum Cibalense* was in 316, not 314. Bruun's numismatic conclusions are at odds with those of Carson and Kent, and Kent has recorded a protest against them in reviewing this book (*Numismatic Circular* lxx [1962], 111). In presenting his views on chronology without hedging them in any way, Bruun is indicating a *pons*, and I imagine that his earnest wish would be for each reader to decide whether or not he is right. But the problems are probably more open to discussion than he is willing to concede, and his condensed style and reference, for certain critical parts of his argument, to other publications thus become a considerable hindrance to all but the handful of scholars who are competent, through a close acquaintance with the numismatic evidence, to take part in the debate. Those few will find his logic admirably tight and cautious, and will be able to use the book easily. Others may think it a shade perverse of the author of a separate publication of this length not to have made it more self-contained. Even if the final judgment should turn out to be that Bruun's conclusions cannot be as fully supported as he now claims, his book would still contain a great deal of value, and much of his detailed argument would be the necessary basis for further work. It would all need to be reconsidered, and an alternative explanation put forward that was capable of absorbing the evidence of all the coins, if the main conclusions were to be challenged. What he has built, therefore, will not be pulled down in a day. The framework of the numismatic chronology is given by consular and

anniversary issues, and the rest of the coins are fitted into that framework on various grounds such as style and type, and by reference to the course of historical events. The strength of this refined and formidable numismatic method is that it takes into account a very large number of fragments of independent and indisputable evidence. It is indisputable, that is to say, that the coins are evidence and that they must somehow be taken into account; but the evidence is not always clear, nor is it always tidy. Thus, for example, we are advised of 'the frequent "anachronisms" such as the commemoration of the second consulships of Crispus and Constantine II in the year the Caesars actually held their third consulates'. There is a clear appreciation, too, of the part of expediency in the monetary affairs of a world at war, in glimpses such as this: 'Constantine's expeditionary force had been equipped far in advance with coin models in order that coining could start as soon as the mint was captured. On the other hand, the civilian administration followed slowly in the tracks of the Emperor, and its arrival at Nicomedia changed the types brought forth by the army into new up-to-date ones'. It is at this point, and points like it, that one must test the weakest links of the argument, namely the interpretations placed on coin-types. How nicely were the allusions calculated at each particular stage of affairs? How much were out-of-date dies used? How far can the chronology be established independently of the interpretation of the coin-types? Thus, for example, on 'the exact mechanics of gold minting during the first post-war years': [p. 61] 'I believe that two different groups can be distinguished representing successive stages of coining, the first comprising slightly anachronistic types created several years earlier, the second comprising comparatively recent inventions.' When the types are as oblique in their reference as (among the first group) *Vota publica*, it might be obvious to someone immersed in them that they are anachronistic, but to anyone else it must seem that there is an element of intuition in the analysis, and that the conclusion (about Constantine's expeditionary force, quoted above) cannot, in the nature of the evidence, be cut and dried: on what grounds *successive*; and in the circumstances what meaning can be attached to *slightly* anachronistic?

Those who argue right up to the edge of probability have their own reward in the sense of exhilaration that their work brings; but the only thing that really matters is whether a view is right or wrong. In Constantinian chronology it seems to me too soon to say whether Bruun is right or wrong. His numismatic grasp merits profound respect; anyone who, if offered a Constantinian gold coin for five pounds, would not be equipped to decide whether it was authentic might reflect on the extent to which he is outclassed.

D. M. METCALF.

DODD (E. C.) *Byzantine Silver Stamps: with an excursus on the Comes sacrarum largi-*

tionum by J. P. C. Kent. (Dumbarton Oaks Studies, 7.) Washington: the Dumbarton Oaks Research Library and Collection. 1961. Pp. xix + 283. 1 map. 5 tables. 103 plates. \$15.

This is a corpus of Byzantine hallmarks on silver, mainly of the sixth and seventh centuries, and mainly church plate. No such marks have been found on gold or silver objects of a date earlier than the fourth and fifth centuries of our era, and the earliest examples are on gold and silver ingots rather than on shaped vessels. The early marks on plate represent an official guarantee of the quality of the metal, the purpose of which was to aid the evaluation of the objects for tax payments. Anastasius, who was responsible for a major reform of the coinage (A.D. 498), similarly set up a new system of control of silver, in accordance with which stamps, usually four in number, were applied to silver vessels at some point during their manufacture, after the ultimate use of the silver had been determined and the vessel shaped, but before the decoration was finally completed. Justinian elaborated on the Anastasian reform in the control of silver plate (as he did also with the coinage); the usual number of stamps was increased to five. His system survived with very few changes until the middle of the seventh century. Each of the four or five stamps on an object affords, by its shape, monogram, inscription, and so on, a means of dating the object. When the evidence of all of them is combined, a fairly consistent chronological sequence emerges of the silver stamps used from the time of Anastasius to Heraclius. By no means all Byzantine plate, however, was hallmarked. The use of the main ('imperial') series of marks was very probably restricted to Constantinople. There is literary evidence to support this conclusion. Other marks, among which some can be associated with Carthage and Antioch, are relatively few in number. Beyond doubt the main series was used on semi-finished vessels sold by the government department of the *sacrae largitiones*, for the names of the *comites sacrarum largitionum* can be read in the monograms of the marks. It does not follow necessarily that the objects stamped in Constantinople were also decorated there; some of them are thought to be of provincial workmanship. The decline of the system is part of the general decline in central administrative practice in the seventh century, and, like most declines, is difficult to trace out exactly. The dwindling importance of the office of the *sacrae largitiones* is discussed in an able chapter by Dr Kent. Dr Dodd's attempts to connect the end of the system of hallmarking with Heraclius' levy on ecclesiastical treasures and with the striking of the hexagram are perhaps less rigorous than the rest of her very careful argument. The exact date of the important Lesbos treasure of 1951 remains to be taken into account. Silver treasures of the late seventh and eighth centuries are so scarce that few conclusions ought to be drawn.

The intelligent and thorough observation of anti-

quities, and the comparative methods and cautious argument of this monograph, are of the highest standard. The firm association of the hallmarks with the office of the *sacrae largitiones* is a most gratifying result; for the rest, the conclusions are unspectacular but solid. Evidence that one of the imperial monograms belongs to Justin II, not Justin I, is of wider value and interest than just for the immediate topic.

D. M. METCALF.

BROWN (B. R.) **Ptolemaic Paintings and Mosaics and the Alexandrian Style.** (Monographs on Archaeology and Fine Arts, VIII.) Cambridge, Mass.: Archaeological Institute of America. 1957. Pp. xvii + 108. 45 plates. Price not stated.

In this extremely useful book Mrs Brown gathers all or nearly all the paintings and mosaics that have been actually found in Alexandria or the vicinity. She works out a chronology for them, considers what they tell us about Alexandrian painting, and compares this with the literary record and with what has been made of that record by modern scholars. The paintings are without exception from tombs: wall- and ceiling-paintings, *loculus* slabs, stelai and grave-vases. The few mosaics are from dwelling-houses, and the best of them reach a higher level of quality than the paintings, which are poor works, mostly in poor condition. The only monuments to which absolute dates can be assigned are twenty-six of the polychrome Hadra vases which bear inscriptions giving the date of burial by the year of a king's reign. The king is in no case named, but for some it is possible to establish a high probability. Of only five of these is the find-spot known: the Soldiers' Tomb, from which come also a large number of painted *loculus* slabs; and Mrs Brown begins her study from this monument. The dated vases from this tomb are placed from 250 to 239 B.C., but Mrs Brown's study of the other material makes it clear that the tomb was in use over a considerably longer period. She distinguishes four Styles in the painted *loculus* slabs and stelai, three of which are represented in this tomb and the fourth adumbrated. She shows that the first stems directly from Attic funerary art of the fourth century, and must belong to the early years of the city of Alexandria. The second she associates with the post-Praxitelean school of the early third century. The paintings of the third Style she compares to the first Pergamene school of sculpture in the later third century, and thinks that they lead on to the second century paintings in Delos. The fourth Style is a crude popular one which develops during the period of the third and continues after it. Mrs Brown's definition of the first Style and its Attic connexions is entirely convincing. With the others a greater degree of subjectivity seems to enter into the distinction of style among these sorry objects; but by and large it probably works. To this scheme Mrs Brown links the other paintings discussed, of which the most

important (the best piece of painting we have from Alexandria, though a daub at that) is the picture with horsemen and standing women from the Mustapha Pasha Tomb. She gives reasons for dating it rather early in the third century; and the kind of preoccupations shown in the boldly foreshortened horses seen from a low level confirm this, I think, by relating it to the Alexander mosaic. The finest of the mosaics here, from Thmuis, is convincingly compared to, and dated slightly earlier than, second-century works from Pergamon. Three mosaics in a far more classical style are argued to be classicising works of the first century B.C. or A.D. For the tessellated mosaic 50 this is supported by the find-spot, since it comes from a house in the Shabti district, where throughout the Hellenistic age was a cemetery, later built over. The style is certainly derived from that of pebble mosaics of the fourth to third centuries, a fact now confirmed by the Pella finds, one of which is particularly close to it in composition. No. 51 is a pebble mosaic, and would, if Mrs Brown is right, be a throw-back in technique as well as style. Some of the features—compact laying, lead thread for outlines—which she regards as borrowings from the developed tessera-technique, are now found in the Pella pebble-mosaics (the thread there is lead, not, as stated in my *Greek Painting*, bronze). The Pella mosaics are not certainly dated, but I cannot doubt that they are early and show the pebble-technique developing towards the ideals attained in tesserae. I should be inclined to suppose that no. 51 is of the same period; and do not feel quite sure (in spite of the provenance) that no. 50 is not a very early tessera mosaic deriving directly from the pebble tradition. In her last chapter Mrs Brown discusses the literary tradition and the question of Alexandrian art. She shows convincingly that nothing in the surviving monuments lends support to the suggestions that have been put forward about Alexandria as the home of impressionism, Nile-landscape and caricature; and that there is hardly more basis for these conjectures in the literary sources if they are strictly examined. She concludes that all these things were common to all Hellenistic art, in whose various movements Alexandria will naturally have shared. One small point which does not affect her main argument: she questions (following Dawson) whether the Demetrius whom in two places Diodorus mentions as having been generous host in Rome to the fugitive Ptolemy VI Philometor, describing him once as son of Seleucus and once as *τοπογράφος*, was necessarily either an Alexandrian or a landscape painter, but rather perhaps a map-maker or topographer. Valerius Maximus, however, who tells the same story (v. 1.1; Overbeck 2141) though he does not name the beggar-king's benefactor, describes him as *Alexandrinus pictor*.

MARTIN ROBERTSON.

tektur, 10.) Berlin: W. de Gruyter. 1962. Pp. xii + 42. 53 plates. 4 text figures. DM 68.

This book gives an admirably clear and concise account of one of the most difficult Hellenistic temples. It suffers somewhat from the circumstances of its composition: Schleif, who was responsible for the supplementary investigations of the 1930's, died in 1945, his section on the architecture being written separately, more than twenty-five years ago: Goethert, who took part in those investigations did not assume the responsibility for the publication of the sculpture until after the death of Rodenwaldt, also in 1945.

Thanks partly to the character of its foundations (sand in deep trenches rather than solid masonry) Schliemann had no difficulty in completing the destruction of the temple. The surviving marble fragments, easily recognisable, though scattered, are few in number. Our knowledge of the details is therefore limited, and restoration, even of the basic ground plan, must remain conjectural. Schleif's restoration is reasonable, while text and illustrations make clear enough what is fact and what conjecture.

The most important problem concerns the date of the temple. The controversy goes back almost to the original excavation, Schliemann considering that the temple was built by Lysimachus, Dörpfeld preferring a date in the Augustan period. In recent years the attribution to Lysimachus seems to have prevailed. Schleif gives no indication of his ideas on the matter, beyond pointing out that differences in workmanship do not necessarily imply a construction period extending over more than two decades. His restoration of the plan, reasonable enough in view of the proportions of the temple (6 × 12 columns, cella with opisthodomus) suggests a relatively early date. Goethert, however, comes out strongly in favour of an Augustan date, his evidence being cogently marshalled in the third chapter. All the same, there are difficulties. If the building is Augustan, it presumably derives from Pergamene traditions (the style of the famous Helios metope, Goethert suggests, bears this out, as well as the beamless coffer ceiling, comparable with that of the great altar at Pergamum). Yet the technique of the temple, as well as many of the details seem to belong to early rather than late Pergamene art, and are remarkably pure, if they are to be attributed solely to Augustan classicising tendencies. For instance, the profile of the capitals is relatively straightforward, and similar to the profile used in the Pergamene temple of Athena Polias; particularly it does not have any of the extraneous mouldings which disfigure many late Pergamene capitals, nor the additional band under the annuli, which occurs in late Hellenistic Doric, and, more important, is retained even in the purer Roman capitals of the first century B.C. (such as those of the propylon to the Roman agora at Athens). The capitals also have the single square empolion hole usual in Greek buildings down to the third century B.C. This again contrasts with

the late Pergamene technique of a square empolion set on edge between two long rectangular dowel holes. Nor am I satisfied by the late date given to the sima: its decoration has some similarity with painted ornament in certain third century B.C. Macedonian tombs while its profile (in spite of Miss Shoe) can be traced back at least to second century B.C. Pergamum.

For these reasons, and because of the probable arrangements of the plan, I am not convinced by the evidence for an Augustan date. These arguments must not be allowed to detract from the virtues of this book. That they can be made is due solely to the admirable presentation of the known facts. Failing a full study of Hellenistic architectural practice, the conclusions to be drawn from the facts must remain rather subjective.

R. A. TOMLINSON.

SMITH (A. C.) **The Architecture of Chios: Subsidiary Buildings, Implements and Crafts.**

Ed. P. P. Argenti. London: A. Tiranti. 1962.

Pp. viii + 171. 7 maps. 226 plates. £4 10s. 0d.

The mediaeval and later history of Chios is at least as absorbing as its ancient history and, thanks to Dr Argenti, far better documented. Its long prosperity under the Genoese trading company left its mark on the architecture of the island, and subsequent Turkish rule and massacres have not wholly effaced it. Moreover, its humbler village architecture shows a greater variety than that of any other Aegean island. Before the last war Arnold Smith

spent four years studying and drawing Chian architecture of the fourteenth century and later. After his death Mr A. Walton prepared further drawings for the book and Dr Argenti completed the text, adding an introduction and a section on travellers' accounts. All types of religious and domestic architecture are described with especial attention to the Genoese houses in the Chian Kampos. Other buildings of particular note are the fortresses and the mediaeval villages whose tortuous plans around a great central refuge tower can still readily be made out. It is the mixture of Italy and the Aegean which makes these buildings of such interest, and the book does a great service to the island and to scholarship by recording evidence which might all too easily disappear.

The archaeologist will find much familiar in the ground plans of the simpler houses, and may well be surprised and chastened to discover the variety of construction possible in the same materials which were in use before Romans or even Genoese came to the island. And he need look no further than pl. 157 for Odysseus' bed on Homer's own island (as Miss Lorimer had observed, *Homer and the Monuments* 274). The drawings are clear and well printed. Many are much reduced. They do not suffer, and there must be over a thousand individual drawings in the many plates. Some may regret the absence of photographs since only from them can the quality of the masonry and wood-carving be properly judged.

JOHN BOARDMAN.

SHORT NOTICES

Acta classica: Proceedings of the Classical Association of South Africa. Vol. 4. 1961. Cape Town: A. A. Balkema. 1962. Pp. 131. 3 plates. Subscription £1 5s. 0d.

These *Proceedings* include the following papers in the field of Hellenic studies: E. L. de Kock, 'The Sophoklean Oidipus and its Antecedents', mainly an account of the evidence for the story of Oedipus in the Epic Cycle; P. J. Conradie, 'Die Mite van Oedipoes by Sophokles en Jean Cocteau' (in Afrikaans with a summary in English); P. J. Bicknell, 'The Fourth Paradox of Zeno', offers a new interpretation of Aristotle *Physics* 239 B 33-240 A 18; N. C. Conomis, 'Varia Graeca', discusses inscriptions relating to the orator Lycurgus and to the biographical tradition on Pollux, *P. Oxy.* 2331, passages from Bekker, 'Anecdota' and some miscellaneous glosses. The *Proceedings* as a whole give a very encouraging impression of classical research in South Africa.

Innsbruck. Serta philologica Aenipontana. Ed. R. Muth. (Innsbrucker Beiträge zur Kulturwissenschaft, 7/8.) Innsbruck: Institut für vergleichende Sprachwissenschaft. 1962. Pp. viii + 456. Price not stated.

This volume includes the following articles in the field of Hellenic studies: L. R. Palmer, 'Linear B Texts of Economic Interest'; Fritz Gschnitzer, 'Zum Namen Poseidon'; Albin Lesky, 'Zur Eingangsszene der Patroklië'; Johannes Theoph. Kakridis, 'Helena und Odysseus'; Franz Hampl, 'Die Ilias ist kein Geschichtsbuch'; Karl Völkl, 'Zur Lage des homerischen Ithaka'; Hans Schwabl, 'Beobachtungen zur Poesie des Hesiod'; T. B. L. Webster, 'Some Notes on the New Epicharmus'; Hans Diller, 'Erwartung, Enttäuschung und Erfüllung in der griechischen Tragödie'; Franz Stoessl, 'Zur Iris-Lyssa-Szene in Euripides' Herakles'; Werner Nagel, 'Zur Darstellungskunst Platons, insbesondere im Dialog "Laches"'; Karl Vretska, 'Zu Form und Aufbau von Platons Symposium'; Egon Braun, 'Psychologisches in den Politika des Aristoteles'; Walther Kraus, 'Menanders Dyskolos und das Original der Aulularia'; Peter Henkel, 'Zu Theokrit, Id. II, vv. 38-42 (28-32)'; Wilhelm Krause, 'Probleme der antiken Grammatik'; Ioannes Gerstinger, 'Ad Cononis Diegeseon fragmentum 1'; Gerhard Reiter, 'Zu den griechischen Farbnamenkomposita'.

Proceedings of the IX International Congress of Papyrology, Oslo, 19th-22nd August, 1958. Oslo: Norwegian Universities Press. 1961. Pp. xvi + 392. 6 plates. Price not stated.

This volume includes the following articles in the field of Hellenic studies: Eric G. Turner, 'Euripidean

Hypotheses in a New Papyrus'; Wiktor Steffen, 'Die neuen Iambengedichte des Archilochos. Zum Papyrus von Oxyrhynchos 2310, Fr. 1, Kol. 1'; J. A. Davison, 'Notes on Alcman'; Augusto Traversa, 'I papiri epici nell'ultimo trentennio'; Manuel F. Galiano, 'Les papyrus d'Eschyle'; J. W. B. Barns, 'The Placing of Papyrus Fragments'; Aristide et Rita Calderini, 'De papyris ad historiarum scriptores pertinentibus nuper repertis quid ad studia profecerint'; F. M. Heichelheim, 'A Chronological Table of Hellenistic History'; Georges Daux, 'Note sur l'intérêt historique des affranchissements de Delphes'; Orsolina Montevicchi, 'Quaedam de graecitate Psalmorum cum papyris comparata'; Anton Paap, 'Die griechischen Nomina sacra'.

The Greeks. Ed. H. Lloyd-Jones. London: A. C. Watts and Co. 1962. Pp. xii + 262. 6 plates. 2 maps. 15s.

It is well that the talks broadcast in 1961 in the series 'The Greeks' have now been published in revised and expanded form. M. I. Finley's talk 'The Growth of the Polis' was not available, for reasons explained in the Introduction, and is replaced by a chapter on the same theme by A. Andrewes, and a new chapter on 'Greek Mathematics and Astronomy' has been contributed by George Huxley. The other chapters are 'The Homeric World' by Denys Page; 'Athens and Sparta' by A. H. M. Jones; 'Greek Literature after Homer' by K. J. Dover; 'Greek Tragedy' by H. Lloyd-Jones, who also edited the series and wrote the Introduction; 'Greek Science' by G. S. Kirk; 'The Greeks and their Philosophy' by A. H. Armstrong; 'The Visual Arts of the Greeks' by Martin Robertson (now expanded to a chapter of seventy pages); 'The Hellenistic World' by E. Badian.

JACQUOT (J.) Ed. **Le théâtre tragique.** (Angers, Juin 1959; Royaumont, Mai et Décembre 1960.) Paris: Centre National de la Recherche Scientifique. 1962. Pp. 539. NFr. 46.

This volume contains thirty-eight papers on tragedy ancient and modern, read to groups of scholars from many European countries at several meetings held at Angers and Royaumont.

The whole volume will be of interest to students of Greek or any other tragedy, but papers directly concerned with Greek studies are: 'Origines', by F. Robert; 'Eschyle, Ombres sacrées', by J. de Romilly; 'Sophocle, Individu et Norme', by J. C. Kamerbeek; 'Euripide, La religion traditionnelle', by J. Duchemin and 'L'actualité de son temps', by E. Delebecque; 'Exigences du public et ressorts de la tragédie', by F. Robert; 'Déclin de la tragédie à Athènes et en Angleterre', by H. D. F. Kitto. There

is wide divergence in the treatment of Greek tragedy in these papers, and in general they are in accordance with the lines of approach adopted by the authors in other publications. The last three papers, especially 'La Tragédie et l'espoir' by J. Jacquot, the editor of the book, are a valiant attempt at synthesis of discussions that ranged over the whole field of European drama.

P. T. S.

BRINDESI (F.) **La famiglia attica: il matrimonio e l'adozione.** (Bibl. di cultura, 66.) Florence: La Nuova Italia. 1961. Pp. 91. Lire 500.

This appears to be a section of a larger work dealing with legal aspects of the family in Athens. The discussion of original sources and of leading authorities is strongly reminiscent of a thesis, and the mode of treatment is calculated to repel anyone but a devoted student of Athenian law. The first portion is devoted to certain aspects of marriage and the terms *ἐγγύησις* and *συνποιεῖν*. The larger second portion is concerned with adoption, its forms, formalities and conditions. It includes a useful discussion of the 'Solonian Law' on adoption, of wider general interest than the rest of the pamphlet.

R. J. H.

WYCHERLEY (R. E.) **How the Greeks Built Cities.** 2nd ed. London: Macmillan. 1962. Pp. xxi + 235. 16 plates. 52 text figures. £1 5s. 0d.

The new edition contains a few revisions and additions but is basically unchanged.

KAULEN (G.) **Die Stilphasen der geometrischen Kunst in Keramik und Plastik, von Mykene zum 7. Jahrhundert.** Cologne: the Author. 1962. Pp. 32. 4 plates. Price not stated.

A brief preliminary survey of a detailed study of geometric style in pottery decoration and minor sculpture. All phases from Bronze Age, through Submycenaean, Protogeometric, Early, Ripe and Late Geometric, play their part in a hypothetical development of plastic form which imposes dates where no other chronological evidence is available for the clay and bronze figures involved. The shapes and decoration of clay vases are found to develop in an analogous manner.

New York. Metropolitan Museum of Art. **Ancient Art from New York Private Collections: Catalogue of an Exhibition, December 1959-February 1960.** By D. von Bothmer. New York: the Metropolitan Museum of Art. 1961. Pp. x + 85. 104 plates. \$5.

319 objects, mainly Greek vases, but with some sculpture and prehistory, and a little from Egypt and the Near East. Most have been published before but there is here full bibliography and discussion as well as many illustrations.

HAFNER (G.) **Geschichte der griechischen Kunst.** Zurich: Atlantis-Verlag. 1961. Pp. 527. 523 illustrations. Sw.fr. 76.

A full and informative text, with no footnotes, excellent pictures and a good index.

Hamburg. Museum für Kunst und Gewerbe. **Kunst des Altertums in Hamburg.** By H. Hoffmann. Mainz: P. von Zabern. 1961. Pp. ix + 46. 99 plates (including 4 in colour). Price not stated.

Excellent photographs of the collection in the Hamburg Museum für Kunst und Gewerbe, although little not published elsewhere. There is a brief commentary, full bibliography and short history of the collection.

POULSEN (V.) **Griechische Bildwerke. (Die blauen Bücher.)** Königstein: K. R. Lange-wiesche Nachfolger H. Köster. 1962. Pp. 112, including 98 plates. DM 6.60.

A picture book of good photographs of well-known pieces, with a brief introductory essay.

Boston. Museum of Fine Arts. **Greek Gods and Heroes.** 5th ed. Boston: Museum of Fine Arts. 1962. Pp. 105. 89 illustrations. \$1.75.

The new edition, in the care of Hazel Palmer, includes a number of additions and changes in the text.

BOARDMAN (J.) and POPE (M.) **Greek Vases in Cape Town.** Cape Town. 1961. 20 pp. 16 plates.

Publication of 32 vases in the South African Museum, of which 20 are illustrated. Among the 20 Attic vases, the most noteworthy are no. 1, a mid-sixth century black-figure amphora, belonging to the Group of Leningrad 1469; no. 14, a fine red-figure stamnos by the Chicago Painter; no. 18, a mid fifth-century red-figured lekythos, portraying a girl dancing the Pyrrhic, and inscribed *ΖΕΦΥΡΙΑ ΚΑΛΕ*. The remainder are Myc. III B, Late Cypriot, Ripe Corinthian, Etruscan bucchero, and South Italian red-figure.

Greifswald. Greifswalder Antiken, von A. Hundt und K. Peters. Ed. E. Boehringer. (Gedächtnisgabe für Erich Pernice.) Berlin: W. de Gruyter. 1961. Pp. xvi + 148. 72 plates. DM 118.

This volume commemorates Pernice and gives a bibliography of his works. The Greek and Roman antiquities in the University collection are published very fully (they are for the moment housed in Göttingen). Most are Greek vases, largely fragments, with some prehistoric pottery, and bronzes, terracottas, coins, etc. A special section (by Boehringer) is devoted to a study of replicas of the Hermes Farnese type. Descriptions are full and accurate. No. 120—perhaps the forefront of an

animal (inverted on pl. 11) with cloven hoof drawn in outline as on Chian vases; no. 130—Attic Swan Group; no. 196—not a fight; no. 376—closest is *Greek Vases in Cape Town* (Boardman and Pope) pl. 16.30; no. 457—cf. Brown, *The Etruscan Lion* 132, pl. 47c, d.

J. B.

WELLESZ (E.) **A History of Byzantine Music and Hymnography.** 2nd ed., revised and enlarged. Oxford: Clarendon Press. 1961. Pp. xiv + 461.

Byzantine music being a recondite and highly technical subject, it was a great service when, in 1949, Professor Wellesz rendered it intelligible to the general reader in its historical and liturgical contexts. It is not surprising that a second edition has been demanded; and he has taken the opportunity to make a fairly extensive revision, expanding certain sections and adding a section on a field that has only

been exploited since the first edition appeared—melismatic chant and psalmody. A number of excursuses are printed as an appendix, and the book has been re-indexed. A revised Introduction sets out 'the present state of studies in Byzantine chant'. Many new lines of research are being explored and many scholars are now at work, but all progress stems from the labours of a triumvirate. Tillyard published an important article in 1911, Wellesz a series of articles from 1918 onwards; subsequently they were joined by the late Carsten Høeg. The three men conferred at Copenhagen in 1931, agreed on rules for the transcription of Byzantine melodies and planned the series of *Monumenta Musicae Byzantinae*, which is now famous. The transcription and publication of documents in adequate numbers was the essential thing: that there should be documents to publish fills the humble student of ancient Greek music with envy.

R. P. W.-I.

BOOKS RECEIVED

- Abinnaeus papyri.** *The Abinnaeus archive: papers of a Roman officer in the reign of Constantius II.* Ed. H. I. Bell, V. Martin, E. G. Turner and D. van Berchem. Oxford: the Clarendon Press. 1962. Pp. xiv + 191. £3 3s.
- ACHILLES TATIUS.** *Leucippe and Clitophon.* Comm. by E. Vilborg. (Studia Graeca et Latina Gothoburgensia, xv.) Göteborg: Institute of Classical Studies. 1962. Pp. 140. Sw.kr. 20 (unbound); 25 (bound).
- ADCOCK (F. E.).** *The Greek and Macedonian art of war.* Berkeley and Los Angeles: University of California Press (London: Cambridge U.P.). 1962. Pp. vii + 109. 8s. 6d. (Paperback repr. of 1957 ed.).
- AGNELLO (G.).** *Le arti figurative nella Sicilia bizantina.* (Istituto Siciliano di studi bizantini e neoellenici. monumenti, 1.) Palermo: Ist. Sic. di studi bizantini e neoellenici. 1962. Pp. 375. 288 illus. Price not stated.
- ALLAN (D. J.).** *Aristote le philosophe.* Trans. C. Lefèvre. Louvain: Nauwelaerts. 1962. Pp. viii + 248. 1 map. Fr.b. 120.
- ARDIZZONI (A.).** *Intorno a due definizioni antiche di ποιητα e ποιησις.* [Riv. di filologia e di istruzione classica, 90.] Rome: the Author. 1962. Pp. 13. Price not stated.
- ARISTOTLE.** *Vita Aristotelis Marciana.* Ed. O. Gigon. (Kleine Texte, 181.) Berlin: W. de Gruyter. 1962. Pp. iii + 79. DM. 12.
- ASTERIUS.** *Index Asterianus.* By E. Skard. (Symbolae Osloenses, fasc. supplet. xvii.) Oslo: Norwegian Universities Press (for Oslo U.P.). 1962. Pp. 159. N.kr. 30.
- BARR (S.).** *The will of Zeus.* [English edition.] London: Weidenfeld and Nicolson. 1962. £2 10s.
- BROCCIA (G.).** *Struttura e spirito del libro vi dell'Iliade.* 1. Dalle androctasie del preludio alla homilia. (Biblioteca di cultura superiore, sez. i, 1.) Sapri: the Author. 1962. Pp. 121. L. 1,800.
- DEROY (L.).** *Initiation à l'épigraphie mycénienne.* (Incunabula Graeca, 2.) Rome: Edizioni dell'Ateneo. 1962. Pp. 131. Numerous text figures. L. 2,500.
- DETIENNE (M.).** *Homère, Hésiode et Pythagore: poésie et philosophie dans le pythagorisme ancien.* (Collection Latomus, lvii.) Brussels: Latomus. 1962. Pp. 113. Fr.b. 175.
- DODD (C. H.).** *Wilfred Lawrence Knox, 1886-1950.* [Proc. British Academy, xlvii.] London: Oxford University Press for the British Academy. 1963. Pp. 263-72. 2s. 6d.
- DÖLGER (F.).** *Εὐχαριστήριον Franz Dölger zum 70. Geburtstage von ehemaligen griechischen Schülern gewidmet.* Munich: C. H. Beck. 1961. Pp. vi + 118. DM. 18.
- FRASER (P. M.).** *Some Alexandrian forgeries.* [Proc. British Academy, xlvii.] London: Oxford University Press for the British Academy. 1963. Pp. 243-50. Plates xlii-xlvi. 4s.
- GAGÉ (J.).** *Matronalia: essai sur les dévotions et les organisations culturelles des femmes dans l'ancienne Rome.* (Collection Latomus, ix.) Brussels: Latomus. 1963. Pp. 289. Fr.b. 425.
- GALLAVOTTI (C.) and SACCONI (A.).** *Inscriptiones Pyliae ad Mycenaeam aetatem pertinentes.* (Incunabula Graeca, 1.) Rome: Edizioni dell'Ateneo. 1961. Pp. xvi + 204. L. 2,500.
- GIULIANO (A.).** *Il commercio dei sarcofagi attici.* (Studia archaeologica, 4.) Rome: 'L'Erma' di Bretschneider. 1962. Pp. 89. 1 map. L. 4,000.
- HAFNER (G.).** *Ein Apollon-Kopf in Frankfurt und die Niobiden-Gruppe des 5. Jahrhunderts.* (Deutsche Beiträge zur Altertumswissenschaft, 17.) Baden-Baden: B. Grimm. 1962. Pp. 64. 26 illus. Price not stated.
- HAMPE (R.) and WINTER (A.).** *Bei Töpfern und Töpferinnen in Kreta, Messenien und Zypern.* Mainz: Römisch-Germanisches Zentralmuseum. 1962. Pp. xi + 128. 50 plates (incl. 1 in colour). 60 text figures. Price not stated.
- HARDY (W. G.).** *The Greek and Roman world.* Rev. ed. Toronto: McClelland and Stewart (London: Bailey Bros. and Swinfen). 1962. Pp. x + 118. £1 10s.
- HAVELOCK (E. A.).** *Preface to Plato.* Oxford: Basil Blackwell. 1963. Pp. xii + 328. £1 12s. 6d.
- HERACLITUS.** *Das Wort Heraklits: [Fragmente].* Ed. and trans. L. Winterhalder. Erlenbach-Zürich: E. Rentsch. 1962. Pp. 160. Sw.fr. 15.
- HERODOTUS.** *The histories.* Trans. H. Carter. London: Oxford University Press. 1962. Pp. xvi + 617. 1 map. 12s. 6d.
- HIPPONAX.** *Les fragments.* Ed. O. Masson. [Thèse.] (Études et commentaires, xliii.) Paris: C. Klincksieck. 1962. Pp. 195. NFr. 40.
- HOMER.** *Odyssé: sjätte sången.* Ed. R. Strömberg. *Odyssé: tjugotredje sången.* Ed. R. Strömberg. Göteborg: the Editor. 1962. Pp. 74. 47. 9. 4 text figures. Price not stated.
- ISOCRATES.** *Discours,* tome iv. Ed. and trans. G. Mathieu and E. Brémond. (Association G. Budé.) Paris: 'Les Belles Lettres.' 1962. Pp. 268. NFr. 21.

- Kadmos: Zeitschrift für vor- und frühgriechische Epigraphik.** Band 1, Heft 2. Berlin: W. de Gruyter. 1962. Pp. 87-171. Numerous illus. Annual subs. DM. 48.
- KASSIES (W.). Aristophanes' traditionalisme.** [Diss.] Amsterdam: G. van Soest. 1963. Pp. 129. Price not stated.
- KAVAFIS (K. P.). Honderd gedichten.** Trans. G. H. Blanken. Amsterdam: Moussault. 1962. Pp. 148. Fl. 6.90.
- KERSCHENSTEINER (J.). Kosmos: quellenkritische Untersuchungen zu den Vorsokratikern.** (Zetemata, 30.) Munich: C. H. Beck. 1962. Pp. xi + 245. DM. 29.
- KLOSTERMANN (R. A.). Erzbischof Basileios von Smyrna: ein neugriechischer Prediger.** (Studia Graeca et Latina Gothoburgensia, xiii.) Göteborg: Institute of Classical Studies. 1962. Pp. 47. Sw.kr. 8.
- KOKOLAKIS (M. M.). 'Ο Υποβολιμαῖος τοῦ Μενάνδρου.** [Ἀθήνα, Ξ ζ'.] Athens: the Author. 1962. Pp. 114. Price not stated.
- LASSO DE LA VEGA (J. S.). Héroe griego y Santo cristiano.** (Univ. de la Laguna, Aula de humanidades, 1.) La Laguna: Secretariado de Publicaciones. 1962. Pp. 88. Price not stated.
- Λυκία, I.α. Ἀνδριώτης (N.Π.). Τὸ ἰδίωμα τοῦ Λιβυσίου τῆς Ἀγκίας. β. Μουσαίου-Μπουγιούκου (Κ.). Παροιμίαι τοῦ Λιβυσίου καὶ τῆς Μακρῆς.** ("Ἐσθλο Πέτρον Μπακάκου 1960-61.) 2 vols. Athens: Centre d'Études d'Asie Mineure. 1961. Pp. 125, xxxi + 473. 1 map. Price not stated.
- MERKELBACH (R.). Roman und Mysterium in der Antike.** Munich and Berlin: C. H. Beck. 1962. Pp. xii + 347, with 6 plates and 9 text figures. Price not stated.
- NOVOTNÝ (F.). Charisteria Francisco Novotný octogenario oblata.** (Opera Universitatis Purkynianae Brunensis, facultas philosophica, 90.) Brno: Department of Classical Studies of the University. 1962. Pp. 271. 9 plates. Kčs. 37.
- OEHLER (K.). Die Lehre vom noetischen und dianoetischen Denken bei Platon und Aristoteles.** (Zetemata, 29.) Munich: C. H. Beck. 1962. Pp. x + 294. DM. 35.
- ORIGEN. Homélies sur S. Luc: texte latin et fragments grecs.** Ed. and trans. H. Crouzel, F. Fournier and P. Périchon. (Sources chrétiennes, 87.) Paris: Les Éditions du Cerf. 1962. Pp. 564. NFr. 33.
- The Oxyrhynchus papyri, xxviii.** Ed. E. G. Turner and others. (Egypt Exploration Society, Graeco-Roman Memoirs, 39.) London: Egypt Exploration Society. 1962. Pp. xii + 224. 12 plates. £9.
- Palmyra. Palmyre: fouilles polonaises 1960.** By K. Michałowski. Warsaw: Państwowe Wydawnictwo Naukowe. 1962. P. 284. 4 plans. 315 illus. Price not stated.
- PANAETIUS RHODIUS. Fragmenta.** Ed. M. van Straaten. 3rd ed. (Philosophia antiqua, 5.) Leiden: E. J. Brill. 1962. Pp. xxv + 62. Fl. 12.
- PAPANIKOLAOS (K. N.). Ἡ ὀργάνωση τῆς παιδείας στοὺς Ἰόνιους τοῦ Πλάτωνα.** [Νέα ἀγωγή, Γ' / Δ', 1961.] Athens: the Author. 1962. Pp. 38. Price not stated.
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- PÉLÉKIDIS (C.). Histoire de Péphébie attique des origines à 31 av. J.-C.** (École française d'Athènes, travaux et mémoires, 13.) Paris: E. de Boccard. 1962. Pp. 350. Price not stated.
- PETIT (P.). La civilisation hellénistique.** (Que sais-je?, 1028.) Paris: Presses Universitaires de France. 1962. Pp. 128. 6 text figures. Price not stated.
- PHILO. Oeuvres.** Ed. R. Arnaldez, J. Pouilloux and C. Mondésert. 11/12. De ebrietate. De sobrietate. Trans. J. Gorez. Paris: Les Éditions du Cerf. 1962. Pp. 159. NFr. 14.70.
- PLUTARCH. Dialogue sur les oracles de la Pythie.** (Collection 'Erasmé', textes grecs, vi.) Paris: Presses Universitaires de France. 1962. Pp. iv + 82. NFr. 8.
- Re-appraisal: some new thoughts on the teaching of classics.** Ed. T. W. Melliush. (Supplement to Greece and Rome, vol. ix, no. 1.) Oxford: the Clarendon Press for the Classical Association. 1962. Pp. 48. 5s.
- ROBERTSON (C. M.). Between archaeology and art history: an inaugural lecture.** Oxford: the Clarendon Press. 1963. Pp. 26. 3s. 6d.
- ROSSI TAIBBI (G.). Ed. Vite dei santi siciliani, 3: vita di Sant' Elia il giovane.** (Istituto Siciliano di Studi Bizantini e Neellenici, testi, 7.) Palermo: Istituto Siciliano di Studi Bizantini e Neellenici. 1962. Pp. xxxii + 238. 1 map. 2 plates. L. 4,000.
- ROUNDS (D.). Comp. Articles on antiquity in Festschriften: an index.** Cambridge, Mass.: Harvard University Press (London: Oxford U.P.) 1962. Pp. xx + 560. £8.
- RUSSO (C. F.). Aristofane autore di teatro.** Florence: Sansoni. 1962. Pp. vii + 384. Price not stated.
- Samothrace. Excavations conducted by the Institute of Fine Arts, New York University.** 4, i. The hall of votive gifts. By K. Lehmann. (Bollingen Series, ix, 4.) New York: Pantheon Books. 1962. Pp. xvi + 186. 20 plates. 90 text figures. \$10.

- Sborník prací filosofické fakulty Brěnnské University.** Ročník x (1961), řada archeologicko-klasiká E 6. Brno: Department of Classical Studies of the University. 1961. Pp. 320. 28 text figures. Kčs. 38.
- SCHACHERMEYER (F.). **Die ägäische Frühzeit** (Kreta und Mykenai). IV. Bericht, die Jahre 1957 bis 1961 umfassend. [In Anzeiger für die Altertumswissenschaft, xiv, 3/4.] Innsbruck: Prof. R. Muth. 1961. Pp. 129-72. Price not stated.
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- SCHWINGE (E.-R.). **Die Stellung der Trachinierinnen im Werk des Sophokles.** (Hypomnemata, 1.) Göttingen: Vandenhoeck & Ruprecht. 1962. Pp. 139. DM. 18.
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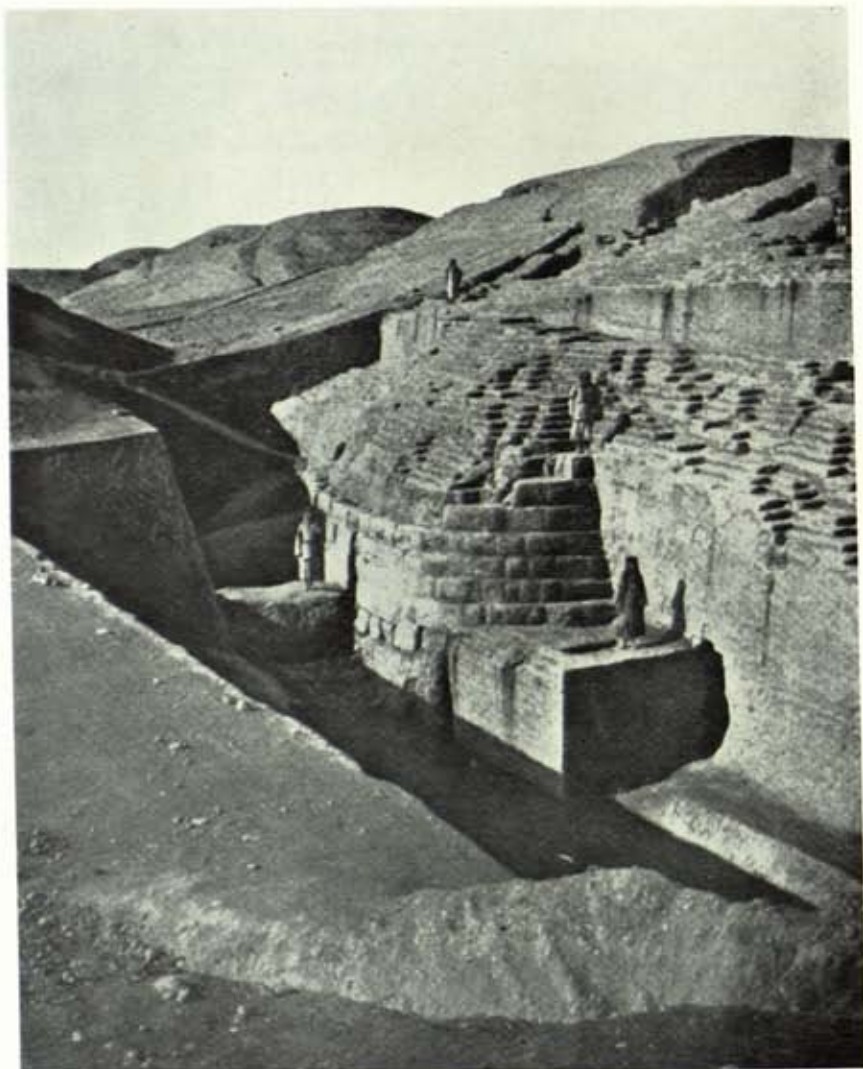
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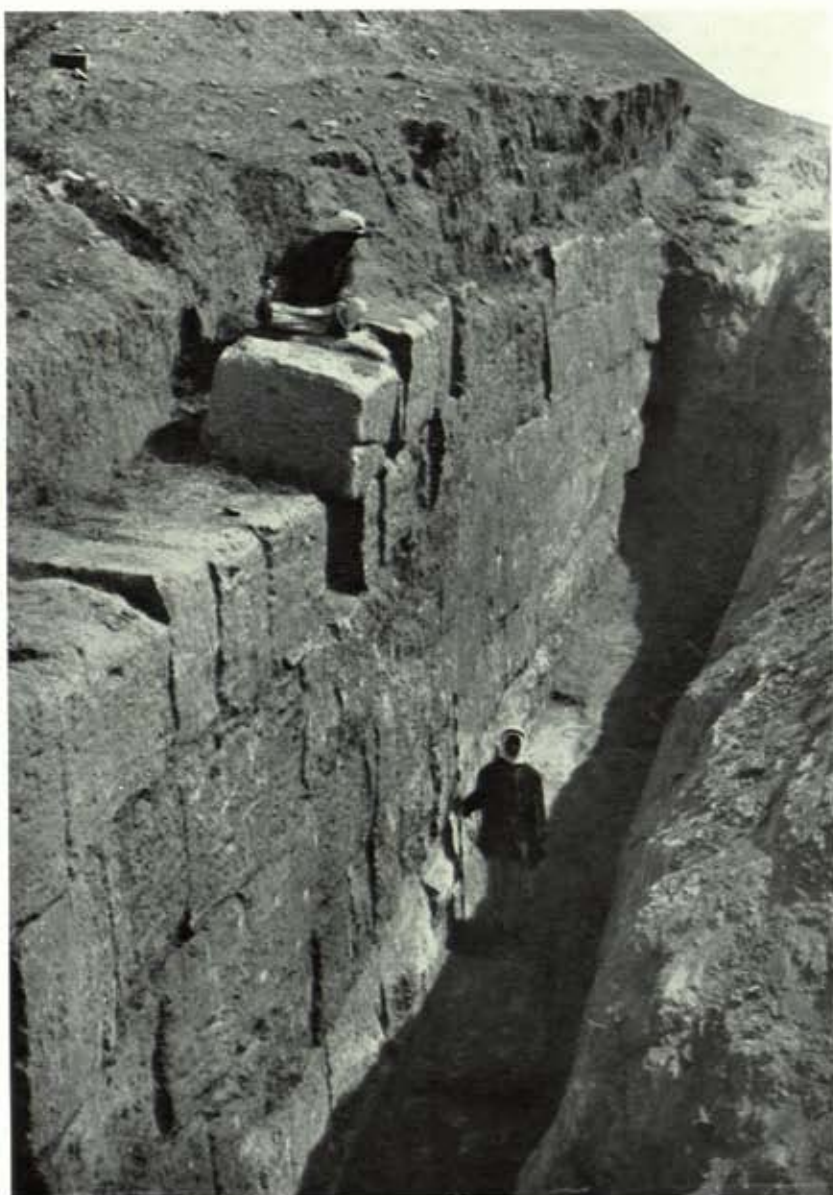


b. Nimrud, a general view. Photo M. Mallowan
XENOPHON AND THE WALL OF MEDIA



The bastion wall at Assur

XENOPHON AND THE WALL OF MEDIA



The quay wall at Nimrud

XENOPHON AND THE WALL OF MEDIA



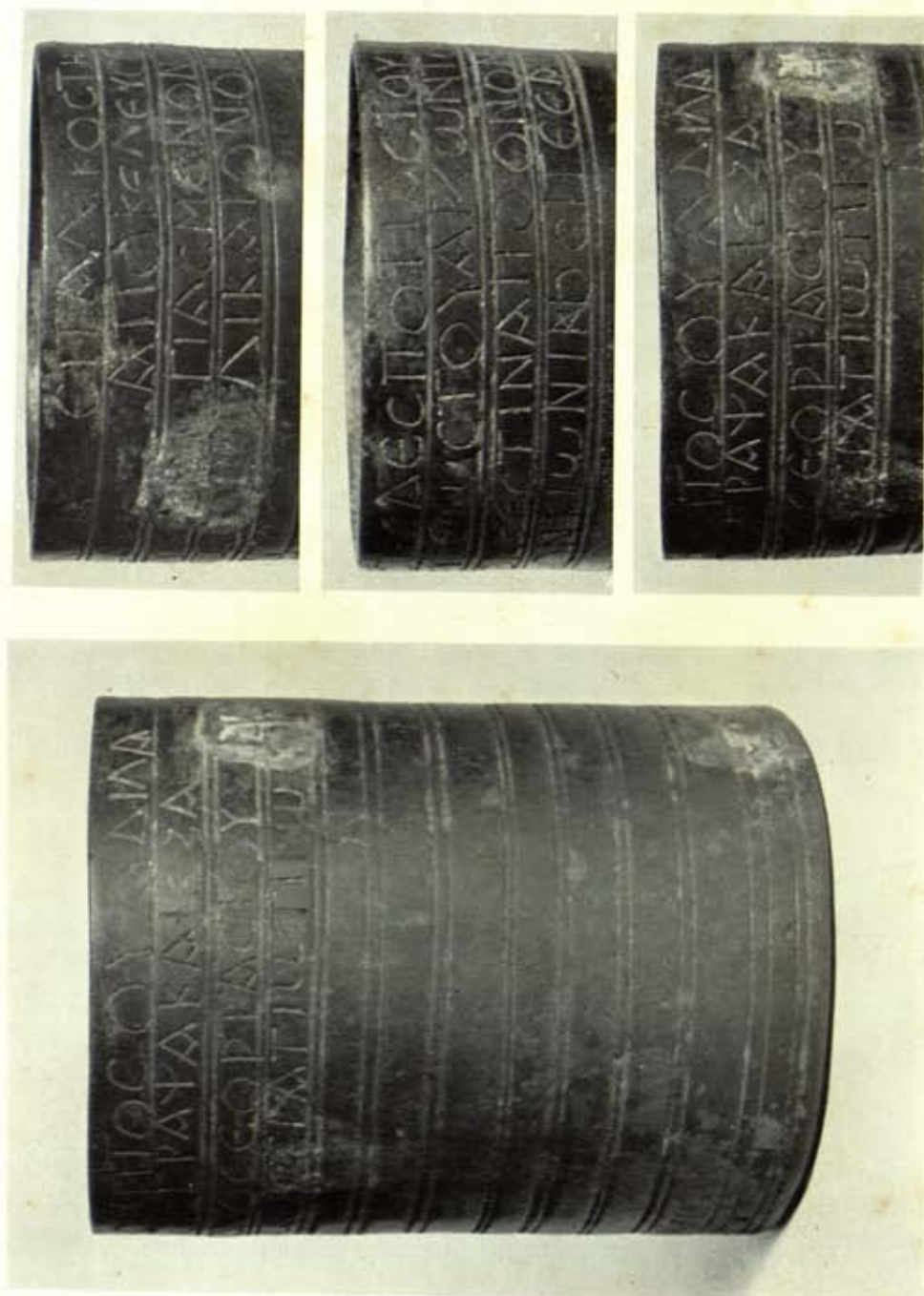
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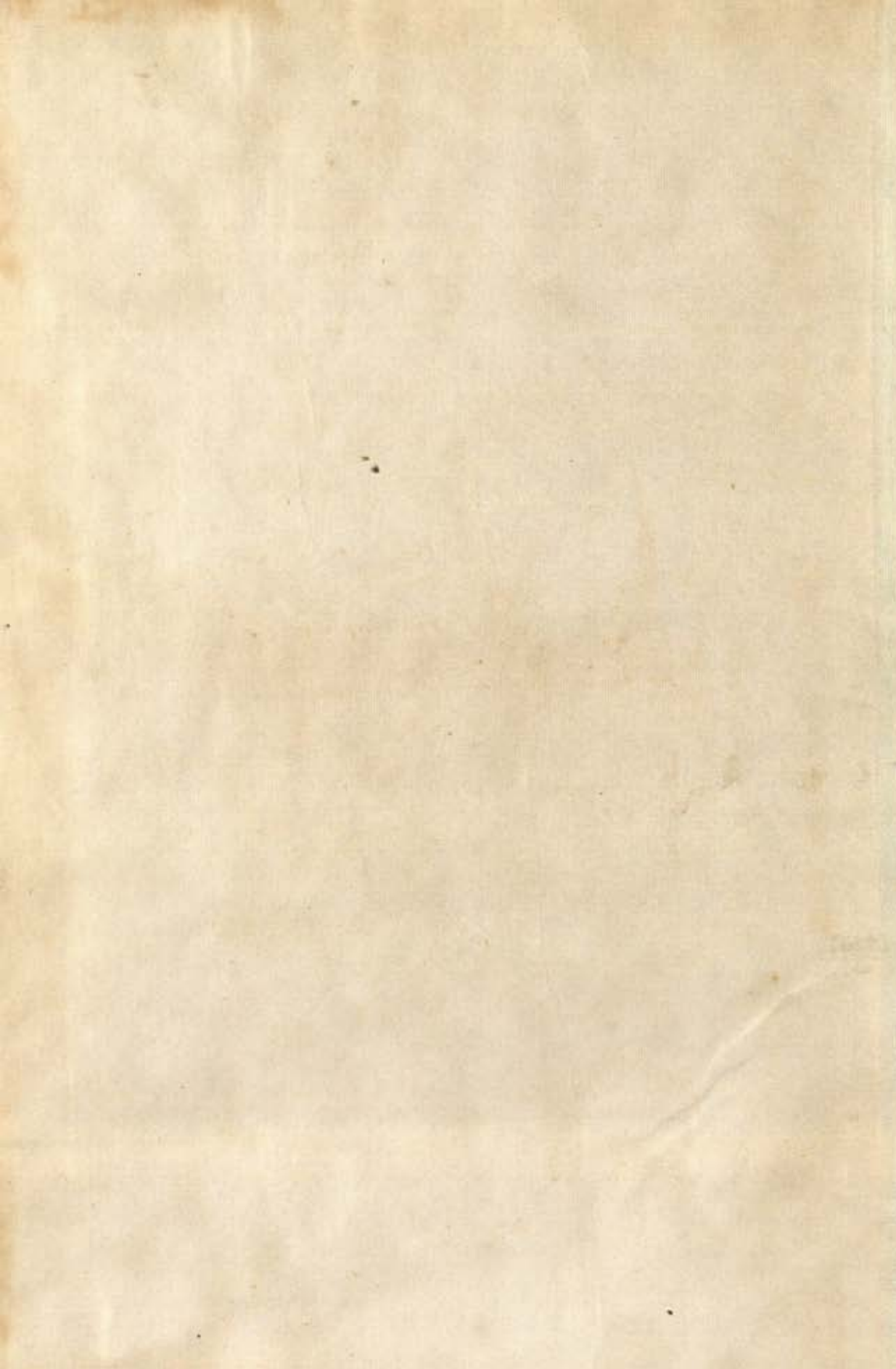
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