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THE TROJAN WAR

Mr Finley's article is an elaborated version of a talk first broadcast in October 1963. It was offered for publication with the intention of stimulating discussion of a problem which has been exercising archaeologists and historians. For this reason its author does not here answer the provisional criticisms and comments offered by Professor Caskey, Mr Kirk and Professor Page which are also printed below. It is hoped that this presentation will help to define for readers the very varied issues which attend the problem of the date and nature of the Trojan War.

I. The Trojan War,¹ by M. I. Finley

In concluding the first chapter of his Troy and the Trojans, Professor Blegen writes (p. 20): 'It can no longer be doubted, when one surveys the state of our knowledge today, that there really was an actual historical Trojan War in which a coalition of Achaeans, or Mycenaean, under a king whose overlordship was recognised, fought against the people of Troy and their allies.' Whatever 'the state of our knowledge today' may be, or may be taken to mean, one must insist that there is nothing in the archaeology of Troy which gives the slightest warrant for any assertion of that kind, let alone for writing 'it can no longer be doubted'. Blegen and his colleagues may have settled, insofar as such matters can ever be determined with finality by archaeology, that Troy VIIa was destroyed by human violence. However, they have found nothing, not a scrap, which points to an Achaean coalition or to a 'king whose overlordship was recognised' or to Trojan allies; nothing which hints at who destroyed Troy.²

Mainland Greek archaeology and the Mycenaean tablets are equally devoid of any information on that central question. What is effectively new in the state of our knowledge today, as against the state of knowledge two generations ago, is the tangential, but nonetheless important, testimony in documents from the world outside the Achaean and Trojans, and a radically new appreciation of the nature and techniques of oral poetry. But the base of the whole structure of current belief about the Trojan War obviously remains the Iliad and Odyssey. That is a platitude, but it needs to be reasserted and underscored, and I propose to argue that we have not advanced very far in a rigorous, critical assessment of the poems as evidence for the historical narrative of the Trojan War; that all statements of the order of Professor Blegen's 'the tradition of the expedition against Troy must have a basis of historical fact' are acts of faith not binding on the historian; that there is evidence which, though far from decisive, at present weighs the balance the other way.

The first problem of analysis is an operational one. Everyone is agreed that the Iliad as we have it is full of exaggerations, distortions, pure fictions and flagrant contradictions. By what tests do we distinguish, and, in particular, do we decide that A is a fiction, B is not (though it may be distorted or exaggerated)?

There is, of course, a first test which we all apply: we eliminate as pure fiction the scenes on Olympus, the divine interventions and all the rest of that side of the story. Yet I am

¹ The main argument of this paper was first presented, naturally in very different form, on the Third Programme of the BBC on October 24, 1963 and then published in the Listener on November 7th.
² I hope no one will remind me of the single bronze arrowhead found in Street 710 (Troy iv (1958) 12, 51) or of the sunken pithoi. Even if one accepts Blegen's not wholly convincing deduction (Troy and the Trojans 156) that the pithoi show 'that there was an emergency of some kind', they reveal nothing about the source of the danger.
not being frivolous when I suggest that this is at best an equivocal first step, one which makes the rest of the operational analysis more difficult. The ‘Homeric’ picture of the gods is admittedly widely divergent from the thirteenth-century one, both in its omissions and in its innovations. Many of these divergences touch the core of religious belief and of ritual. By what reasoning do we permit oral transmission so much latitude with the supernatural side of the story while denying it equal freedom with the human side? The answer is that we impose our own evaluation of what is and what is not credible on the ancients. We treat the human side of the tales as possible fact, the supernatural side as certain fiction. But did the bards and their audiences (and many Greeks in later times)—the men who were doing the transmitting and the manipulating—draw this distinction? Were the scenes on Olympus less ‘real’, less ‘factual’, to them than the miracles of the Bible are to many today? The operational analysis must work with their conceptions in these matters, not with ours; that is why I suggest that the human-supernatural test is a misleading one.

How much latitude of divergence we allow is the decisive question. Everyone allows a good deal, but nearly everyone then stops short and agrees that ‘the tradition of the expedition against Troy must have a basis of historical fact’. In the absence of literary or archaeological documentation, there is no immediate control over this will to believe. But there are oblique ways of getting at the possibilities, first by examining three other heroic traditions which we can check. There is a difficulty here because these others developed through oral poetry in times when there was an amount of literacy and of written documentation which may have acted as a contaminating influence. I shall return to that point briefly at the end; now I shall look at the Song of Roland, the Nibelungenlied, and the South Slav traditions about the battle of Kossovo as if the specific difference of total illiteracy did not exist.

In the year 778 Charlemagne invaded Muslim Spain. On the way home the rear of his army was ambushed and massacred at Roncevaux in the Pyrenees by the Basques, who were Christians. The incident was humiliating but without long-term significance. It is mentioned briefly in several chronicles of the age and that should have been the end of it. Instead, the incident, or rather Count Roland, one of the men who fell, burgeoned into an heroic tradition all over Europe, one which is still alive in very odd ways. It was brought to Sicily by the Normans, and even today in Sicily there are puppet shows about Roland and the other paladins of Charlemagne, and the same scenes appear on their decorated donkey-carts. Roland competes in peasant culture with the Sicilian Vespers and Garibaldi. The latter are obviously appropriate, Roland just as obviously is not—except as a champion of Christendom against the infidel, a completely unhistorical role into which he had been transformed at a date which cannot be fixed precisely. The earliest known text of the Song of Roland is a 4,000-line poem written about 1150. By then the ambush at Roncevaux had become an heroic battle of the paladins of Charlemagne against a Saracen host of 400,000 led by twelve chieftains, some of whom had Germanic or Byzantine names. The courtly atmosphere of the poem is not that of Charlemagne but rather that of the First Crusade, whereas the political geography fits neither period but the tenth century. In sum, the poem seems to have retained precisely three historical facts about Roncevaux and no more: that Charlemagne led an expedition into Spain, that the expedition ended in disaster, and that one of the victims was named Roland.³

In the approximately contemporary and also widely travelled Nibelungenlied, of the central characters Gunther and Atli-Etzel (Attila) are historical but had no actual relationship with each other. Gunther was king of the Burgundians on the Rhine from 411 to 437, when he was killed by invading Hun mercenaries in the Roman imperial service. The Hun

³ For all this, see P. LeGentil, La Chanson de Roland (Paris, 1955) chs. i–iii. The Roland tradition is also used, I think in the wrong way, by C. Nylander, ‘The Fall of Troy’, Antiquity xxxvii (1963) 6–11, to support his argument that ‘Homeric Troy’ is Troy VI.
kingdom, of which Attila did not become ruler until 445, was not involved. The *Nibelungenlied* turns the invasion of Burgundy by the Huns into its reverse, a complicated move initiated by the Burgundian princess Kriemhild, wife of Attila. For this there is no basis whatever; nor is there for the existence of Kriemhild, who is the one character tying the whole epic together, or of Siegfried and Brunhilde, the key figures in the first half. On the other hand, Gunther and Attila are drawn into contact with the Ostrogoth Theoderic, disguised as Dietrich von Bern, who ruled most of the western Empire from 493 to 526, with Piligrim, bishop of Passow from 971 to 991, and with many minor figures, equally anachronistic or fictitious. The *Nibelungenlied*, in sum, retains even less recognisable or coherent history than the *Song of Roland*, if it can be claimed to retain any at all.4

The South Slav heroic tradition about Kossovo, a really decisive battle, is in some respects more securely anchored in history than either the French or the German, though it is more difficult to control since it remains scattered in collections of shorter poems, never (except artificially) brought together in one long composition. The Ottoman invasion and the shattering defeat of the Serbs under Prince Lazar at Kossovo in 1389 remain fixed in the tradition. But then the variations and inventions begin, of which only two need be mentioned: the conversion of Lazar's son-in-law and chief support, Vuk Branković, into a traitor (perhaps under the influence of the *Song of Roland*, which Serbs in later times would have learned in Ragusa), and the heroisation of Marko Kraljević ('the uncrowned king of heroic poetry'), a curious figure, unimportant in real life, who certainly did not fight on the Serbian side at Kossovo and who seems to have accepted Turkish suzerainty quite cheerfully both before and after.5

We must therefore reckon with three possibilities of fundamental distortion (apart from pure invention): (1) that a great heroic tradition may be built round an event which itself was of minor significance; (2) that the tradition may be picked up by regions and people to whom it was originally, as a matter of historic fact, utterly alien and unrelated; (3) that the tradition may in time distort (not just exaggerate) even the original kernel so that it is neither recognisable nor discoverable from internal evidence alone. I am suggesting not that all three always happen, but that they may, and sometimes do, occur. Working backward, how can we tell? The 'facts' in the *Song of Roland*, the South Slav songs and the *Nibelungenlied* all look alike. They bear no stigmata which distinguish the wholly fictitious from the partly fictitious. Suppose all documentation about the period of Charlemagne were lost: How should we then be able to determine which bits of the *Song of Roland* were historical, which not? How could we know whether the battle of Roncevaux was or was not fought against the Muslims? Indeed, how could we know that there had been a battle there at all? It is only from external evidence that we know how to answer the last two questions. Schematically stated, the *Song of Roland* has the right battle but the wrong enemy, the *Nibelungenlied* has the right enemy but the wrong battle (and a wildly wrong battle-site), whereas the Serbian tradition has them both right.

Archaeology has settled the question, Was there a Trojan War? It has failed to suggest who attacked and destroyed Troy. Which model shall we then follow among the heroic traditions? I submit that the possibility cannot be ruled out that the *Iliad* (and the whole

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4 It does not matter for my purposes which school of *Nibelungenlied* scholarship one prefers; see either A. Heusler, *Nibelungensage und Nibelungenlied* (5 ed., Darmstadt, 1955), or F. Panzer, *Das Nibelungenlied* (Stuttgart, 1955), esp. chs. vii–viii. It is not without malice aforethought that I quote the latter's final sentence (p. 285) dismissing all efforts to find historical roots for Siegfried and his family: 'Die Verselbßigung von Personen und Vorgängen des Epos mit geschichtlichen waren doch nirgends ohne weitgehende Umdeutungen, ohne Gewaltsamkeit und inneren Krampf durchzuführen und blieben damit unbefriedigend.'

5 See M. Braun, *Das serbokroatische Heldenlied* (Göttingen, 1961) 100–2; D. Subotić, *Yugoslav Popular Ballads* (Cambridge, 1932) ch. ii. Subotić writes (p. 87): 'It remains a mystery why the Yugoslav heroic poetry should have made him [Marko] out to be the greatest national hero, while converting Vuk Branković into a traitor.'
Greek tradition) is wrong on this question; that the possibility must be seriously considered that the better analogy is with the Roncevaux tradition rather than with the Kossovo tradition. The fact that the Greeks themselves accepted the historicity of the tradition has no probative value. It is impermissible to defend the tradition on the ground that men like Thucydides 'may well have based their beliefs on a greater body of surviving oral and written evidence than that which has come down to us' (my italics). They had no written evidence whatsoever, and the validity (not the quantity) of the oral 'evidence' is precisely the point at issue. All Europe once accepted the Roland tradition as history, too. For centuries there was neither wish nor motive to challenge or check the tradition. 'Historical consciousness', in Jacoby's words, 'is not older than historical literature.' By then it was too late. All that Thucydides could do was sit down and think hard about the tradition. We can do better, thanks to archaeology and thanks to the written evidence from Egypt, North Syria and the Hittite archives.

The next line of investigation is to look at the documents. On the Hittites I need hardly do more than summarise certain of the conclusions in Page's History and the Homeric Iliad: (1) the Achaeans are mentioned in some twenty texts ranging from the late fourteenth to the end of the thirteenth century B.C.; (2) the Achchijawa with whom the Hittite rulers were concerned was not across the Aegean on the mainland but near at hand, an independent island or coastal state, most likely based on Rhodes and possessing some territory of its own in Asia Minor; (3) Troy is absent from the extensive Hittite archives save for one possible reference; and (4) a fortiori the archives provide no direct information on the relations, if any, between Troy and Achchijawa. One text from the final half-century of the Hittite Empire reports the rise of a kingdom of Assuwa in western Asia Minor which led a serious, but unsuccessful, coalition war against the Hittites. The southernmost member of the coalition was Lycia, the northernmost may have been Troy. A second text mentions both Assuwa and Achchijawa, but it is too fragmentary to be intelligible. Professor Page has devoted the third chapter of his History and the Homeric Iliad to a most ingenious and intricate reconstruction of this complex situation, the 'background of the Trojan War', from which there emerge two vital suggestions: that Assuwa and Achchijawa were eventually brought into direct conflict with each other, and that Troy VIIa fell in this context.

From this or any other reconstruction of the few relevant Hittite texts the only conclusion one could possibly draw is that the Trojan War was an exclusively Asiatic affair. It is the Iliad which causes trouble. Page writes about his reconstruction (p. 111) that between the Hittite annals and the Iliad 'there are large and obvious differences. The Iliad's league of natives is led by Troy, not Assuwa; and the Achaeans who attack them are not the Achaeans familiar to the Hittites... but an expeditionary force from the mainland.' The question is thus squarely put. Professor Page says 'these are differences, not disagreements'. I prefer the alternative view, that, to return to my analogy, they are fundamental disagreements exactly like those between history and tradition over who fought at Roncevaux. Page concedes that this is a serious possibility, which he then rejects because of the Catalogues in the second book of the Iliad. Before turning to them, however, I want to consider the so-called Sea Peoples.

The last half-century of the Hittite Empire was filled with rebellions and wars in Asia Minor, but the Empire was actually destroyed, by 1200 or 1190, by invaders from the north. By 1190, too, Troy had fallen; so had most of the great fortresses in Greece and important

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6 Blegen et al., Troy iv 10.
8 See P. Mertens, 'Les Peuples de la Mer', Chr. d'Eg. xxxv (1960) 65–88. In what follows I shall cite neither sources nor modern literature as I do not enter into any controversial matters except on the identification with the Achaeans of the Akiyawasa or Akawash of the Merneptah stele, on which see Page, op. cit., 21 n. 1.
local states in northern Syria like Ugarit and Alalakh; there was turbulence in the west, in Italy, Sicily and Libya; there were repercussions as far east as Babylonia and Assyria. It would be going too far, on present evidence, to link all this widely scattered activity into a single unified operation, but there is a case for thinking that a significant, perhaps the main, generating impulse was a massive penetration over a longish period by migrating invaders from the north. A number are named in Egyptian texts. Identification of the various peoples remains highly controversial, but in any event it would be wrong to believe either that the ‘Sea Peoples’ were a coherent, firm coalition moving in a single sweep, or that the Egyptian lists are either complete or wholly accurate. ‘Northerners coming from all lands’, says the Merneptah stèle. The evidence suggests to me the analogy with the Germanic migrations into the Roman Empire: broken in rhythm, confused in the interrelationships among the migrants, confused even in the motives. Like the Germans, too, these northerners, when they had finished, had considerably altered both the ethnic composition and the political situation in a large area from western Asia to the central Mediterranean.

Given this context, the most economical hypothesis is that Troy VIIa was destroyed by, or in association with, the marauding northern invasions. This is no more speculative an hypothesis, after all, than Professor Page’s, for no Hittite text says that Achchijawa and Assuwa came to blows and no Hittite text says that Troy fell at the hands of the Achaeans or anyone else. The documentation about the northern invaders—it is really necessary to stop employing the misleading term ‘Sea Peoples’—is still very thin, but as it grows, the scale and range of their destructive activity grow apace. Even the Hittites, it begins to appear, may have been affected, though perhaps only indirectly, decades before their Empire was actually smashed. This could scarcely have been guessed from the Hittite archives, and it is therefore no objection to my hypothesis to note the lack of textual evidence regarding Troy, for which there is no documentation of any kind.

Neither hypothesis requires, or indeed allows for, a mainland Achaean coalition whereas both provide a proper historical context and a motivation for the siege and destruction of Troy, which the Greek tradition does not. This question of motive is customarily, though uneasily, pushed aside. Presumably no one any longer accepts the rape of Helen as a sufficient cause of the Trojan War. But what are the possible alternatives which would explain a large-scale attack from the mainland? Troy was a powerful fortress. No ordinary booty raid, like those described by Nestor in II. xi 670–84 or by Odysseus in Od. ix 39–42 and xiv 229–85, would have had a hope of being effective, and

d temporary destructions in the east’. Her own article, ‘The Fall of the Mycenaean Empire’, Archaeology xiii (1960) 66–75, makes a serious attempt to do so and comes to very different conclusions from mine on the central question, largely, I believe, because she does not abandon the Greek tradition, even to such pseudo-problems as trying to reconcile the archaeology with the tradition of ‘the mutual exhaustion of the Trojan War’. On this general question see Starr, op. cit., 66–8. Desborough’s The Last Mycenaens and their Successors (Oxford, 1964) appeared just as this article was going to press. It seems to contribute nothing new to this particular discussion. His conclusion that the Trojan War took place between 1250 and 1230 is based not on any archaeological evidence for these two decades but on the argument that, if the tradition is to be preserved, no other dates are compatible with the archaeology (pp. 220 f., 249).
no booty raid on the necessary scale can be exemplified, so far as I know, nor, in this instance, given any plausibility (as I shall argue later). As for a commercial war, I frankly refuse to take the idea seriously until one of its proponents offers a reasonable explanation why mainland Achaeans should have organised and mobilised themselves on a great scale in order to destroy a centre to which they had long been sending a continuous supply of pottery and from which, we are told, they received horses, which they needed, wool, which in fact they may not have needed, and perhaps gold.

On any hypothesis, I am confident that the explanation of the Trojan War must be either political (in the sense in which war was incessantly being waged in Asia Minor and the Near East for political reasons) or ‘accidental’ (that is to say, external) incursions into the area from outside for reasons which must be sought (and cannot be found at present) to the north. Neither kind of explanation excludes the possibility of an Achaean share in the operation (as distinct from an Achaean initiative or monopoly). Page has conjectured a political struggle between Achchidjava and Assuwa. I prefer the hypothesis that Achaeans joined a marauding force of northerners, just as they had been part of the mercenary force engaged by the Libyans when they attacked Egypt in the reign of Merneptah (1220 B.C. or thereabouts). We do not know who those Achaeans were or where they came from, and I have no suggestion to make about the Achaeans who, on my speculation, shared in the destruction of Troy. They could have come from Asia, from the Aegean or from the Greek mainland. The essential point is that the half-century or more of migration, invasion and marauding was one of general disruption, precisely like the age of the Germanic migrations, during which allegiances and alliances were shifting and blurred. It would be an obvious guess that, when their own society was under such severe pressure, bands of Achaeans took to buccaneering and mercenary service, sometimes as allies of the invaders. The Merneptah stele makes it unnecessary to guess, as does, in a different way, the career of Attarssijas, so dramatically described in Page's third chapter. (If new texts should confirm Otten's recent suggestion of a direct link in Alasija (Cyprus) between the 'Sea Peoples' and the marauding of Attarssijas, I should feel myself on very firm ground indeed.) The invaders themselves, again like the later Germans, ultimately sought to settle, but on the way they looted and burned, detoured, played the mercenary, as circumstances directed. For them to smash Troy, with or without Achaean supporters, is an altogether different, and far more intelligible, manoeuvre than the Homeric tale.

The archaeology is not inconsistent with a smash-and-grab raid, though an unusually devastating one. Life was then resumed in Troy: the citadel was reoccupied, 'new houses were superposed over the ruins of their predecessors', 'the fortification wall evidently still continued to stand, or was repaired'. It is only in Troy VII b 2 (which Professor Blegen puts 50 or 60 years after the destruction of VIIa) that we find novel architectural features and the Knobbed Ware which points unmistakably across the Hellespont. Does that mean that a foreign population did not enter Troy until then? The question is at present unanswerable because we do not yet properly understand the significance of Myc. IIIC pottery, which far exceeds IIIB in number in Troy VII b 1. In considering the same problem for the Greek mainland, where the uniform IIIB style of the great centres was replaced by locally varied IIIC after their destruction, and only later by proto-Geometric, Desborough concludes: 'It might be argued that some one of the variations of the new pottery style should belong to newcomers, and this is not impossible, though it is not provable, as in each district L.H. IIIC pottery seems to be clearly linked at the outset with the preceding style.' In sum, the pottery finds in Troy are in the present state of our knowledge compatible with any explanation of the Trojan War. If the Philistines could

13 Blegen, "Troy and the Trojans" 165-6 and ch. viii generally.
sit down at once to make Myc. IIIC pots so could new occupants of Troy, especially if potters survived the attack and went on working, as they obviously did in many other places. Alternatively, if the attackers moved on after wreaking all the damage they could, then the continuity is no problem at all, nor is the probability of a further incursion, this time for permanent settlement, 50 or 60 years later.

There is one archaeological argument, however, which, in my view, is more compatible with my hypothesis than with any other, and that bears on the date of the destruction of Troy VIIa. Blegen and others place it near the middle of the thirteenth century (Blegen himself tending to take it further back all the time, even to 1270), arguing from Furumark’s chronology of the pottery and from a hypothetical tempo in the development and change of the relevant styles, IIIB and IIIC. Most archaeologists, I believe, now tend to reject both the reasoning and the date. Mrs Vermeule, for example, has put the matter squarely: ‘It must be emphasised that the general character of the pottery in all these destruction levels is similar, whether at Troy and Ugarit or at Mycenae and Pylos. There is real difficulty in making any distinctions of date among them.’\textsuperscript{15} Imported IIIB pottery was still current in Ugarit and Alalakh when they were destroyed about 1190 by the northern invaders.\textsuperscript{16} And so it was in Troy, too, to be followed there by the immediate emergence of IIIC, and that argues for a date nearer 1190 than 1250 for the fall of Troy VIIa.

It is obviously very convenient for my argument to get the destruction of Troy down in date into the heart of the invasion period. But it is not altogether essential, at least not for my rejection of a mainland coalition. Suppose the dates are moved back, provided they are all moved together as they must be. The argument from motive would still stand. On any dating, it is reasonable to imagine that, just when the Achaeans states of the mainland were faced with grave difficulties and even total destruction at home, they would take it into their heads to join forces in a wild and risky venture overseas, committing their manpower to go after booty, captive women or whatever? It is surely more reasonable to think that when their own world was threatened, bands of Achaeans left to join the marauders in the search for booty or new homes or just escape and hope (provided one feels the necessity of getting some mainland Achaeans into the story at all, which I do not much care about one way or the other).\textsuperscript{17}

And now, finally, the Catalogues. As part of the \textit{Iliad} they are a mess on any interpretation. Again I need not go into details—about the central role of the Boeotians, the irreconcilable conflict between Catalogue and narrative over the kingdoms of Agamemnon, Achilles and Odysseus, the numerous disagreements in other matters—since they are all laid out in the fourth chapter of Page’s \textit{History and the Homeric Iliad}. I agree fully that the Catalogues and the narrative in the \textit{Iliad} as we have it developed separately in the oral tradition and were eventually joined mechanically at a time when they had acquired their many contradictory and irreconcilable elements. There is only one question to be considered: Does the Achaean Catalogue, for all its distortions and fictions, retain a large, hard core of Mycenaean reality which compels us to believe in the existence of a mainland coalition against Troy?\textsuperscript{18} Page and others answer in the affirmative, essentially on the single argument that a substantial number of the place-names fit known Achaean sites and that a small but still substantial number were gone in post-Mycenaean times and were

\textsuperscript{15} Op. cit., 68.
\textsuperscript{16} W. C. Hayes \textit{et al.}, ‘Chronology’, rev. CAH i ch. 6 (1962) 67–8 (Rowton), 75–6 (Stubbings); \textit{cf.} Desborough, \textit{op. cit.}, 12.
\textsuperscript{17} Blegen is acutely aware of this difficulty and he tries (\textit{Troy and the Trojans} 163–4) to get round it by dating the destruction of Troy VIIa midway in ‘the ceramic phase IIIB’ (about 1260), the destruction of the mainland centres ‘toward, or at the end of’, the phase (by or about 1200). If that distinction is untenable, as other experts on the pottery say, then the whole structure of his chronological argument falls.
\textsuperscript{18} I do not propose to waste time on other, fanciful, possibilities, such as the existence and preservation of a written Order of Battle; see Page’s review of Jachmann, \textit{Der homerische Schiffskatalog und die Ilias}, in \textit{CR}, n.s. x (1960) 105–8.
unknown to Greeks of the historical period. I accept both statements though I believe
they are exaggerated. But I draw a very different inference. The fact that Greeks from
the eighth or even the ninth century on had lost all trace and memory of Dorion or Aepy
or twenty more such places has no relevance to what may have been remembered two or
three generations or even two centuries after the destruction of Troy and of the lost places.19
‘Destruction’ is a dangerous word. Few places were ever so destroyed that no life continued
or returned there, and anyway people lived on with memories, even if they moved elsewhere.

It was in the post-destruction, post-Mycenaean generations that the traditions about
the heroic age and the Trojan War took shape.20 That seems to be characteristic of ‘heroic
ages’ nearly everywhere21: they are looking back after a break-down, and the past itself
moves along with the generations of the present. Witness the Boeotians in the Achaean
Catalogue. If in the early Dark Age the idea of a mainland coalition were invented, that
is, if the main attackers in the Trojan War were shifted from Asia to Europe, that could
have been done only by Achaeans (perhaps I should say ‘ex-Achaeans’) who looked to the
mainland, and primarily to the Peloponnese, as their original homes (wherever they were
now living). It would then have been simple enough, and indeed inevitable, for the
specific place-names to be selected, in the first instance, from the place-names of Mycenaean
civilisation.

We have a choice of explanations, neither of which is easy for us to visualise opera-
tionally in our kind of world. One is that a very long muster-roll was passed on orally,
generation after generation, either unattached to poems about the war itself or attached to
versions very different from the one which finally survived, gradually distorted and in
particular acquiring a wholly false Boeotian colouring. The other is that the very idea of a
coalition and the appropriate catalogues were both built up without historical foundation
during the generations after the Trojan War. Neither explanation gets round the
grotesqueness of the final interpolation, and the choice between them is subjective and not
a very happy one. My own choice is determined, negatively, by the failure of the
‘Mycenaean geography’ argument to carry conviction; positively, by everything else I
have said thus far.

It is actually possible to narrow the field for subjective decision a bit further. If one
agrees with Professor Page that it is ‘certain that the Catalogue was originally composed
in Boeotia’ and if one accepts the tradition, repeated by Thucydides (i 12), that the
Boeotians migrated from Thessaly sixty years after the Trojan War, then it must follow
that a considerable period of time may have elapsed between the Trojan War and the
making of the [Achaean] Catalogue’ (p. 152). The interval, Page continues, was never-
theless ‘too brief to allow us to regard as fictitious the expedition with which the Catalogue
is connected’. The point of disagreement is therefore the length of time required for it to
become permissible to believe in total invention. (The Achaean Catalogue, we recall, is
the only ground for deciding that there are ‘differences’, not ‘disagreements’, between the
Ilid and the Hittite annals.) Obviously we cannot pinpoint the interval between the War
and the Catalogue at exactly sixty years; it might have been a hundred. But even sixty
is, in my view, long enough.22

I do not underestimate the strain it puts on the imagination to suggest that ‘unofficial’
Achaean participation in a marauding operation was twisted and magnified into our heroic
Troy War. But I do not believe the strain is any greater than that imposed by the

19 This point has been made in a review by A.
20 The basic discussion is now G. S. Kirk, The
21 See Sir Maurice Bowra, The Meaning of a Heroic
Age (Earl Grey Memorial Lecture, Newcastle, 1957).

22 It is enough to cite the classic article of R. H.
Lowie, ‘Oral Tradition and History’, reprinted in
his Selected Papers in Anthropology (Berkeley, 1960)
202–10.
transformation of Christian Basques into Saracens and of a Hun invasion of Burgundy into a Burgundian invasion of the Hun kingdom. Yet we know those things happened; we can also suggest, after the fact, the psychology which underlay the transformations, and it would not be difficult to spin out an explanation of the rise of the Achaean epic. At this stage, however, when the suggestion is still only hypothetical, that would be a pointless gesture.

It can, and no doubt will, be argued that all comparisons with Roland and the other heroic traditions are false because they have all been contaminated by chronicles and other written documents (which is certainly true), whereas the Greek tradition was purely oral and a proper professional tradition of creative oral poetry is conservative and therefore tends to be more accurate and ‘historical’. (A similar objection can also be raised against the undeniable evidence of the worthlessness of non-poetic traditions in illiterate societies in the Americas and Africa: there it is not writing which contaminates but the lack of a poetic tradition.) I do not know how one meets such an argument for the obvious reason that it is impossible to study a strictly oral poetic tradition over a long enough period of time. Control can come only from written documents, and the very existence of the latter automatically removes a culture from consideration.

In the end, the one hope for progress from hypothesis to verification (of any of the various explanations of the Trojan War) is that new Hittite or North Syrian texts may yet produce direct evidence. Until then, I believe the narrative we have of the Trojan War had best be removed in toto from the realm of history and returned to the realm of myth and poetry. The Song of Roland tells us much about feudalism in the eleventh century, nothing of any value about Charlemagne’s court and the battle of Roncevaux. The Iliad and Odyssey, likewise, tell us much about the society in the centuries after the fall of Troy and scattered bits about the society earlier (and also later, in the time of the monumental composers), but nothing of any value about the war itself in the narrative sense, its causes, conduct, or even the people who took part in it.

No one in his right mind would go to the Song of Roland to study the battle of Roncevaux or to the Nibelungenlied to learn about fifth-century Burgundians and Huns. I do not see that the situation is any different with respect to the battle at Troy. True, we have nowhere else to turn at present, but that is a pity, not an argument.

II. ARCHAEOLOGY AND THE TROJAN WAR, by J. L. Caskey

Material evidence from the site of Troy has indeed not proven that the place was captured by Mycenaean Greeks. Proofs, of the kind that Mr Finley demands and that we all should like to have, rarely come to light in any archaeological excavation. When they do appear they must be in the form of written documents, and in a language that can be read. At Troy these have not been found. If the argument is to be based on that fact, let us clear the ground altogether by asserting another negative: the physical remains of Troy VIIa do not prove beyond question that the place was captured at all. An accidental fire, in unlucky circumstances, on a day when a strong wind was blowing, might account for the general destruction that is known to have occurred. Furthermore, if this citadel was not sacked—and indeed if it was not sacked by Greeks under Agamemnon—we are left without a compelling reason even to go on calling it Troy.

All this is familiar to students of the problem and scarcely calls for a detailed restatement. The archaeological evidence, like the literary and historical, is incomplete and inconclusive.

I do not propose to re-enter that controversy here (for the latest critical survey of the literature, see P. Vidal-Naquet in Annales xviii (1963) 703–19), or to repeat the reasons for my view that one may legitimately reject the narrative as fiction but not the social and cultural institutions.
One may take refuge in a declaration of doubt, but not many will choose to do so; Homer will not let us rest. And if an experienced archaeologist, who is not ignorant of Greek literature or of historical method, presents a short account of Troy for the benefit of non-specialists, surely he may be permitted to include a concise statement of his considered opinion upon this subject without adding a cloud of provisos and reservations that would defeat his audience.

The facts observed in the course of excavations at Troy remain unchanged. The Sixth Settlement ended with an earthquake of great violence. In Troy VIIa the people used the same fortifications, only slightly modified, and almost the same kinds of pottery and implements, but built their houses in a different way from those of Troy VI, different enough to make us certain that their manner of life had changed also. Settlement VIIa perished utterly in a fire. Of the succeeding settlement, VIIb 1, only a few remnants were discovered and tested in the campaigns of 1932–38. The walls of its houses and the objects of daily use resembled those of VIIa, and there is no evidence that the inhabitants were of another stock. To what extent the fortifications were again restored is not certain, nor can we say positively how VIIb 1 came to its end. Troy VIIb 2 appears not to have been fortified. Again there are signs of continuity, but innovations appear also. The socles of the house walls often are built with stones set upright, a new structural feature, and about one-half of the pottery is of a distinctly different, handmade, foreign type.

Throughout these four periods Mycenaean wares were imported and imitated at Troy, and it is this pottery which gives the best evidence of chronology that we possess: the ‘best’, and requiring the closest scrutiny, but still far from perfect. Again let the facts be stated plainly, and the limitations be quite clearly understood, especially by those who are not used to handling pottery. Whole or nearly whole Mycenaean vases are rare in the stratigraphically certified layers at Troy, as at most other sites on the borders of the Mycenaean world. Sherds, which may be displaced and are therefore much less reliable for accurate determination of chronology, represent many more pots, hundreds altogether, but even so they are scarce enough. And exact dating of Mycenaean shapes and styles has not been established. Mr Finley may properly refer to this uncertainty, but having done so he may not imply that his arguments for a lower dating have been substantiated thereby.

The limitations were well known to Professor Blegen and his colleagues as they examined the pottery from Troy for the nth time about ten years ago (some twenty years after the Cincinnati excavations began). Arne Furumark’s book was at hand, a monumental compilation of the whole body of Mycenaean material as known from publications before the second World War, and it was carefully consulted. But Finley’s present suggestion that Furumark’s methods and conclusions were uncritically accepted is an egregious mistake. So also is the studied but undocumented remark that ‘most archaeologists’ now reject his reasoning and his dates. Both were remorselessly examined from the start and both remain subject to criticism and correction. The fact that they also continue to command general respect is sufficient testimony to their basic value.

What then may be said today about the archaeological evidence for Trojan chronology? The ceramic style called Mycenaean IIIb had come into existence before Troy VI was destroyed. Mycenaean IIIC is not found in Troy VIIa but appears in VIIb 1 and thereafter in VIIb 2. In spite of the uncertainties outlined above, these observations remain valid; to question them without rehandling the pottery itself is a waste of everybody’s time. Assigning absolute dates to the pots and fragments, however, is another matter. That Troy VI ended near 1300 B.C. need not be doubted, but the stylistic change from Mycenaean IIIb to IIIC has not been firmly fixed, as Finley (among others) quite properly points out. In time it will be determined, but as yet we must allow a latitude of some twenty or thirty years toward the end of the thirteenth century, with a distinct probability that there was a moderate chronological overlapping of styles in some regions.
The stratigraphy observed at Troy, specifically in Squares E9 and J5, was insufficient to reveal whether pottery of Mycenaean IIIC styles appeared early or late in Period VIIb 1. If these styles reached Troy early in that period, and therefore soon after the destruction of VIIa, I should agree that it was difficult to accept Professor Blegen’s most recent conjecture of c. 1270–60 B.C. as the date of the destruction. If VIIb 1 lasted for, say, two generations, and Mycenaean IIIC pottery arrived toward the close of the period, there is no difficulty. This is another of the uncertainties that cannot be resolved at present.

A number of Finley’s arguments are provocative and undoubtedly will stimulate lively debate. Too often, it seems to me, they end by saying, in effect: ‘You cannot prove that you are right, and therefore I declare that you are wrong.’ I myself feel invited to answer in the same vein by stating my own opinion (in no wise exclusively mine) that the development of Carolingian, Burgundian and South Slavic tales, fascinating as the subject may be, has very little indeed to do with that of the Iliad and Odyssey, and that ‘faith’ in the value of early Greek tradition is a quite respectable possession, if by the word one means the conviction at which a scholar has arrived after long and sober reflection upon all the available evidence.

But let us return to the war at Troy. If the sack of Settlement VIIa is ever shown to have occurred after the fall of Mycenae and Pylos, or at the same time, we shall indeed have to reject most of Homeric tradition. An army other than that of Agamemnon will have been the conqueror, and for its origin we may have to look despondently among those restless unnamed peoples of the north. Finley’s suggestion that some dispossessed Achaeans might have joined them, and so have brought the event into Greek legends, is ingenious and would become plausible if the lower chronology prevailed.

On the other hand one can easily picture a different set of circumstances, equally practical and unromantic, which accords with the earlier date of the Trojan War and Achaean leadership, without denying a rôle to the shadowy tribes whom Egyptians called Peoples of the Sea.

Troy occupied an exceedingly important place, strategically, on the Hellespont. In the fourteenth century it may have been a strong bulwark against foreign intrusion into the Aegean, and as such, we may suppose, a friendly ally or at least a respected peer in co-existence with Mycenae. Then around 1300 Troy VI was ruined by a natural catastrophe and the succeeding generation of Trojans, in VIIa, was obviously weaker. Perhaps it was thought to be less trustworthy as a guardian, and precisely at the time when there were reports of serious new dangers from the north. That, surely, was the moment for Achaeans, who were still strong, to improve their defences at home and to send an expedition to secure the Hellespont. The venture was ill-advised, as it turned out: Troy was more powerful, even now (in Period VIIa), than the Achaean general staff had expected; the siege was long and so exhausting that the Greek forces, though ultimately victorious, could not take advantage of their success and occupy the straits; their remnants straggled homeward. Thereafter, of course, Troy was indeed spent, and when the barbarians did finally arrive the inhabitants of the town that we call VIIb 1 were unable to offer serious resistance.

This is a fantasy, if you will—imagined, unsupported by positive proof—but it is not less valid than the theory that Mr Finley propounds. In my opinion an explanation of the events along these lines will come nearer to the truth.
III. THE CHARACTER OF THE TRADITION, by G. S. Kirk

That epic traditions can distort historical events is commonly accepted, and rightly so. Mr Finley's thesis makes a welcome opportunity for asking what sort and extent of distortion are to be expected from traditions of different kinds. He cites distortions in the Nibelungenlied, the Chanson de Roland and the Kossovo poems as support for the idea that the Homeric tradition could have developed out of an attack on Troy by northern invaders, perhaps with Achaean freelance helpers. The position shared by Finley with Heubeck, Nylander, Starr¹ and others is that, if the incomplete archaeological phenomena do not at all points accord with the Homeric tradition about Troy, then it may well be the tradition that is at fault.

My main task in this discussion is to consider the argument from supposedly analogous and more recent traditions. I may as well say at once that I am not convinced that difficulties over the date of Troy VIIa and over the idea of a Panachaeus expedition in the thirteenth century B.C. can best be met by the assumption of a major misunderstanding in the oral tradition. At the same time I see the dangers of drawing too precise a division between major and minor, unacceptable and acceptable, degrees of misunderstanding.

Nilsson stated in 1933 that the Teutonic epics 'raise many more problems than they can solve', and unfortunately that is still the case.² The Nibelungenlied, in particular, is an academic puzzle almost of the order of the Homeric one: how did it develop, what were its different regional components, how far is it oral? The stages by which the eventual twelfth-thirteenth century version was reached, the nature of the distortions and additions due to successive centuries, are still largely unknown. Trying to construct Homeric probabilities on the basis of the Nibelungenlied is like trying to solve metaphysical problems on the basis of the metaphysics of Anaximander—it might be all right, if we knew what Anaximander's metaphysics were. For example, Finley's statement that 'there is no basis whatever' for the existence of Kriemhild or Siegfried means 'there is at present no known basis'. Certain other facts, it may be conceded, are more positively known: it is true that the chronological displacement of Theoderic is a notable example of historical distortion in a heroic tradition, and Finley rightly emphasises, too, the reversal of actual allies and enemies. Yet against these undeniable examples I must raise a point of central importance to which Finley himself has briefly alluded: that, although almost everything remains unknown about the processes by which events in central and northern Europe in the fifth and sixth centuries A.D. came down into the poetical form of our much later Nibelungenlied, it is known that written records, and the passage of written narrative material from one country to another, were elements in the situation.³ Curiously enough the art of writing can promote not accuracy but inaccuracy in the understanding of past events, at least until the copying of books, by well-organised scriptoria or later by printing, ensures the wide propagation of a stable version of what happened. Most of the early medieval chronicles were copied only in small numbers, if at all, and one-sided local versions proliferated. The position of Latin as a lingua franca in the literate world made the conflation of different ethnic accounts all the easier. When a copy of a regional chronicle happened to pass abroad it could be distorted, misunderstood, and conflated with other and incompatible material, with little if any control.

Something similar happened with written copies of poems. The Hildebrandslied fragment, in which Attila and Theoderic are already made contemporaries, dates from as early as around A.D. 800; and we know from Egginhard's Vita Caroli Magni (29) that Charlemagne

¹ See Finley's n. 9 for references.
² M. P. Nilsson, Homer and Mycenae (London, 1933) 185.
himself commissioned collections of ancient and barbarous songs—what an opportunity for conflation, confusion, and error! Moreover the minstrels themselves often travelled farther afield (in ethnic terms, certainly) than their Greek predecessors; though *Widsith* need not be taken too seriously.4 These circumstances, together with the complexity of the migrations and invasions which in central Europe followed the disruption of the Roman Empire and accompanied the steady expansion of the Germanic peoples, helped to produce an extraordinary hotch-potch of quasi-historical traditions. The effects of this can often be judged by comparing English and Nordic versions of the same historical events.

Each region and ethnic group had its own version of important happenings and its own special heroes. Time-relations and personalities, even the whole trend of heroic enterprises, could undergo serious distortion in a mixed tradition. To the complications produced by the interplay of local and external versions must be added the effects of all these written records on the oral singer. It appears from analogous modern situations that he believes, often wrongly, that he cannot compete in accuracy with any written account; he tends in these circumstances to abandon the ideal of realism and historicity and feels free to embroider and conflate the stories he has inherited. In a true oral tradition, on the other hand, which suffers neither competition nor intrusion from literacy, the risk of serious conflation is initially slighter; and, in addition, the broad lines of heroic events handed down from earlier generations are controlled by a knowledgeable, conservative, and strongly localised audience.

For all these reasons it is of primary importance, in assessing the probability of serious distortion in a heroic poetical tradition, to decide whether the tradition in question is purely oral or whether it is complicated by the effects of writing.

Everything suggests that the Homeric oral tradition was a pure one, free from intrusion by literate chronicles or poetic versions ossified in writing. Writing seems to have died out after the fall of the Achaean palaces near the end of the Bronze Age, to be revived only with the introduction of the simpler Phoenician system at a time when poetical traditions about Troy were already well established. As for the Bronze Age linear scripts, there is no evidence to suggest that they were used for the recording either of annals or of literature. The external evidence of the availability of scripts is confirmed by the internal evidence of the epic language itself, in which a highly developed formular structure also argues for a completely oral tradition.

The consequence is that the sort of thing that happened, in the course of transmission, to Attila and Theodoric (who becomes Dietrich of Bern) is unlikely to have happened, in anything like the same degree, to Agamemnon or even Achilles. Admittedly Ajax and Nestor may have suffered some chronological displacement; and the latter, like Idomeneus, may have worked his way into a central tradition from an outlying region. Yet the cultural and historical background remains broadly constant. When the conditions of a heroic age last for many generations (as we see from the Muslim tradition of Bosnia and Montenegro, fed by heroic circumstances which persisted from the sixteenth to the nineteenth century), it is easy for individual heroes to extend their range both in time and in space and for similar events and personalities to become compressed. What is less easy is for a hero to stray right outside the sort of heroic *milieu* to which he originally belonged. That is what seems to have happened with certain figures of the Teutonic saga; and here the impurity and geographical diffusion of the tradition are largely responsible. For the sake of completeness one other cause of displacement should be mentioned: if a hero possesses marked *genre* characteristics, perhaps originally derived from life, then he may become uncommonly popular with singers and audiences and intrude, like Marko Kraljević, into backgrounds and circumstances outside his own.

Persons and events may suffer distortion for rather different reasons; and it is with events that Finley’s argument is primarily concerned. Now some poems and some traditions are concerned with minor or highly localised events, others with major ones of widespread effect. So, for example, the Muslim tradition represented in the first volume of Parry-Lord, Serbo-Croatian Heroic Songs, takes as its main subject frontier skirmishes of the sixteenth century, while the Christian Serbian tradition focuses on the crucial battle at Kossovo in 1389. Other things being equal, the latter type of event tends to become more securely fixed in its outlines than the former, in which the details of each small-scale encounter are liable to be conflated with those of many others. It is possible that the battle of Roncevaux, described in the Chanson de Roland, was of this minor type, though that is not undisputed. The encounters behind the Nibelungenlied also belong to this type rather than to the Kossovo category; and here, too, the tradition is built on a complex of battles (whatever their scale) rather than on a single event—another factor which tends towards conflation and distortion. The distinction between major and minor encounters, and singular and plural ones, must be firmly carried in mind when we try to assess distortions in the Homeric tradition about Troy (though it is true, as Mr Finley would not allow me to forget, that our only evidence for the magnitude of the attack on Troy is precisely the subject of dispute, namely the Homeric tradition itself).

The Chanson de Roland differs from the Nibelungenlied in the degree of its identifiable distortion and the number of different regional versions and components that may be involved; but resembles it in the profound obscurity of its development. Again, however, written annals and records, many of them in Latin and therefore able to cross regional and racial frontiers, were certainly available. They undoubtedly affected some stages of the developing tradition, and the use of written Gesta Francorum is even mentioned in the poem (3262; cf. 1443, 1683, 3742); so are ‘charters of St Giles’, who claimed to be at Roncevaux (2095–8). Yet surviving records mentioning the battle of 778 are few, and indecisive over scale and detail. They do not justify a categorical estimate of the degree of distortion in the twelfth-century poem, at least so far as the nature of the enemy and the importance of the engagement are concerned. With personalities (like the traitor Ganelon) distortion or chronological displacement is more certain, but also more predictable; and it is generally agreed that the cultural background is much affected by the more recent times of the first crusade. Elaborate theories have arisen about the formation of the poem, and even the most unlikely of them have had their vogue. My impression is that there is more to be gained from using Homer to elucidate Roland than vice versa.

The South Slavic tradition is regionally more unified than the Teutonic or even the Frankish, though complicated by the juxtaposition of different religious elements (Christian and Muslim) with opposing political sympathies. In most respects it is more closely comparable with the Homeric tradition than are the other two. The poetical distortion of the events of Kossovo is not very serious, as Finley agrees; and this really removes the South Slavic tradition from the case he is trying to make, or even places it at the disposal of his opponents. By the evidence of contemporary chronicles, both Christian and Turkish, the poems of the Kossovo cycle get the enemy, the result, and most of the main personalities right. There are slight differences between prose and poetical accounts of the manner in which Miloš Obilić killed Sultan Murad, but that is a detail and does not involve the degree of distortion with which Finley’s case is concerned. It is the kind of imprecision which is inevitable in any oral tradition, even a tightly-controlled poetical one. More serious distortion is seen in the interpolation of Marko Kraljević as a conspicuous hero on the Serbian side, and in the casting of Vuk Branković in the role of traitor. In the latter case it may be conceded that personality and event are interwoven; and the explanation of the

Serbian defeat as due to Vuk’s defection involves a substantial departure from historical reality. The historical Vuk Branković seems to have turned over to the Turkish side some years after the great defeat, as a result of a personal quarrel. Somehow this allowed him to fill a role that must have been greatly desiderated by singers and their audiences, that of scapegoat or acceptable cause of defeat. For defeat to be caused by inferior bravery or skill at fighting was intolerable in a genuine heroic tradition; betrayal, inferior numbers or divine displeasure are the acceptable thematic causes. As for Marko, I have already mentioned that this relatively obscure historical person became a legendary figure with genre or folk-tale characteristics, and so worked his way into songs and events with which he originally had no connexion. He apparently did not fight at Kosovo, but he lived at that period, and it is not altogether surprising to find him in the Kosovo songs—though it is surprising to find him so prominent there. It is unlikely that literate influences played much part in the transformation on this occasion; but written chronicles, the cultural domination of a literate priesthood, and even written copies of poems, are potentially important elements in the heroic tradition of Christian Serbia. That is why the Muslim songs provide on the whole a safer (though still distant) parallel to the Homeric tradition than those of the older but more impure and more sophisticated Kosovo cycle.

In short, the comparative material adduced by Finley does not seem to meet his requirements, mainly because that part of it which contains notable distortion is significantly different, in its manner of development and transmission, from the Homeric poetry. Against my distinction of pure and impure oral traditions he replies that we cannot know what happens in a pure oral tradition, simply because, being oral, it is removed from literate study. But the fact is that a genuinely illiterate tradition may have as its subject events recorded elsewhere by other, literate parties; and this allows some control. Thus Finley himself hopes for new Hittite documents to allow us to assess the accuracy of the illiterate Homeric tradition. Moreover the detailed operation of illiterate singers can be studied, in modern times at least, by literates like Parry and Lord. Some distortion of the picture there must be, but not necessarily enough to disguise the main characteristics of a pure tradition. What may be agreed, on the other hand, is that many of the arguments I have brought forward have been a priori in character. They are not to be ignored on that account alone, in a context where so much is hypothetical; and they are not entirely unsupported by factual evidence, like the spread of written chronicles and the intermixture of national traditions on the one hand, and the observed effects of printed songs and histories on Slavic singers on the other—effects which include the corruption not only of the quality but also of the narrative content of their songs.

Let us now look more directly at the Homeric tradition about Troy. The scale of the war is greatly exaggerated, and many Iron Age objects and customs have intruded themselves in the course of transmission; that is admitted. Yet we must constantly bear in mind precisely what Finley’s kind of theory requires us to accept: not merely that the Homeric tradition is distorted, but that it is so severely distorted that its whole picture of a united expedition from the Achaean mainland is false. A small minority of roaming Achaean refugees may have been involved (according to this theory), but the main enemies of the Trojans were northern invaders of whom the Homeric tradition gives not the slightest hint. Now that tradition is not just a vague legendary one; it preserves a great deal of accurate information, not only about Bronze Age social institutions but also about Bronze Age armour, buildings, and people—and some of this information is crystallised in recognisable relics of Bronze Age terminology. I am quite prepared to agree with Finley that the total picture of life given in the Iliad and Odyssey owes almost as much to the circumstances of the early Iron Age as to those of the real Achaean world of the late Bronze Age. Misunderstandings of Bronze Age methods of fighting, especially perhaps over the use of chariots, probably imply that there was no detailed poetical account available to the singers who
developed the kind of poetry that has survived in our Iliad. But can we believe that the interruption of the tradition, whether poetical or non-poetical, caused by the upheavals at the end of the Bronze Age can have been so severe as to destroy not merely the details but the very outlines and whole substance of events belonging to the last heroic period of the Achaean civilisation? The magnification of a heroic past is common enough; how common is the virtual creation of a great heroic enterprise, in this case based merely on the exploits of an assumed expatriate minority, and that at a time when genuine older and indigenous traditions (Thebes, Argonauts, Pylus) evidently still survived? I have emphasised elsewhere that the early Dark Age probably had more to do with the creation of the poetical tradition about Troy than is often assumed. Finley accepts this, and uses it as a means of arguing that the interval of a mere couple of generations at the end of the Bronze and beginning of the Iron Age was enough to allow the kind of distortion over Troy which he envisages. I believe there are grave difficulties in this assumption, and that the parallel referred to in his note 22 is as incomplete, on close inspection, as that provided by the Nibelungenlied and the rest.

It seems highly unlikely that Achaean freebooters could turn in men’s imagination into a great coalition from the mainland, except under one or both of two conditions: first that there was an almost total break in culture and tradition between the end of the Trojan War and the early Iron Age, and second that the distorted version grew up overseas, out of the stream of mainland tradition, where curious unhistorical versions could proliferate unchecked. The first of these conditions certainly did not apply; so much is proved, if by nothing else, by the amount of certifiable Bronze Age information (quite apart from the Trojan War) which descended into the Iliad and Odyssey. The second looks more promising; but one instantly runs into a problem of motive. If the distorted version grew up overseas, it was probably in Asia Minor or the Levant, among the descendants of Achaean settlers there—descendants, most probably, of the hypothetical Achaean freebooters themselves. Yet what motive could they have had for elaborately making out that the attack on Troy was the work, not of their own direct ancestors, or even of those of other expatriate settlers and refugees, but of the mainlanders themselves? Did they wish to connect their ancestors with the main Achaean palaces, now long since collapsed? This looks possible, but too complicated; one would expect such people to exaggerate, rather than disguise, the expatriate achievements and new regional affiliations of their fathers and grandfathers.

A different argument from motive is used by Finley against the Homeric picture of a coalition expedition. Once again it is helpful to be made to see an old difficulty in a new light, but once again I remain unseduced by his objections. First of all, there are many other wars in history whose exact motive the modern historian cannot adequately explain. The Trojan War is not unique in this respect, and does not for this reason have to be abolished. The interaction of long-term motives and immediate casus belli adds to the complication of reconstruction. The Trojan War was probably not ‘commercial’ in the sense of being fought to win or defend markets or continuing sources of raw materials. Commerce may have been something like that in the settled days of the Achaean palaces, but it can have been so no longer by the middle of the thirteenth century B.C. It is not at all sure, however, that a large-scale booty raid is out of the question. Many of the buildings of Troy VIIa were not of themselves impressive, but the huge wall of Troy VI still remained to give the appearance of wealth. It may well have been supposed on the mainland that this wall was not adequately repaired, that Troy was now vulnerable to attack. If the Homeric tradition is right, then this supposition was not completely correct; but that is immaterial. Judging by its imports of Achaean pottery, Troy VI in its later

stages had been a useful commercial partner; Troy VIIa, by the same criterion, was ceasing to be so. But was it really worth attacking? Was it completely drained of all its old wealth, its metals and its horses, so conspicuous in the Iliad? Did the earthquake which ruined Troy VI destroy all of this—or, more to the point, was it known on the Achaean mainland to have done so? Did the earthquake empty the royal graves of precious objects of gold such as had been buried with the kings of Mycenae and Midea and in the rich and numerous tholos tombs of Messenia? It matters not whether there were in fact such graves at Troy (probably there were not); it is the remote speculations of mainland Achaeans that concern us here. To cut a long story short, the rumour may have been widespread in the thirteenth century B.C. that 'Troy is still as rich as anything'; and we cannot even be sure that such a rumour would have been totally incorrect.

Would the prospect of such booty have been enough, in the conditions of the times, to motivate a large expedition from the already threatened palaces of the mainland? In itself, perhaps, not so; there may well have been other reasons too, like the need for the Achaean nobility to restore its prestige or for a failing king to assert his authority. I do not underestimate the effort required: moving a contingent across the Aegean was no easy task, especially if it included horses. Perhaps only a small force went at first, to be gradually increased as a long siege proved necessary. That is just the sort of detail the tradition would simplify. As for the coalition aspect of the expedition, inter-palace ventures were nothing new in the Achaean world, judging from the Argonautic tradition and the south-Achaean alliance against Thebes. Even so, the organisation of a joint force remains somewhat surprising in the probable circumstances. Yet personally I should find it more surprising if the general tendencies of oral transmission in an illiterate society were to be seriously upset, than if events, decisions and political changes in a largely unknown Aegean world were to have been rather different from what we may at present expect. In short, the nature of the tradition about Troy is still a most serious impediment to the kind of reconstruction supported by Finley, unless we water down that reconstruction so much (for example, by arguing that the supposed Achaean freebooters had originally come from different mainland states, and so were a coalition in a way) that it differs from the tradition only in emphasis.

IV. Homer and the Trojan War, by D. L. Page

Many of us have been tempted in our time to look for a connection between the fall of the Hittite Empire, the collapse of Mycenaean Greece, the destruction of certain places in Syria and Palestine, and the assault of 'northerners' and 'sea-peoples' upon Egypt. Mr Finley has now put our hopes or fears into a quite definite form, suggesting that 'northern marauders' (represented in our records solely by the Delta-raiders) were 'a significant, perhaps the main, generating impulse' for all these (and some other) catastrophes; and he maintains in particular that they are likelier candidates than the Achaeans for the prize of Troy. If I disagree, it is mainly because I find the Finley-hypothesis incapable of verification at any significant point, whereas the Achaean-hypothesis is the apex of an assembly of observations which suffice to bear the construction put upon them; though it is salutary to be reminded with so much clarity and force that it is theoretically possible that they might bear the weight of quite different constructions if only we had more bricks to build with.

The evidence of Homer, that Greeks from the mainland sacked Troy (this I call the 'basic narrative'), cannot be proved to the exclusion of other possibilities. That is frankly admitted; and let it be admitted with equal candour, (a) that it has been confirmed by other evidence at certain material points, (b) that no rival account of the destruction of
1. We have learnt in the present generation that the Homeric epic has a continuous history reaching back to the Mycenaean era. Its formula-system includes elements which must have entered it at a quite early phase of that era. A few of these elements can be identified with certainty, and it is prudent to allow that there are others which, for lack of evidence, we happen to be unable to identify. We all agree that the basic narrative has been greatly amplified with fictions; but the fact that the basic narrative may have begun very near the time of the events themselves, and must have begun within a few generations after them, is evidence (I do not say proof) that the basic narrative is trustworthy; for it is unlikely that poets at Court or in the market-place would tell of the participation of the great families on the mainland, if their audience knew (from fathers and grandfathers) that the great families had not participated. We make no claim to proof of anything here, only (as elsewhere) to evidence, unthought of a hundred years ago, in favour of acceptance of the basic narrative.

2. Homer says that Troy was sacked. The world smiled, when it did not sneer. Now Hissarlik has been fully excavated, and nobody doubts that Troy VIIa was destroyed within the Mycenaean period.

3. Homer says that the centre of Achaean power was at Mycenae, rich in gold. This too was surely a fable; few believed it. After nearly a hundred years of excavation at Mycenae and other palaces on the mainland, there is (I suppose) not a sceptic left.

4. Homer says that Troy was besieged by Achaeans. Archaeology has brought the Mycenaeans into very close contact with Troy; indeed it has shown that the Mycenaeans were active throughout the Eastern Mediterranean for a long time, settling in Colophon, Miletus, Rhodes, and Cyprus, trading all round the coast from Troy to Tell el-Amarna.

5. Now come the Hittite documents, and (if we accept the equation of Ahhijawa with Achaia) we can add many touches of colour to the picture of Achaeans and others on the west coast of Anatolia: Achaean kings and buccaneers penetrating quite a long way inland; north of them, a league of Assuwa, including (probably) Troy itself; the Hittites, hitherto keepers of the peace, withdrawing in the late thirteenth century. If Mr Finley has difficulty in thinking of a motive and a historical context for the Trojan War, conflict between commercial (and perhaps imperial) rivals within the vacuum created by the Hittite withdrawal from western Asia Minor will provide one likely enough; and the stage for it is set not by mere conjecture but by the Hittite documents.¹

6. Homer says that Greek forces came from the mainland against Troy. Even for this (the main point of disagreement between us) there is a measure of confirmation in the fact that Homer’s Order of Battle describes the mainland as it existed before the collapse of the Mycenaean world. The Catalogue has been amplified, and the historical picture distorted; but the basic narrative is much likelier to be true than false, if (as we believe) it has its origin in a time when father would have known from grandfather whether it was true or not. Of course I recognise the room for disagreement

¹ Mr Finley writes: ‘The Greek tradition does not ... provide a proper historical context and a motivation for the ... destruction of Troy; bad feeling between two powerful kingdoms is not, historically, an unprecedented motive for war. As for historical context, archaeology and the Hittite documents have filled the gap more than adequately. Mr Finley writes also: ‘Page has conjectured a political struggle between Ahhijawa and Assuwa. I prefer the hypothesis that Achaeans joined a marauding force of northerners.’ This is not so simple a preference as it sounds: my conjecture is based on documents which show Ahhijawans and Assuwas active in contiguous territories in the same period, both warfaring peoples; Mr Finley’s hypothesis postulates a marauding force of northerners for whom, at this time and in this area, there is no evidence whatsoever.
here, but I question the principle of Mr Finley’s objections: he allows his audience to remember the names and descriptions of numerous places which had been unoccupied since the Mycenaean period, but does not allow them to remember whether their forefathers had sailed to Troy or not. In my view you might add to the story (Boeotians and the like), but there must have been a story to add to, and it must not be in contradiction to folk-memory on a point of such magnitude.

Mr Finley says nothing about the Trojan Catalogue; it was an important step in my argument, and I still think it uncommonly solid ground.

The list of confirmations could be extended into numerous details, mainly matters of language and formula; I have done what I can elsewhere. The position is this: our Epic witness makes a number of historical assertions (or implications); we can only prove or disprove a few; on all the most important of these, the witness is reliable. We can confirm other assertions up to a point far short of proof; on almost all of these, the witness is reliable so far as we can judge. Excavation at Troy; excavation at Mycenae and elsewhere; Nilsson on mythology; Parry on oral technique; Hittite documents; archaeological researches throughout the eastern Mediterranean; Order of Battle in the Iliad; detailed study of language and formulas—all have added their mite of proof or confirmation; none has given reason to doubt the basic narrative. And we have still said nothing about what seems to us the huge intrinsic improbability inherent in Mr Finley’s opinion—that an Epic originating within a few generations (at latest) of the events should include or develop into a fiction of the alleged magnitude. In this respect the Chanson de Roland and the Kossovo-Epic are, on Mr Finley’s showing, on our side, not his.

I have already admitted that the evidence is not wholly satisfactory. There is a big difference between acquitting a man of perjury and proving that he has told the truth. There are too many unknown factors. We should be on much firmer ground if we were not so ignorant of the sequences, time-intervals, and other relations (if any; we make no assumptions) between the following events: (i) the collapse of the Hittite Empire; (ii) the sack of Troy VIIa; (iii) the destruction of Pylos; (iv) the fall of Mycenae and Tiryns; (v) the building of Borneer’s great wall across the Isthmus (and was this ever finished?) Marinatos thinks not. If not, was that because the danger had receded, or because invasion came too soon? These are not the only possibilities. Again, who destroyed Pylos? Against whom was the great wall across the Isthmus built? Why was Gla abandoned (if it was) in the generation of its building (whenever that may have been)? We do not know, and it would be most imprudent to postulate a single cause.2 We certainly have no reason at present to believe that any of these matters is an obstacle to belief in an overseas expedition to Troy. We do not know the chronological order of events, let alone the time-intervals. It remains perfectly possible that the overseas expedition antedated awareness of threats to the mainland by five years (or two years or ten years; a very short interval would suffice).

The Homeric account has been confirmed since 1870 to an extent unimaginable before that time. It is very likely the true account; at least it is the only one which can claim the support of various and abundant evidence in both literary and archaeological records. But its claim to our confidence would be much stronger if we could relate it to the catastrophes on the mainland. This being so, is it not reasonable to allow marauding northerners to knock, however timidly, at the door of our conservatism?

Where so little is known, little can be absolutely denied. For Troy and the Greek mainland, there is no evidence whatever of the activity of the Delta-raiders or of any

2 A hypothesis is not necessarily the more probable for being 'economical'; imagine how misleading it would be to apply the principle of 'the most economical hypothesis' to the northern (or eastern) frontiers of the Roman Empire at sundry periods; or to Central Europe during the wars of Frederick the Great; or to the European occupation of Northern America.
peoples directly or indirectly connected with them. The connection of the Delta-raiders with the Hittites is at present so remote and tenuous that the scales of judgment remain quite unaffected. Mr Finley seems to me to be substituting the wholly unverifiable for the partly confirmed. The future may prove him right; the present is not in his favour. If we are to follow him in principle, why go so far afield? The most obvious candidates for the prize of Troy are the Hittites; I could make a good case for them. The next most obvious are the Thracians and their neighbours. Thirdly, for all I know, some proto-Dorians. There is no evidence that any of these peoples had anything to do with Troy VI or VIIa: they have therefore as much or as little right as any 'northern marauders' to gather round our gates. Our present tenants, Homer's Achaeans, may not be quite certain of the legality of their lease; but at least they have a lease, and those clauses which can be inspected have proved valid enough. The inquiry will continue; and those of us who look with disfavour on gate-crashing brethren of the Delta-raiders will do well to remember the words of a colleague of Mr Finley and myself: the historian, like the art-critic, must not forget 'the subtle difference between the ability to make sense and the possibility of understanding'. We claim to have achieved the former; the latter is quite out of our reach.
A FRAGMENT OF NEW COMEDY: P.ANTINOOP.15

(PLATES I–II)

Bibliography

2. T. B. L. Webster, *CR*, n.s. 2 (1952) 57 f.

The fragment is written on the two sides of a leaf of a papyrus codex, dated by Roberts to the fourth century A.D. Our experience so far indicates that so late a manuscript would be likelier to contain Menander than any other author of this genre. Yet the first editor (p. 31) thought the piece ‘un-Menandrian’; and to Webster the ‘tone’ seemed more like that of Apollodorus of Carystus (see *CR*, l.c., 60). The re-examination of the text whose results are now offered was begun after it had been observed that certain passages which might have been thought to rule out Menander’s authorship might have been wrongly read or wrongly understood. After it had been completed, Morel’s article appeared, ascribing the piece ‘mit aller Entschiedenheit’ to Menander. We note with pleasure that at several points our findings concur with his, although we are unfortunately not able to pronounce a verdict with anything like his confidence.

Here is a new text, based upon an examination of the original manuscript, together with infra-red and ultra-violet photographs (the latter reproduced in Plate I) kindly provided by Professor E. G. Turner:

<table>
<thead>
<tr>
<th>(i)</th>
<th>(ii)</th>
<th>(iii)</th>
<th>(iv)</th>
<th>(v)</th>
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<tbody>
<tr>
<td>[ \text{\textit{Xycilipos}} ]</td>
<td>[ \text{\textit{Kantharos}} ]</td>
<td>[ \text{\textit{Gopria}} ]</td>
<td>[ \text{\textit{Philinoc}} ]</td>
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1 An early version of this paper was read by Lloyd-Jones at the Seventh Congress of the Association Guillaume Budé at Aix-en-Provence in April, 1963; a later version to a meeting of papyrologists at the Institute of Classical Studies of London University at Gordon Square in July of the same year. The first editor of the text, Mr C. H. Roberts, and Professor E. G. Turner have both discussed the problems with us; Mr Roberts has lent us photographs and Professor Turner has provided excellent new ones; and at the London meeting an important suggestion was made by Mr John Rea. Barns is the author of the palaeographical part of the paper, Lloyd-Jones of the rest; but all responsibility is shared between us.
Πειναίας

Δείνος - Τις πεννήσες των ιδίων Πολεών

Εμπορίε - Οι Πόλεις Ηπείρου Καιτοπόλεων

Πεμπή - Αγιά Μακαριάνανες το Πατρί

Αφικ - Άγιο Κατανυκτικούς Επόμενα Πέος

Σεμα - (4-5) Το Μετάφρασμον Της Μέλεος

Μίκ (5-6) Ενημερώντος, Απόκοιτον, Πώς Τό Απο (5-6) Ικετεύον Ημέραν Ευμένειαν

Ούνος (6-7) Δοκηγόν - Δικιά

Μετατόπισμανούλι

Δίκαιον Καλοτής - Νικότης - Δικαιοτήτης

Αυτή αποκεφαλίστηκε Παράδοσις Καιβοι

Δεδομένος - Απόκοιτον, Πώς Μετατόπισμανούλι

Τι προσφέρεις σε τον Νικότης Κακάδην

Απαντά (4-5) Παραδειγμάτων Πόλις

Τι θέλεις (4-5) Τι Προιν Ελευθερίας

1. Εν Πρώτω Επίσημων οριζοντιακά μεταφράζουμε τους Παράδειγματος Πόλις

2. Παραγράβοντας, μπορεί να δουμε το Προσφέρεις σε τον Νικότης Κακάδην

3. Αποκρίνεσαι σε τον Νικότης Κακάδην

4. Παραδείγαμα της Μεταφράσματος Πόλις

5. Απαντά (4-5) Παραδειγμάτων Πόλις

1—5. This preliminary material has been divided on the assumption that it consists of the title and other information in the middle (ii, iii) and a list of characters on either side (i, iv).

(i) 3. First letter could be α.

4. First letter certainly κ: then a letter with a long tail, presumably ρ (this seems to suit the traces better than to read it as a letter or sign following α in the next line). Next, what seems to be the bow of α; next, traces of a vertical; next, another vertical (ι;?); next, close to the last-mentioned letter, almost certainly ν. ων at end seem certain.

5. A half letter space to the left of the column below, preceded by a trace of ink, indeterminate, on the broken edge.

(ii) 1. After ε, possibly ρ; before ρ, indeterminate traces; after ρ, what might be read as ο (so Roberts); but trace of a tail below? Last, upright. See p. 27.

2. What Roberts read as a horizontal stroke might be σ; before this, uncertain and almost completely effaced traces of two letters? (Possibly το.)

4. Apparently σε; but possibly traces of ink under the semicircular mark.

(iii) I can suggest nothing here, and do not know how many lines there were.

(iv) Above: σα almost certain.
6. Before τη: traces of a letter joining horizontal of τ; α quite possible. τη: iota adscript added later, as elsewhere.


9. After ωπια: trace of a letter, indeterminate. Before ξ at end, presumably ι with a loop at the bottom; this hand has other examples of ι with such a loop, but in none is it as large as this.

10. The first letter after the lacuna may well be the top of ρ. Next: θ probable. Part of γ after ρ, all visible in the original plate, has subsequently been lost.

11. Trace of a high horizontal (γι, η, or ρ) before ενη. At end, discoloration and damage. Before ποα: arc, probably top of η. After ω: γ, η, or π.

12. After απο: a vertical (presumably τ), then a small indeterminate trace. After κοι: it seems impossible to read anything but ο with a rough breathing above. Then, over a break in the papyrus, apparently η (possibly γ or τ, but I think τ impossible). What might be traces of a letter (or two small letters) added above η. At end, traces of the rounded back of a letter (ι ?) close to δ.

13. No trace of a horizontal at the top joining o after the lacuna (see p. 28). Plate in ed. pr. shows ι complete; lower part lost through subsequent damage. After γευρ: e as well as ι is a possible reading. What Roberts read as a high point could be part of a letter. What follows seems too large for the bow of ρ; more probably ι. To the left of the top of this is a small mark like an acute accent. After: ι: υρ?

15. After αο: ρ rather than i? There seems to be a tiny trace of the lower part of a bow at the top. Next letter: trace of an arc (e, ο, ω ?). After a lacuna of about three letters: trace of bottom of a vertical. After a lacuna of two more letters: first, ε or η apparently joining the horizontal of ρ. Then what might be the bow of ι, followed by a vertical (ι ?). Then another vertical. Last letter: possibly ε.

16. Iota adscripts added, as usual. ηθι: θ no longer visible as in original plate, having suffered subsequent damage.

17. At end: trace of horizontal of letter joining α: γ? After a: γ or π.

18. After μοι: two small traces of ι, δ, λ, or χ. This is closely followed by the top of a tall vertical, probably t.

19. After lacuna: what may well be ι with part of its bow missing, joining ρ, of which the tail is lost.

20. After the lacuna, possibly θ, or, as Roberts says, a circumflex accent above a trace of a letter. Next, loop at the top of a? Then one or two letters; then the top right-hand part of an arc, presumably η. Rough breathing above ι; after this, ι scratched through.

21. In margin, small cursive writing by a fine pen; ρ with small bow and long tail, preceded by a trace of a letter joining it; this will be α or ε. At end, first letter after lacuna could be a μ (so Roberts) of cursive shape, as often elsewhere.

22. At beginning: if αλλα, the second λ is very close to the first. Above, a mark or marks read by Roberts as 'ολ': if the writing at the level of the previous line is the name of a character indicating a change of speaker, we should expect a paragraphs here. Although it is very unlike those on the verso, this is conceivably an ill-formed paragraphs.
I can see no certain traces of the η read by Roberts (see p. 27) in the left margin above; part of the surface may have disappeared since he edited the text. To the right of where we suppose it to be, however, there is a hole, with traces of ink, above the beginning of the first line.

1. Before σ, top of a vertical, preceded by a trace of the top of another; probably ν. At end, traces of a tall vertical, close to α; probably τ.

2. After τροος: trace above σ to the right; β, τ, κ, ξ, ν, or high point? Before δέ: apparently τ (not one letter); but if so, what precedes τ is puzzling; hardly ε. Iota adscript added after ν.

3. Before lacuna, tall vertical (τ or η). After lacuna, the bow of a letter (β or ρ) at the top.

4. After ησ, perhaps nothing but a high point with a fortuitous mark. After ου: two traces of a letter: δ, λ, ξ, or χ? After this, no traces of tails of letters on the next fragment.

5. A thin diagonal stroke of ink in the left margin does not look like a letter, and is perhaps fortuitous. After κα, tail only of a letter, presumably γ, visible below a hole in the papyrus in which its upper rounded part (which must have been rather smaller than in other examples) was lost. First limb of λ not visible. Horizontal of first τ higher than that of second. Letter added above second τ; probably ε. η and σ of χρησις surprisingly widely spaced.

6. After χλαμ.δ, trace of an indeterminate letter, close to the last. At the end, the traces look like ησμα.

7. Note that ω at the end has a cursive ligature with following ρ.

8. First two letters obscured by a stain. At end, probable trace of an apostrophe above trace of right limb of a λ. First δ of οδύναν much crowded, but probably not added.

9. There seem to be traces of an acute accent over the second ε of περιδερα. At end, before final α, trace of an upright (τ ?); certainly no trace
A FRAGMENT OF NEW COMEDY: P.ANTINOOP.15

of a horizontal (such as τ). The tail of a, produced into the margin (cf. 16, 17, below), runs through the two dots of a double point indicating change of speaker.

10. Before lacuna, τε almost certain. A possible trace of the top of a tall upright (ρ?) seems visible after this. After lacuna, either right-hand part of ρ with curved right limb (cf. 10, 11, 17, below), or an apostrophe. The line certainly ends with a with its tail produced into the margin (cf. 9, 11, 17). For the ligature between μ and a, cf. recto 11; verso 16.

11. Before lacuna, traces suggest a rather than o, as read by Roberts.

12. At beginning, o (so Roberts, Fraenkel), ligatured with the following letter, seems almost certain. At end, small, crowded writing: τα.λα.τ? 

13. At beginning, trace above the line, possibly the end of a paragraphos. Before τ??: traces suggest ε or α rather than i. Before this is certainly room for three letters. Before lacuna and after τ: probable.

14. First letter: probably traces of ε. After σ: the space suggests that an apostrophe has been lost in a small lacuna.

15. First, two uprights with traces of a diagonal (doubtless ρ); then bottom of o, with ligature to the right; then the tail and a trace of the bow of ρ; i with curl at bottom, as often elsewhere. After lacuna, traces of a letter touching η (doubtless μ).

18. First letter: an upright (η, τ, ρ, or π). After ωρ: bottom of a rounded letter (β, ε, θ, o, σ, or ω).

After lacuna there are signs of alteration which damage makes it hard to follow. Before μετερων the traces look more like μ.α (cf. Roberts’s note) or μ.η. Above the line there seem to be traces of more than two letters before ζ; it is uncertain whether some traces well to the left of this, above the lacuna, belong to the correction or to the text in the line itself.

19. After θεμή: what might well be ρ; then the top of an arc (θ, o, π, or ρ); then a trace of a small arc joined by the trace of a horizontal (or ?); next, indeterminate traces of two or three letters. Before the next η: a, or ε pointed at the top (as in recto 10, 12, 18; verso 11, 13, 16, 19, 21).

20. After lacuna, a tiny trace of a letter (i?) nearly touching the next, which is unmistakably ξ, followed by ω with a cursive ligature (as in recto 9, 15, 19; verso 7, 12, 15, 19).

21. First letter: upright, with trace of horizontal a little way down (η?). After σ: the trace of a top of a letter (a, δ, η, i, κ, λ, μ, ν, ε, or v).

23. First letter: high horizontal.

Recto

Restored and corrected text.

(i) ε [. ] ρ [. ]
   [ ...]
   [ ... σ]
   [. ] [. ]
   Kρατίνος

(ii) Ε [. ]

(iii) Δ [. ]

(iv) ξα
   Αδαππός
   Κάνθαρος
   Γόργιας
   Φιλίνος
   θεράπαινα

5 ] a

πεμπτ[ό]ν γεγομένα μήνα, πεισθέις τῶν πατρί-
ἀδρ[ής] γ[εγο]μένα, νυκτός—οδ[ε] δέσποινα Νήθ,
σε μάρτυραμεν ἐπάγουμ' ὦ δέλημ γόκο-
μ[ου]ν ἄντις ἡμῶν νίκτ' ἀποκοιτάζ[ε] ποῦ
πούποτ[ε] ἀπὸ τῇ[ς γυναικίν] ழós ὥστε ἦν ἐκεῖν ἔδ.[]
οὐποτ[.. .] αὐτής ἔγονον ὁ ὅκ. οὐκ ...

10 μετά τοῦ γάμους [..]

dικαίων ἔρωτα καὶ στ...[. .] [. .] [εταί. [

a] [γυμναὶ ἔλευθερον γὰρ ἡθεὶ καὶ βίων]
διδάσκοντο τὴν [φίλ]ος τῶν ἦγατ[ων]
τι προσφέρεισ μοι δίκε[ξ] ἱνώνουσα καθ’ ἐν [..]
Supplementa sunt ed. pr., nisi aliter indicatur.
2. 'Fort.'[tos]' Barns (vide p. 22).
4. Ἐράτικος Barns.
6. τὸς Morel: τὸς ed. pr.

VERSO

Restored and corrected text.

?.[.] ης γυναικος
[...]. τροσ [ ] ἄιε τῇ γυ[ τὸ δικτύλοιο [...]] ἐκεῖνης' οὐ[ ]

5 (Α) άνοιξου, εἰ τι καὶ φιλάσττει χρύσιμον[ τῷ ἕδωκε καὶ δῖ[ ]βεβλη[ ]

10 (Α) ἐμοι προσέγγικε τὸν λόχον τε [φαν] ᾠμα. οὐκ [ἐκ]θῆς ἐπιγεγραμμένα [... . . .] δια;


20 οὐ τῶν ἐπιτηδείως ἔχει, µά τὸν Δία, [κ]ητεῖν ᾠσαιντ' οὐχ ἡμέτερον τῇ ἐμποδών[ τα]ραχὴν ἰκανός θείμεν ποτ' ἂν ἔντοσ ποτε

22. al.[ed. pr.]

5. φιλάστει Καὶ.
6. ημῶν Lloyd-Jones (ημῶσης disperit Barns).
7. διασπαραγμένης II.

13. Fort. τ[ριθοὶ, τριθματ'] (Barns).
19. θείμεν ποτ' dispexit Barns (coniceraul Lloyd-Jones).
23. ]σ[ ed. pr.
All who have handled the text so far have followed the first editor in assuming that what we now call the recto is the verso. This is because of the ξα above the list of characters on the top right-hand side of the recto and the ξ (¿) read by Roberts at the top left-hand corner of the verso. ξα stands for ‘61’, and is presumably the number of this page in the codex. Roberts took the alleged ξ to stand for ‘60’, and therefore inferred that the order of the pages was indicated by these numbers. But to the right of the trace read by Roberts as ξ and taken by him to mean ‘60’ is a hole which may well have contained a β, as Roberts now agrees (see above, p. 24). This supposition is strengthened by the position of the figure ξα at the right top corner of the leaf. In ancient codices the page number may be written either in the middle of the upper margin, or at the outer top corner of the page—the right corner of the first side (recto) of the leaf, and the left corner of the second (verso); a position at an inner corner would be most surprising. The conclusion seems almost inevitable that the page on the other side of 61 is not 60 but 62; and we shall see presently that the sense of the text strongly favours this conclusion.

The page numbered ‘61’ contains what seems very like the beginning of a play. But Roberts hesitated to suppose this, partly because of the supposed obstacle offered by the page numbers, and partly because he could see a list of characters only on the right-hand side of this leaf. Five characters, only one of them female, and she a servant, seem too few for the cast of an entire play, and Roberts suggested that the list might contain only the names of the characters who figured in a single act. If that were so, it would be easier to understand how the supposed page 60 could precede page 61.

But we have seen that the supposed page 60 was almost certainly page 62; and the difficulty presented by the list of characters is now removed by the observation, first made by Turner and confirmed by Barns, that the names of characters are to be found on the left-hand side of page 61 as well as on the right. We are therefore free to recognise page 61 as containing what all appearances indicate that it does contain, the beginning of a play. We shall see presently that the content of both sides of the page fits admirably with this supposition.

Speaking of the indistinct traces visible at (ii) 1 of the recto, Roberts wrote, ‘Μ[ε[άρ]δρου cannot be said to be impossible, but could never be read on its palaeographical merits’. We may bear in mind the possibility that this reading is correct (see p. 22 above); but we have no right to use it as a piece of firm evidence.

The letters to the left of ll. 4 and 5 of the recto, read by Barns as Κατάνθαρος | α, were read by Roberts as Κάνθαρος | λα(γει). The name Κάνθαρος is a genuine Attic name (see Kirchner, Attische Prosopographie and Pape-Benseler, s.v.). But in a comedy we should expect the bearer of a name that could mean a kind of bottle to be a bibulous character; Canthara in Terence’s Adelphi is a nurse, and nurses in the New Comedy are notoriously given to the bottle. The speaker of the prologue is clearly not a parasite or any other kind of bibulous character, but a young husband whose situation resembles that of Charisius in the Epitrepontes; and it is a distinct gain not to have to suppose that he was called Cantharus. Further, the indication of the speaker of the prologue by the writing of his name with λα standing for λα(γει) beneath it is unexamplified, and it is a relief not to have to accept it. Κατάνθαρος, the name now read in place of Κάνθαρος, is of course the name of several Athenians apart from its illustrious bearer (see works quoted above, s.v.); it is found in comedy at Asinaria 866 and Adelphi 581, and is the name of one of the Advocati in the Phormio, and doubtless comes from the Menandrian original.

Recto

L. 1. Morel rightly remarks that τις indefinite should be preferred to τις interrogative. If we read τις, the question should properly be answered ‘No one’; if we read τις it should be answered ‘No’, which is the answer actually given.
L. 2. Roberts is wrong to say that ‘the oath by Demeter is normally a woman’s oath’. I have counted thirty instances in comedy without finding one that can safely be attributed to a female character. ‘Bei Demeter schwören die athenischen Männer; sie hat ihre Stelle in dem Bürgeridee’: Wilamowitz on Aristophanes, Lysistrate 271; Roberts is thinking of the oath by the two goddesses.

L. 3. The asyndeton at the beginning of the narrative helps to sustain the effect of excitement already conveyed by the opening question and the unusually emphatic oath with which the speaker himself answers it; at the beginning of the next line the asyndeton is repeated, with iteration of the verb γεγάμηκα. Not only in comedy but also in tragedy prologue speeches often begin with the speaker claiming to be particularly unfortunate and going on to justify his claim, thus helping to put the audience in the picture; among many instances, the openings of the Trachiniae and the Acharnians come to mind. The fact that tragedies also use this kind of opening is not insignificant, for the tone is not only excited but exalted.

L. 4. The invocation of Night helps to sustain this note. Morel is over-specific in seeing here an echo of Hecuba’s address to Night (Euripides, Hec. 68 f.). Electra (id., El. 54 f.) and Andromeda (id., fr. 114 Nauck) do the same; characters in many plays now lost are likely to have done so; and when in the prologue of Plautus’ Mercator Charinus promises not to follow the example of other characters in drama who tell their sorrows to Night or Day, to the Sun or to the Moon, it is absurd to insist that this play rather than any other must have been in Plautus’ or Philemon’s mind.

L. 10. μι[σρον] δ[ι]βην strikes a tragic note. Pfeiffer in making the supplement quoted Aeschylus, Eum. 318 μαρφυες ὅβρα; note also Sophocles, Ajax 354 ὃδ’ ὧς ἔφικας ὅβρα μαρπυεῖν ἀγν. ὦν λέγω λόγον reminds Morel of ὅδ’ ἐγὼ λόγον λέγω in the heroine’s rhea in the Didot Papyrus (l. 10 on p. 143 of Koerte-Thierfelder, vol. i) and of τῶν λόγων ὄν ἀν λέγω in the prologue preserved in the same place (ib., p. 145, l. 2). Even if we were certain that both these texts were by Menander (and in the case of the former, at least, it is not even likely; see now the excellent treatment of W. Bühler, Hermes 91 (1963) 345 f.), we could not be sure that so obvious a turn of phrase might not have been met with in the many comedies once extant that were not by this writer.

L. 11. ἀπόκοιτο[ος]: cf. Epitr. 10 and Lucian, Dial. Deor. 14 (10) 2, both in similar contexts.

L. 12. All have so far acquiesced in the reading ἄσο[ς] ἦν ἐξευτ ἔδει. But the epic, Ionic and tragic word ἄσον would surprise in any comedy; ‘my wife, whom I had to have near me’ makes utterly ridiculous sense; and finally ἄσο[ς] is not in the manuscript. What is in the manuscript is pretty certainly ὃσον. Two letters have possibly been written in above, but they cannot be made out, neither can we think of two letters whose insertion would restore the metre and the sense.

L. 13. Roberts prints ὃσος’ ἀπόκοιτος. But the τ is not visible in the manuscript; and in fact there is no trace of a horizontal at the top joining o after the gap, as there would surely be if τ were right.

In the light of all this we see that we can hardly hope to know whether the part of l. 12 that follows γνωρίζεικάς went with the sentence before it or with the sentence after it. To try to supplement l. 13 or l. 14 would be a waste of time.

L. 15. Roberts wrote δίκαιον ἔρως: but though a Hero speaks the prologue of the Menandrian play that bears his name, he has no place here. It is clear that the speaker was in the situation of the unfortunate Candaules, who according to Herodotus i 8 ἱεράθη τῆς ἐσωτερίκη γυναικός. Morel’s contention that δίκαιον must have agreed with ἔρως in the missing part of the line is too positive; he may be right, but there are other possibilities.

We know no other place in Greek literature where the three words ἔρως, φιλα να, ἀγατή recur at such short intervals, in each case referring to love between a man and a woman, and indeed between a husband and wife.
L. 16. Editors punctuate after αὐτῆς: but delayed γὰρ is common in Menander, and in view of the condition of l. 16 we cannot know whether a pause really fell in this place.

ἐλευθέρως in the sense of ἐλευθέρως is Menandrian; see Wilamowitz on Ἐπίτρ. 147 (his 106, p. 65). Ll. 16–17 remind Morel of Dyc. 764 ό θεοκλάσμων γάρ ἦσεν πρὸς τὸ πράγμα ἐλπίδως, and he regards the resemblance between the two as actually proving that our fragment is Menander’s work. With all respect for Dr. Morel, we find the use of the word ‘proof’ here a considerable exaggeration. We are not so familiar with Dophilus, Philemon and other writers of New Comedy that we can regard a single turn of phrase as giving decisive evidence of Menander’s authorship. Still, ll. 16–17 have a special charm, reminiscent of some of the best things in Menander; and we freely admit that the subjective impression that they looked like his work played some part in leading us to investigate this problem.

The use of δεθεῖς here is rare; it seems to have originated as a metaphor from magic (cf. our ‘enchant’); see LSJ ssv. δεῖ (A), καταδεῖ.

ἀγαπῶ in Menander is used twice of a woman loving an infant (Sam. 32, 63), once of a wife’s care of her husband (ibid., 170), once of a father’s love for a son (fr. 599). The imperfect has inceptive force: ‘I grew fond of her.’

L. 18. It is not safe to guess at what stood at the end of this line. ἄπλως and δλως (the latter suggested by Turner) have both crossed my mind; but in other passages δλως is used with only one word such as ἄποισ or ὑκατος, and perhaps a different sort of word, possibly a noun agreeing with ἄπνεατ, stood in this place.

L. 19 Maas ἀρ. Roberts found the γ’ ‘tasteless and awkward’, so much so that he considered trying to remove it by a violent double change of extreme improbability. τὴν καρδίαν ἄλγω is quite normal, as Morel has pointed out; a limiting accusative of the part affected is common with this word, and is sometimes found even where mental pain is referred to, as at Aristophanes, Lys. 512 ἄλγωσας τάνδοθεν. The words τὴν καρδίαν ἄλγω go closely together, forming a single notion; so that if we read ἐπεὶ ... γε the position of the γε becomes perfectly regular (see the examples of ἐπεὶ ... γε, ἐπεὶδὴ ... γε, etc., collected by Denniston, The Greek Particles a 142–3.) The supplement just fits the space, and seems to us probable.

But who is the person addressed in ll. 18–19? Webster (CR, l.c., 58; cf. Studies in Later Greek Comedy 218) thought it was the Therapaina mentioned in the list of characters, who as we shall see presently is in all probability the person with whom on the verso a male character, probably the speaker of the prologue, is engaged in opening some receptacle. Morel, however, confidently asserts that the person addressed is again the Night.

The speech, he says, is a monologue; and it follows that as Night has been addressed earlier Night must be addressed here also. One might, I suppose, say to the Night, ‘Why do you show me all these things?’ It would perhaps be over-nice to protest that such a question would be put with better reason to the Moon; but can one really say to the Night, ‘Why do you bring me (προσφέρεσ) all these things and show them to me?’ The opening of the speech is indeed a monologue; but there is really no reason why a prologue speech beginning as a monologue should not have near its end (there is probably a change of speaker at l. 21) an address to another actor. Cario begins the prologue speech of the Plutus with an address to Zeus and the gods, but ends it with an address to Chremylus (18–21); the two prologues happen to be just the same length. The person who on the next page is helping a male character, probably the speaker of the prologue, to investigate the contents of some receptacle, seems to be female (see on l. 12 of the verso, below); very likely the receptacle itself is one of the articles that she is bringing and showing him while he is still delivering his prologue speech. The text is defective, and certainty is not to be had; but since it is likely that not more than between 12 and 17 lines separate the portions
of the play on recto and verso, the two characters engaged in both are probably the same, and they are probably the young master and the slave-woman.

About the remaining traces on this page we will say nothing, except to point out the obvious fact that it would be foolish to offer any supplements.

**Verso**

L. 2. Editors have acquiesced in Roberts’ γυνη. This form may have existed in New Comedy (see Menander fr. 937 in Koerte-Thierfelder); but there is no need to suppose that it was written here. If the line ended τη γυνη, it would still have been no longer than l. 6 or l. 12 below.

Several supplements of the first four lines have been attempted; but a minute’s reflection should make it clear that not enough of the text is preserved to make it worth while to try to fill the gaps.

L. 5. Morel rightly points out that the first editor’s reading is improbable, involving as it does a split anapaest; but his own conjecture ει τι καιρος ανδρεις χρησιμον is almost equally objectionable, since it is not at all in the manner of the New Comedy. I do not doubt that Rea’s φολάττει is correct.

L. 6. Barns is sure that ημισεσ was written; to Lloyd-Jones the word looks more like ημασιον. In any case, ημισιοι is a certain emendation; the word may easily have been assimilated to the case of the preceding word.

At Epitrep. 228 and at P. Ghoran i (Page, Greek Literary Papyri, no. 65) 42a, πτερνισ figures among recognition tokens; Pollux vii 42 says that a πτερνισ was half a χλαμις.

L. 9. Necklaces are recognition tokens in the Epitrepontes and Perikeimones, as in Euripides, Ion, περισεκλεις is attested for Menander (fr. 899) and Nicostratus (fr. 33 Kock); but other comic poets doubtless used it, and its value as an evidence of Menander’s authorship is not high. One might wear an anklet on one ankle, but no one wore them on both; so pairs of anklets did not exist.

L. 10. Barns’ reading of the last three letters as ιμα rules out several supplements, and also Morel’s idea that οικ ειςεσ must be parenthetical. The missing part of l. 11 probably contained the names of the objects that were written on something or (more probable) had writing on them; our tentative supplements (see app. crit.) are prompted by the memory of the small model animals that serve as recognition tokens in the Perikeimones (338 ff.).

In the Rudens of Plautus (1154 f.), the recognition tokens include a little sword and a little axe; both have writing on them, the sword bearing the name of the girl’s father and the axe that of her mother. Plautus indicates this by means of the word litteratus (1156); and it is possible, though not certain, that επιεγερμυθε in this passage has a similar significance.

Ll. 14–15. The sight of the letters convinces the man that they have found a child’s recognition tokens; but he is so preoccupied with the present crisis that he locks them up again without further investigation. At the crisis of the play they reappeared, no doubt, with decisive effect.

L. 16. σημανονμαι means ‘I will lock them up’; see Fraenkel on Aeschylus, Agam. 609.

L. 17. έπιτηδειος occurs in comedy after Aristophanes only at Alexis fr. 239, 5 Kock: but note P. Ghoran ii (=Page, Greek Literary Papyri, no. 66) l. 126 άπερεποπτηδειος διακεισει.

L. 18. Why should αφανθ have been aspirated? We suppose it happened by mistake. What is in the manuscript could be accounted for if one supposed that the scribe first wrote αφανθ οικ by mistake instead of αφαντ οικ, then wrote in the χ, and then noticed that it was obscure, all the more for being so far to the right, and added ов before it, a little higher up.

L. 19. Barns reads θειμιν; Webster had already suggested this, but as a conjecture, and as part of an ungrammatical sentence. It must be an optative of wish, so that ποτε is the only possible word that can have followed it. It is not really strange that ποτε should occur twice in the same sentence.
Doubtless a paragraphos should be supplied below l. 19; the sense shows that there must have been a change of speaker after θείμεν [πορτ].

It should now be clear that so far as we can tell the style, vocabulary and metre of this fragment contain nothing inconsistent with Menander’s authorship. There are indeed several passages which seem to favour the notion; we have seen that above the recto Μένανδρου may even have been written; and we must take into account the late date of the manuscript. Our knowledge of New Comedy is so slight that we must proceed with caution, and Morel’s positive assertion of Menander’s authorship goes too far. But it is likelier than not that the piece is his; and future editions of Menander should contain it, if only among the Dubia.

Two other papyri have been connected with this one, and must therefore be examined in this place.

(1) A slip inserted between pages 30 and 31 of the editio princeps mentions that in October, 1950, W. Schubart published as no. 23 of his Griechische Literarische Papyri (SB der Sächsischen Akademie 97, 5) a leaf of a papyrus codex which Roberts took to be the same one from which P. Antinoop. 15 derives. We print below Barns’ transcription of this text, made from the photograph kindly sent us by Dr Wolfgang Müller (see Plate II). But the piece throws no light whatever on the problems of our fragment. The writing is not the same—a view in which Barns and Turner concur; and except for the mention of a person called Cantharus and the attribution of a speech to a Therapaina, there is nothing to link the two pieces. ‘It is certainly not identical with the hand of P. Antinoop. 15’, Barns writes; ‘the differences cannot be accounted for by a change of pen. Although the forms of the letters are for the most part very similar, there are some notable discrepancies.’

(1) Epsilon in A(ntinoop.) sometimes has a cursive form (a); so has it in B(erL.), but not quite the same one (b).

(2) Kappa appears in B in two shapes, one conventional but with a long diagonal final limb descending to the right (c), the other a purely documentary form (d). Neither shape is found in A.

(3) Xi in B is a purely documentary shape (e). A’s is quite different (f). This latter form does occur once in B, but only in a marginal addition, written, as Schubart has observed, by the second hand.

(4) Sigma in A has a tendency to dip down at the top (h); in B it has a tendency to curl up at the bottom (g). I think B is somewhat earlier.’
1. First two letters could be ας or possibly ας.
2. Before δ: possibly ε. Before κ: τ or ρ. After κ:
   ο, ε, or ω.
3. First two letters: indeterminate traces only.
4. First letter might be λ. Before αν: γ, τ, υ, π, ρ,
   τ, or υ. At end, two letters, probably η rather than
   one broad η: ε above correcting it.
5. First letter has a tail (ρ or ζ ?). Penultimate
   letter probably ω.
6. Second letter: η, τ, υ, or π? After that, γ rather
   than τ. Certain. Not το, but το (a faint trace of
   the circumflex accent can be made out), with i added
   above by the second hand.
7. Trace of first letter quite indeterminate.
8. Again the first letter is not to be determined.
9. The accent on η looks more like a badly written
   circumflex than a grave. After υ, high point, or
   double point of which the lower half is lost.
10. First letter looks like δ.
11. First letter: trace of curl at bottom of second
    upright of η visible?
12. First letter: doubtless a trace of the bow.
    of ρ.
13. Traces of ι between δ and ω certain.

   1. εις Schubart; but perhaps only one letter
      before μ. At end, trace of a vertical, high; probably i.
   2. Last letter: τ, υ, or χ.
   3. See p. 33. At end, ω [or o.].
   4. At end, ν or π, followed by a trace (low) of λ,
      μ, π, or χ.
   5. β is certain.
   6. The accent on σφρ is placed far to the right
      because of the paragraphs. The bottom of the σ in
      this word curls up and nearly touches the top, but σ
      seems certain. Possible trace of a circumflex accent
      over υ.
   7. ει very likely.
   8. After ν: o or ω.
   9. ου: accent again displaced by the para-
      graphs.
10. The mark above ρ at the beginning of the line
    looks more like a rough breathing than a para-
    graphs. Almost horizontal stroke over an ρ may be
    a badly written grave accent. Last letter probably υ.
11. Last letter: η or η. After τ or τ.
12. At end: η or η. [?]
13. The horizontal stroke over ττ may be another
    displaced accent. Above ε near the end: probably,
    as Schubart says, ω cancelled.
14. First letter looks more like η than υ. At end:
    possibly trace of α.
15. First letter: not to be determined. At end:
    ι or ν?
16. At end: perhaps η, then λ or χ.
When Schubart prepared this piece for publication, he was cut off from access to the original; and the reader will note without surprise that several readings now offered (e.g., at A 6, 7, B 2, 6 and 18) are intrinsically likelier to be right than his. Webster on p. 5 of the article quoted above gives a text based on Schubart’s transcription together with a few supplements of his own, none of them worth making in view of the lack of evidence. He quotes Roberts as suggesting, apropos of the marginal gloss in l. 4, that ‘οὐκ ἔχεις ἐν the margin may be a stage direction ‘he failed to draw the sword’ and then the sword is drawn and the name reveals its owner as νεόπλουτος’. Such a stage direction would be unlike any known to us, and οὐκ ἔχεις does not mean ‘he failed to draw the sword’. It is simpler to assume that οὐχ ἔκειστε was explained by οὐκ ἔχεις[τε; the use of the present rather than the future in a sentence meaning, ‘Aren’t you going to draw the sword?’ is natural enough, but might easily have been thought to need explanation. We find Webster’s attempt to interpret this fragment in detail too hazardous; but his suggestion that the sword is a recognition token, as in the Rudens (sec p. 30), may well be right. Although not from the same codex, the fragment might possibly come from the same play; but the name Κάνθαρος and the mention of a woman (?) slave provide the slenderest of links; and we must resign ourselves to the obvious fact that there is not enough of it for any inference based on it to have much validity.

(2) Roberts printed as P.Antinoop.16, immediately after the piece that forms the main subject of this paper, the beginnings of 27 trimeters preserved in a papyrus leaf written in a different hand, which he assigned to the late third or early fourth century A.D. We reproduce his transcription:

**Recto**


Morel draws attention to κανθαρ[ in l. 2, and argues that it is unlikely that two comedies containing the name Canthus were extant in the Antinoopolis of the late imperial period. We do not feel sure enough about the frequency of this name, nor about the cultural state of Antinoopolis at this time, to determine how much force this statement would have if we could accept the assumptions on which it rests. And in fact we cannot accept them; for we do not know whether κανθαρ[ here is a proper name, and we cannot be certain that the piece was a comedy at all.

Some features may be thought to point to comedy; e.g., l. 17 ends with ἄσύρης (di[ασύρης Roberts), l. 20 with Ἀφιδιὰ (ἐφιδιὰ Roberts); l. 23 infringes Porson’s Law. Other features might be held to point to tragedy; note ll. 5–6, l. 9, l. 16.
Roberts suggests that at l. 5 f. 'a tragic quotation or parody may be detected'. If so, it would be a quotation or parody very unlike any we know of in New Comedy; but in fact there is an easier way of reconciling the discrepancies. All would be understandable on the assumption that the piece came from a satyr-play; and certain other indications favour this possibility. κάνθαρος, whether as a proper name or possibly as a common noun, would not surprise in such a context; and Σύμος is a frequent satyr-name (Charlotte Fränkel, Satyr- und Bakchennamen auf Vasenbilder (1912), 9, 21, 67), or might be an epithet of a satyr.

The evidence is so scanty that it would be absurd to feel much confidence in any guess about the fragment. But such indications as it offers favour its ascription to a satyr-play; and Morel's arguments for connecting it with P.Antinoop.15 are scarcely worth taking seriously.

Oxford.

J. W. B. B. BARS.
HUGH LLOYD-JONES.

[Correction: θραχτε in small writing in the left margin of line 1 of the verso of the text on p. 31 was omitted in transcription, and the omission discovered too late for correction there.—J. W. B. B.]
RELIGIOUS PROPAGANDA OF THE DELIAN LEAGUE
(PLATES III-IV)

There have been found in Samos boundary-stones, horoi, of the temene of Athena, Ἀθηνῶν μεδεών, and of the Eponymoi and Ion, both Ἀθηνῶθεν. All come from the great plain of Khora which runs between Tigani, the ancient city, and the Heraion. On the ground of these inscriptions alone it has been suggested, and it is commonly believed, that Athens made her conquest of Samos in 439 the occasion for seizing all or part of the plain, and that a tithe of the land seized was assigned for the benefit of the three Athenian cults represented, the remainder being occupied by a party of cleruchs from Athens. The plain of Khora is both the largest and the richest in the whole island. No doubt it was the property of the aristocratic Geomoroi, descendants of the early settlers and presumably to be identified with the oligarchs who had revolted from Athens in 441/0. That their lands should have been forfeit upon their own flight into sanctuary at Anaia would not be surprising in the context of the harsh terms which Athens imposed. Moreover, we have an obvious parallel in the confiscation and division of Lesbian territory after the revolt of Mytilene in 427, and in the allotment of a tithe of the divided kleroi to the gods on that occasion (Thuc. iii 50). Epigraphical evidence for temene confirms that similar measures were taken when a cleruchy was sent to Chalkis in 446.

The purpose of this paper is to consider whether the Samian inscriptions themselves will bear the interpretation credited to them; and secondly, if the answer proves negative, to find an acceptable alternative explanation. The inscriptions have never been published with photographs, and the old texts in facsimile type are unhelpful.

I. Athena

1. Heraion Inv. no. 225: PLATE III a. Local marble, ‘from an old chapel half an hour to the North of the Heraion’.
   (0·56); 0·39; 0·10; 0·023–0·026.
   hopos : τεμενος | Ἀθηναυις | Ἀθενον | μεδεων

2 For the revolt the most important sources are Thuc. i 115.2–117; Plut., Per. 24.1–2, 25–28; Ar., Wasps 281–4 and schol.; SEG x 221, 39 (Hill, Sources B 61–62). For the Geomoroi, Plut., Q. Gr. 57 (Mor. 303E–304C: oligarchs c. 600 b.C.); Thuc. viii 21.
3 Thuc. 117.3; Diod. xii 28.3–4; Plut., Per. 28.1–3. SEG x 304 (including Hestiaia, cf. Thuc. i 114.3); IG xii 9 934 (Hill, Sources B 96 (b)); cf. Plut., Per. 23.4; Aelian, Var. Hist. vi 1. See also ATL iii 297. Compare also the regulations for the foundation of Brea, perhaps in 447/6, SEG x 34.10:
4 SEG x 304 (including Hestiaia, cf. Thuc. i 114.3); IG xii 9 934 (Hill, Sources B 96 (b)): cf. Plut., Per. 23.4; Aelian, Var. Hist. vi 1. See also ATL iii 297. Compare also the regulations for the foundation of Brea, perhaps in 447/6, SEG x 34.10:
5 I am most grateful to Professor E. Homann-Wedeking for giving me permission to publish these inscriptions, and also for providing photographs of them. The information about measurements I owe to the generosity of Professor G. Dunst, who will edit the inscriptions for IG xii 6: I give (in metres) height, width, and thickness of the stones, and height of the letters, in that order. I was enabled to study the horoi in Samos in 1938 through the kindness of the late Professor E. Buschor and of Miss B. Philippaki. Various drafts of this paper, which was delivered to the London Classical Society in November 1962, have been read by Professors A. Andrewes and H. T. Wade-Gery and by Mr Russell Meiggs. I have profited greatly from their comments and suggestions.
2. Heraion Inv. no. 214: plate III b. Local marble (as no. 1), from the Heraion. SEG i 375; M. Schede, Ath. Mitt. xlv (1919) 2, no. 1. 
(0·28); 0·39; 0·125; 0·023–0·026.

[ho]pos τεμενος | [A]θηνας | Αθηνων | μεδεσσης

Nos. 1 and 2 go together. They are of the same material, and their measurements correspond closely. The stone is roughly flattened all over, with a panel especially smoothed to take the inscription. Neither text is stoichedon, though no. 2 comes closer to being so than no. 1. They share the Attic dialect, and the style of their Attic script is similar. Above all, they are united in their use of the form ∧ for delta, a form which is never found elsewhere on stone; and of the least common form of rho, P, rounded and tailed. But there is no doubt that two different masons are at work. Their hands may be recognised in three different letters of the alphabet, epsilon, ny, and three-barred sigma. For epsilon, no. 1 has the centre bar shorter than the other two; no. 2 has three bars of equal length. For ny, no. 1 has the vertical strokes nearly parallel; no. 2 has a perpendicular stroke on the right but the other two strokes almost isosceles. The central stroke of sigma is longer in proportion to the other two, and more sharply inclined, on no. 1 than on no. 2. We are not surprised to find that more than one craftsman was commissioned to carve the horoi. The number required for a temenos of course varied with its size and shape. A fifth-century inscription from Chios providing for the sale of a publicly owned estate named Lophitis lists no fewer than seventy-five horoi defining its boundaries.6

The shared delta, ∧, is a cursive form, more suited to writing with pen or brush, and otherwise almost entirely confined to vase-painting and ostraka.7 Since it is scarcely credible that two separate masons engaged on the same task would quite independently use a form unsuited to their material and indeed never otherwise occurring on stone, it seems to follow that they were provided with a written text to copy, in which, no doubt, besides the cursive delta they also found both the arrangement of the words by lines and the punctuation after hopos. Even so, the delta would not be explained unless we assume that the masons had been specifically told to copy not only the wording but the Attic script also—which they, wrongly, took to include the peculiar delta. But this instruction would only be necessary if the Attic script was unfamiliar to them; that is, if the masons were themselves Samian. Only a non-Athenian might imagine that just as the Attic sigma had three bars, not four, so too the Attic delta had a dot in place of its horizontal stroke.

3. Heraion Inv. no. 136: plate III c. Limestone (poros), from the Heraion. SEG i 376; Schede, o.c. 3, no. 2; CIG 2246.
(0·26); 0·30; 0·16; 0·027.

οπος τεμενος | Αθηνας | Αθηνων | μεδεσσης

4. Once in the chapel of Ay. Nikolaos (1 km. SSW. of the Heraion): missing since before 1924. Similar to no. 3, according to Schede, o.c. 2. V. Guérin, Description de . . . l’Ile de Samos (Paris, 1856) 229, identifies no. 4 with CIG 2246.

οπος τεμενος | Αθηνας | Αθηνων | μεδεσσης

A word must be said about the identity of nos. 3 and 4. There are certainly two, and perhaps three, stones bearing the same inscription. Guérin undoubtedly saw no. 4 built into the chapel of Ay. Nikolaos, where it was rediscovered by Schede, who records it there. Buschor told me that it had disappeared by 1924. Schede followed Guérin in identifying

6 SGDI 5653 (BCH iii (1879) 230 ff.).
7 V. inf., p. 45 f. and nn. 60–62.
the stone with \textit{CIG} 2246. But of this Boeckh notes: ‘In Samo prope Imbramus. Per Rosium misit Gul.Gellius.’ The Imbrasos is the stream which runs through the precinct of the Heraion, and I have therefore thought it safer to regard \textit{CIG} 2246 as no. 3, which Schede found at the Heraion. At any rate, Schede is an explicit witness for the existence of two distinct stones, and for the similarity of their inscriptions in both dialect and \textit{stoichedon} pattern. It remains possible that there were once three stones: that seen by Guérin and Schede in the chapel wall; \textit{CIG} 2246, \textit{prope Imbrasus}; and Schede’s own discovery at the Heraion.

No. 3, and presumably no. 4 also, must be emphatically distinguished from nos. 1-2. They are carved in Ionic script, \textit{stoichedon} but for the last line, with \textit{eta} and \textit{omega} and four-barred \textit{sigma}. The dialect too is Ionic—notice the uncontracted \textit{πεμενος} for Attic \textit{πεμενος(ν)}—but mixed with Attic—notice \textit{Ἀθηνας}. Moreover, the material is not marble but \textit{poros}. It would seem reasonable to suppose that nos. 3-4 were erected on a different occasion from nos. 1-2; although this is not a necessary conclusion, and the evidence of the letter-forms is inconclusive. Certainly no. 3 was not carved by the same hand as either no. 1 or no. 2. Equally certainly, in view of the dialect, the mason was a Samian, not an Athenian of post-Eukleidian date, when Athens had forsaken her own script for the Ionic. We need not be so surprised, then, to find the colloquial form \textit{Ἀθηνας}, where an Athenian would have written the more formal \textit{Ἀθηνας}.\footnote{Cf. K. Meisterhans, \textit{Grammatik der Attischen Inschriften} (Berlin, 1885) 50 f. and n. 490.}

II. Ion

5. Vathy Inv. no. 6: Plate IV a. Limestone, from Tigani.
\((0\cdot41); 0\cdot28; 0\cdot12; 0\cdot025.\)

\[\text{hopos} | \text{τεμενος} | \text{λονος} | \text{Ἀθενας}\]

6. Vathy Inv. no. 5: Plate IV b. Limestone (as no. 5), from Khora.
\((0\cdot44); 0\cdot30; 0\cdot15; 0\cdot03-0\cdot55.\)
L. Philippucci, \textit{BCH} viii (1884) 160.

\[\text{hopos} | \text{τεμενος} | \text{λονος} | \text{Ἀθενας}\]

Both inscriptions purport to be of fifth-century date, in the Attic dialect and script. No. 5 is certainly genuine, no. 6 certainly not. It is convicted upon four distinct pieces of evidence. In the first place, the open ends of the letters are made triangular in shape by means of a cross-stroke. But serifs do not appear at Athens or elsewhere until \textit{c. 330}: they are a feature of the Hellenistic period.\footnote{A. G. Woodhead, \textit{The Study of Greek Inscriptions} (Cambridge, 1959) 64, 91. Mr Meiggs tells me of one contrary example, \textit{IG} ii\textsuperscript{2} 8, dated before 460. Its \textit{litterae magnificae} have little in common with the rough work of our \textit{horos}.} Secondly, the \textit{μι} of \textit{τεμενος} has parallel, not splayed, upright strokes. This form cannot be paralleled on fifth-century inscriptions. It first appears in the third century, and becomes common in the second.\footnote{Ibid. 64; cf. E. S. Roberts and E. A. Gardner, \textit{Intro. to Greek Epigr.} ii (Cambridge, 1905) xvi. Mr Meiggs notes the occurrence of the form on \textit{IG} ii\textsuperscript{2} 400, possibly commemorating Oinophyta, but generally agreed to have been recut.} Thirdly, the variation in height of the letters, \(0\cdot03-0\cdot055\) m., is greater than what is permissible on a fifth-century inscription. Fourthly, whereas no. 5 shows a clear boundary between the smooth and visible inscribed surface and the rough-hewn part that would be buried, no. 6 is smooth for its whole length, even though slightly longer than no. 5. (The other \textit{horoi} preserved to a sufficient length, no. 1 of Athena, and nos. 7-8 of the Eponymoi, have the same distinction between visible and invisible surfaces.)
The peculiar features of the forgery, no. 6, are all possibly Hellenistic—or equally possibly those of a modern forgery. Philippucci’s brief note implies that the stone was newly found in 1884; and an antiquarian forgery of that date seems not impossible in Samos. On the other hand, it is perhaps more likely that the inscription is a Hellenistic copy, made in archaising script as a replacement for a horos which had been lost or damaged.

No. 5 cannot be shown to be by a Samian hand. But that may be to say no more than that the peculiar features of the written Attic pattern were faithfully copied by the mason in this instance.

III. EPHONYMOI

7. Heraion Inv. no. 173: PLATE IV c. Limestone, from the chapel of Ay. Yeoryios, just over 2 km. NNE of the Heraion. 0.98; 0.31; 0.18; 0.029-0.032.

hoσος | τευμενος | Επωνυμων | Αθενετευς

8. Heraion Inv. no. 174: PLATE IV d. Limestone (as no. 7), found with no. 7. (0.69); 0.38; 0.22; 0.029-0.032.

hoσος | τευμενος | Επωνυμων | Αθενεβευν


O. Rayet, Bull. de l’École Fr. d’Ath. xi (1871) 231; Roehl, IGA (1882) 8.

hoσος | τευμενος | Επωνυμων | Αθενηθευν

No. 9 is distinct from nos. 7 and 8. Leaving aside the provenance, a sceptic might trace the different distribution of omicron and omega, epsilon and eta, to careless copying by Rayet; and the missing letters at the beginning of ll. 1-2 could have been lost since 1871: but the presence of the last epsilon certainly serves to distinguish no. 8. No. 7 has only one letter missing (apart from the two never inscribed), and it is the same letter that is lost from no. 9. But, Roehl says, ‘Vs. 2 inter N et O lapidem fractum esse neque repugnare quin ibi litteram E intercidente putes tradit Rayet’. There is no possibility of such a thing on no. 7. Hence we are certainly dealing with three separate inscriptions. They would seem to belong together: the careless inclusion of Ionic among the Attic vowels is common to all three; and all three have R, curved and tailed. No. 9, if the published facsimile is to be trusted, bore curved sigma, S, but otherwise there is no reason to suppose that it differs radically from the other two.

But nos. 7 and 8 are not, I think, by the same hand. No. 8 is largely stoichedon, and the lines certainly begin regularly one below the other. On no. 7 there is no stoichedon pattern, and the lines begin irregularly. The cross-stroke of sigma is horizontal on 7, and slants on 8. Ny is consistently shaped on 7, not on 8. Epsilon is a much slimmer letter on 7 than on 8, and the same is true of epsilon and omega. But the text is the same, with Επωνυμων completely Ionic and Αθενεβευν completely Attic. The sharing of this combination cannot be due to coincidence. Both masons must have copied from a pattern, either a single pattern or one written by the same supervisor, wrongly. But a new fact emerges here. Not only were the masons themselves Samian: the error proves that the supervisor too was not an Athenian.

There are two immediately obvious arguments against the accepted view of these stones. First, the original horoi of Athena and those of Ion and the Eponymoi all have the letter sigma with three bars. Orthodox epigraphical doctrine is that this form ceased to be used
on official Athenian documents in 446, and was replaced by the four-barred Ionic sigma.\textsuperscript{11} Now 439 is not so very long after 446; but the script, \textit{prima facie}, should be earlier than 446.\textsuperscript{12} Secondly, the Ionic \textit{horoi} of Athena are replacements, or at any rate a later addition. Who added them, and when? The accepted answer is, the Athenians, when they were based on Samos towards the end of the Peloponnesian War.\textsuperscript{13} The degree of tactlessness implied seems almost incredible. Even if the Athenians had once confiscated Samian land, the parlous situation in which Athens found herself during the last years of the century would have demanded that the land be instantly returned, to assure the friendship of the one considerable ally remaining loyal. Moreover, as we have seen, the later \textit{horoi} are not Athenian in Ionic script, but partly Ionic in dialect as well as fully so in script.\textsuperscript{14}

To these two grounds for doubt now may be added our conclusion from study of the stones themselves: that the original \textit{horoi} of Athena and the Eponymoi were not carved by Athenian masons; and that the overseer himself in the case of the Eponymoi was not an Athenian—there being no evidence as to the nationality of mason or overseer in the case of Ion, nor of the overseer in that of Athena. Let me again emphasise that there is no other evidence for the establishment of an Athenian cleruchy on Samos in 439. We do not have to choose between associating the inscriptions with the cleruchy or attaching them to some other series of events; we have to choose either to connect them with a known series of events or to construct a hypothetical cleruchy on their evidence alone. With that in mind, we may proceed to examine the wording of the texts, which, with the phrase \textit{'Αθήνας κενόννα} and the repetition of \textit{'Αθήνας}, has not unnaturally contributed to the accepted historical interpretation.

First, the Eponymoi. Their identity should not pass unquestioned. The standard doctrine is that put forward by Roehl (on \textit{IGA} 8): ‘Athenienses devicta insula (Ol.85.1) dedicaverunt locum ... heroibus decem eponymis’—that is, of their Kleisthenic tribes. The repetition of \textit{'Αθήνας} compels us to take the \textit{horoi} of the Eponymoi closely with those of Ion. It is therefore perverse not to identify the Eponymoi here with Ion’s four sons, named by Herodotos as eponymous heroes of the four Ionic tribes, once common to Athens and the Ionians. But at Athens the four tribes had been banished from constitutional usage by Kleisthenes in 508/7,\textsuperscript{15} and the Eponymoi now honoured with statues in the Agora and at Delphi were those of the ten tribes which replaced them.\textsuperscript{16} To an Athenian of the mid-fifth century, the word \textit{'Επώνυμον} on its own would signify the ten heroes of the Agora. If the Eponymoi of our \textit{horoi} are in fact the four sons of Ion, then it follows that the wording then it must have been founded on another occasion.


\textsuperscript{12} This was noticed by (among others) J. J. E. Hondius, \textit{Nov. Inscr. Atticae} (Leiden, 1925) 13 n. 29, who asks, ‘nonne fieri potest, ut Athenienses quosdam agros Samii habuerint, ubi frumentantur? Nam Samus erat statio classic Atheniensium’.

\textsuperscript{13} Schede, \textit{Ath. Mitt. xliv} (1919) 5; dismissing the suggestion of Collitz and Bechtel (on \textit{SGDI} 5701) that the cleruchy of 365 was the occasion.

\textsuperscript{14} The argument would be even stronger if we could be certain that the ‘forged’ \textit{horos} of Ion was in fact a Hellenistic replacement, not a modern copy. For Hellenistic Samos was certainly not of such a temperamen as to keep alive a memorial of her conquest by Athens. If she still kept up the \textit{temenos},


\textsuperscript{16} Agora, Paus. i 5.1; earliest mention in 421, Ar., \textit{Peace} 1183 f. and schol. (in the light of Arist., \textit{'Αθ. Πολ.} 53.4; Andoc. i 83, et al.); perhaps also in 424, Ar., \textit{Knights} 977-80. Delphi, Paus. x 10.1: by Pheidias, from the spoils of Marathon, and so, presumably, part of the same programme as Pheidias’ Athena Promachos (cf. Paus. i 28.1), now securely dated c. 450-50 by B. D. Meritt, \textit{Hesp. v} (1936) 373. (The lettering on two stones identified as part of the base of the Promachos by A. E. Raubitschek and G. P. Stevens, \textit{Hesp. xv} (1946) 107 ff., would suggest a rather earlier date; but the identification is not certain.)
of the inscriptions is non-Athenian; that the horoi were not put up by Athenians, whether cleruchs or conquerors.

In Samos itself, however, the Ionic tribes were presumably still extant. They were part of the Panionian heritage and seem, together with certain other tribes, to have been present in several of the fully Ionian cities. The evidence for Samos is indirect, but nevertheless conclusive: we find the Ionic tribes in the Samian colony of Perinthos, founded c. 600 B.C. There is no reason to think that the Samian tribal organisation had been changed since the foundation of Perinthos; and we find the old Ionic tribes still in existence elsewhere in the middle of the fifth century, for instance at Miletos in 450/49.

The heroes of the temenos, then, are current Eponymoi of Samos, and they are not current Eponymoi of Athens. The natural inference is that the temenos was established by Samians, not Athenians. We have already shown that its horoi were carved and supervised by Samians, and the epigraphical arguments and the argument from identity together seem conclusive.

There is yet a further argument against supposing that the temenos was dedicated to the Eponymoi of Athens. It was τέμενος Ἑπωνύμων—of all of them, that is. A temenos is the property of a cult. But where is the evidence for a common, joint cult of the Athenian heroes? There is ample evidence that they were worshipped individually. Even a cursory search of epigraphical sources discloses the separate cult of at least eight of the ten: of Leos, before 460; of Aias in 429/8; of Kekrops in 409/8 and later; of Pandion, with a sanctuary on the Akropolis, at several dates from c. 400 throughout the fourth century and later; of Erechtheus, before the middle of the fourth century; of Antiochus c. 330; of Hippothoon during the fourth century and c. 288/7; and of Akamas between 300 and 250. The only apparently contrary reference is in Demosthenes’ speech against Timokrates, delivered in 355/2: (xiv 8) ἰδὼν δ’ ἦδυκυκτα... καὶ χρήματα πολλά τῆς θεοῦ καὶ τῶν Ἑπωνύμων καὶ τῆς πόλεως ἔχοντα καὶ οὐκ ἄποιδοντα. But even if this passage implied—and I do not believe that it necessarily does so—that there was a common treasury of the Eponymoi, it would not at all follow that they enjoyed a common cult, any more than the establishment by Kallias’ decree of 434/3 of a common treasury to receive the individual revenues of the Other Gods entailed that they should be worshipped collectively henceforth, or that their lands should pass into joint ownership. Against the passage of Demosthenes already quoted we may set the speech against Theokrines, pseudo-Demosthenic of c. 340: (lviii 14) ἐν τειν ὁδεγεῖ τῇ Ἀθηναίᾳ τῶν ἄλλων θεῶν τῶν Ἑπωνύμων τῶν—‘or to any one of the Other Gods or to any one of the Eponymoi’. I conclude that there is ample evidence that the cult of the ten Eponymoi at Athens was individual, not collective. Hence, a collective cult of Eponymoi is not a cult of the Athenian ten. This argument supports the inference already made, that the temenos of the Eponymoi at Samos was established by Samians, not

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18 J. H. Mordtmann, Rev. Arch. xxxvi (1878) 302 f. (cf. id., Ath. Mitt. vi (1881) 49). The statement in the Eusm. Mag. 160.22, s.v. 'Aerentéka, that Samos was divided into two tribes, Chesiéis and Astypalaiéis, is only true of the Hellenistic period (pace Beloch, Gr. Gesch. i 375).
19 SIG 157.2, Ὀλυσσήων. For the other Ionic tribes at Miletos, argued from their presence at Kyzikos, see Mordtmann, Ath. Mitt. vi 49 ff.
21 IG 2 188.48; cf. Thuc. i 20 for the Leokoreion.
22 IG 2 310.35.
23 IG 3 372.9, 59, etc.; IG 3 1156. 35 (334/3). The Kekropion is restored in the Hekatompedon inscription of 485/4, SEG x 5.10.
24 IG 3 1138.8; 1140.8 f.; 1144.8 f.; 1152; etc.
25 IG 3 1146. Literary evidence, of course, goes back much earlier: Odyssey vii 80 f.; Hdt. v 82.3, viii 55.
26 SEG iii 115.23.
27 IG 3 1149.5 f., 1163.26; cf. Paus. i 38.4.
28 IG 3 1166.
29 IG 3 91 (Tod, GHI 51A); cf. IG 3 310.
by Athenians. The similarity of the wording suggests that whoever was responsible for the *temenos* of the Eponymoi also founded that of Ion.

For Ἀθηναία Ἀθηνῶν μεδεύουσα, I begin by quoting Kirchner’s note on the phrase where it occurs in SIG² 129.10 f. (to which we shall return): ‘I.e. τῆς Πολιάδος. Ita dea nominatur extra Atticam a cleruchs sociise Atheniensium.’ In fact, the only evidence for the use of this title by cleruchs is provided by the Samian *horoi*—and that, for us, is to beg the question. But it was long ago shown by Preuner that the form of the title is basically East Greek;²⁰ and the suggestion that ‘this is Athena Polias looked at from outside Athens’ has won wide acceptance.³¹ Forgetting for a moment the entirely conjectural cleruchs in Samos, the natural conclusion is that the phrase is used by non-Athenians to indicate the deity whom Athenians would have called Polias—that is, the *horoi of* Athena were not worded by Athenians.

However, the identity of Ἀθηναία Ἀθηνῶν μεδεύουσα with Athena Polias has not been universally agreed;³² and the former cult title certainly existed in Athens itself, not merely among non-Athenians. There are three main arguments for identity. The first is based upon two passages of Aristophanes’ *Knights*, which incidentally imply recognition of the title Ἀθηνῶν μεδεύουσα at Athens as early as 424:

581 Ο* Πολιάδες, αἰ μεδεύουσα ἄσων . . .
585 . . . μεδεύουσα χώρας
763 τῇ μὲν δεσποίνη Ἀθηναία τῇ τῆς πόλεως μεδεύουσῃ εὐχώμαι.

The latter passage is particularly telling, the precise title in metrical form; and there can therefore be no doubt that it is meant to be understood in the former, where it appears equivalent to Poliouchos. Poliouchos is the metrical synonym of Polias,³³ to which therefore Ἀθηνῶν μεδεύουσα is equivalent. The second argument is similar. The recently discovered ‘copy’ of Themistokles’ decree passed before the evacuation of Athens in 480 provides, ll. 4–5,³⁴ τῇ[μ] μεν πο[λ]λοι[ς] παρακρ[α]τή[σ]ι[ν], τῇ Αθηναί[ᾳ] τῇ Αθηνῶν[ Mitt.] μεδεύουσα [μεδεύουσα]. The same text is quoted by Plutarch, *Them.* 10. But Aristides paraphrases: τῇ μὲν πόλιν ἐπιτρέψαι τῇ πολισχῶθ θεῶ (i. 226 l. 1 Dindorf). The very least this proves is that by the second century a.d. the title Ἀθηνῶν μεδεύουσα had no separate existence.

The third argument involves an excursion on to dangerous ground, the problem of the relation between the ἄρχαιος νεώς, the παλαίος νεώς, the Erechtheion, and the Old Hekatompedon or ‘Doerpfeld’s temple’, all on the Akropolis at Athens. This is not the place to reopen that old dispute. I take it as more or less established that the ἄρχαιος νεώς ὃ τῆς Πολιάδος mentioned by Strabo (396) and referred to in fourth-century inscriptions is in fact the Erechtheion;³⁵ and that the Old Hekatompedon, if indeed it was ever repaired after 479, certainly did not survive entire after the completion of the Erechtheion.³⁶ In
406/5, according to the chronographical interpolator of Xenophon (Hell. i 6. 1) δ' παλαιὸς
τῆς Ἀθηνᾶς νεῶς ἐν Ἀθηναίς ἐνεπηργήσῃ. Whether this temple was the Old Hekatompedon,
if it still stood, or otherwise the Erechtheion itself, it is certain that the Erechtheion
was damaged by the fire. For the building accounts of the temple for 405/4 refer to τὸ νεόν τα
κέκαλαμενα.37 In an Athenian decree honouring the koinon of the Eteokarpathians,
probably to be dated between 394 and 390, we read the reason for the award:38

οτὶ εδοσα[ν]
[τῷ κυπαρίῳ τον επὶ τον νεόν]
[ν τῆς Αθηνᾶς τῆς Αθηνώμ με-
[εδοσαν]

It is suggested that the temple of Athena for which the cypress trunk was given is the
Erechtheion, damaged a decade previously but so far unrepaired owing to the exigencies
of the intervening years.39 Indeed, since there is no evidence that the Parthenon
was damaged, and since the Old Hekatompedon, if it was in fact the παλαιὸς νεῶς burnt by the
fire, was certainly not rebuilt as a temple of Athena,40 the Erechtheion is the only
temple on the Akropolis for which the timber can have been given.41 But the cult title of Athena
in the Erechtheion was Polias. It follows that Polias is equivalent to Ἀθηνῶν μεθονοσ of
the inscription.

Against the conclusion that the titles are synonymous, Rumpf has advanced an objection
which seems extremely cogent at first sight, and which, when it was put forward, appeared
unanswerable.42 In the decree proposed by Themistokles before the battle of Salamis, as
it is quoted by Plutarch (Them. 10), the Athenians decide to evacuate the city, leaving its
protection τῆς Ἀθηνᾶς τῆς Ἀθηνῶν μεθονοσ. According to Kleidemost (FGrHist 323 F 21),
quoted by Plutarch in the same passage, as the Athenians were making for Salamis the
gorgonion fell off the statue of the goddess, which they carried with them. The statue, we
must agree, is that of Athena Polias. Obviously, there is a conflict here; for it is not
sensible to leave the city in the safekeeping of Athena Polias, and then remove her to
Salamis. Rumpf resolves the conflict by concluding that the Athena in whose keeping the
city was left is not Polias; that the latter title cannot be equivalent to Ἀθηνῶν μεθονοσ.
New evidence has come to light, the inscription from Troizen which purports to be a copy
of Themistokles’ decree. The surviving stone seems to have been inscribed early in the
third century and no doubt derives from the decree known to Demosthenes in 344
(Or. xix 303). Its wording at this point is the same as that quoted by Plutarch. But what
we have certainly does not represent an unaltered text of the year 480. At the very least,
it has passed through more than one stage of literary working-over; by some it has even

37 IG ii² 1654: for the date, W. B. Dinsmoor,
Harv. St. suppl. i (1940) 173–5 (superseding his
earlier arguments, AJA xxxvi (1932) 143–60).
38 SIG² 129 (Tod, GHI ii 110) II. 8 ff.: for the
date, Foucart, BCH xii (1888) 153–61; cf. Dinsmoor, AJA
xxvi 155 f., citing other authorities.
39 Dinsmoor, op. cit. 155–60, cf. Harv. St. suppl. i
179 n. 2, dates the inscription c. 377/6. Even if he
were right, our argument would not be affected; for
Dinsmoor agrees that the temple is the Erechtheion,
in which he detects signs of repairs c. 375 (AJA
xxxvi 170–2).
40 The western end may have been preserved as the
treasury known as the Opisthodomos: see Dinsmoor,
op. cit. 307–26. We need not here examine this
theory.
41 A. Rumpf denies that the temple was on the
Akropolis: JDAI li (1936) 65–71. He identifies it
with the παλαιὸς νεῶς; then argues that since any-
things there which could be called παλαιὸς had been
destroyed by the Persians, a παλαιὸς νεῶς must
necessarily be elsewhere. But the adjective might
well be attached to a repaired pre-Persian temple.
In any case, even if Rumpf’s argument seemed
c conclusive, it would still depend upon a conjecture,
that the temple for which the Eteokarpathians gave
the timber was in fact the παλαιὸς νεῶς: we only
know that it was a temple of Athena; and that the
Erechtheion, also a temple of Athena, had been
damaged by fire. For Rumpf’s other main argu-
ment, see below.
42 Op. cit. 68.
been condemned as a forgery. Hence we cannot assume that the phrase which we are considering stood in an original decree of Themistokles. Professor Meritt has shown, however, that a version of the decree had already been constructed and published by 424, and that this version then contained the cult title ‘Δηλων μεδεωνα: its existence is implied by the context of Aristophanes, *Knights* 763 (quoted above), where the Paphlagonian (Kleon) prays to Athena by that name in a scene during which he compares his own achievements with those of Themistokles (810–19). The precise date of the construction is not discoverable; but the fact that it is certainly not an original document of 480 completely destroys the cogency of Rumpf’s objection. It might even be argued, conversely, that in view of the fact that the statue was taken to Salamis the identity of the two titles proves that the opening words of the surviving decree are unhistorical.

With Rumpf’s objection removed, it seems clear that Polias and ‘Δηλων μεδεωνα are in fact the same. We are left with the question why the Athenians should have employed two alternative formal titles for the same aspect of their goddess. The best answer is that provided by Preuner’s evidence: the longer title was composed in the East Greek region, and only afterwards adopted by Athens for use in certain circumstances. It would not be consistent still to maintain that the establishment of the cult outside Athens was made by Athenian command, for in that case Polias would be the title given.

It does not immediately follow that the East Greek composers of the title were Samian. Paton records in Kos a *horos* (Fig. 1) inscribed *stoichedon*:

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horos τευματος Aθηνας Aθηνα | μεδεωνας
HOPΩΣ ΙΛΜ
ΦΝΟΣΑΩΗΝ
ΛΣΑΩΗΝΩΝ
ΜΕΔΕΟΣΗΣ
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The wording is almost identical with that of nos. 3–4 in Samos. But this is merely to say that both follow a conventional form which appears commonly. Foucart’s suggestion that the stone was carried from one island to the other must not be entertained. He compared it with *CIG* 2246 (our no. 3). But the dialect is wholly Attic (here the parallel is rather with our nos. 1–2); the stoichedon pattern is different; and, a conclusive distinction, the sign H is made to do duty for both eta and the aspirate. This last phenomenon is found at Athens even after the adoption of the unaspirated Ionic script, in such ancient formulaic phrases as this. Hence historical explanations of the *horos* have been constructed on the assumption that its date is late in the fifth century: for instance, it has been associated with the activities of Alkibiades. But the angular rho, P, accords ill with such a date. The form was obsolete by c. 445; and there is no evidence that it belonged to the middle of the century.

At this period, four-barred


44 B. D. Meritt, *Greek Historical Studies* (Cincinnati, 1962) 21–34, esp. 27–30. I am grateful to Professor Meritt for sending me a copy of these lectures.


46 W. R. Paton and E. L. Hicks, *Inscriptions of Cos* (Oxford, 1891) 160 no. 148, whence my Fig. 1: o·90; 0·32; height of inscription 0·22. As at Samos, the lower part of the stone is left rough.


48 Cf. for instance *IG* iii 2 2545, 2555, etc.

49 Paton and Hicks, *op. cit.* xcv f.; see also, for a different interpretation, G. E. Bean and J. M. Cook, *BSA* lii (1957) 124 ff.
Attic. Why should these features of the script not be native Koan of the Pentekontaetia? The dialect, to be sure, is Attic; but the Samian horoi of the Eponymoi afford an exact parallel for the combination of Attic dialect with a mixture of Attic and local script: they too use eta and omega; their sigma is Attic, as is the rho at Kos. Little is known of the local script of Kos. Apart from KOΣ, ΚΩΙΩΝ, and ΚΩΣ on the coins, there is apparently only one published inscription which can be dated earlier than c. 400, a horos Aπολλωνιος Πιθω, of c. 450. This stone and the coins together show that Kos used omega and four-barred sigma at this time. The island belongs to the Doric hexapolis, to the other members of which we must look for the probable nature of the Koan script. Angular rho is not found after the end of the sixth century. But in the fifth we meet a form which combines curved strokes with an angular outline, which could conceivably be what Paton saw. However, it is more likely that our rho is an Attic form. Certainly eta was used. Most significant of all, Rhodes made the same sign serve for both eta and the aspirate: we have a Rhodian painted inscription of c. 450 on which the aspirate takes the form H, and an incised inscription of c. 475 using this sign for eta. There is no feature of the Koan horos of Athena which forbids us to consider it a local inscription of the mid-fifth century. The dialect is Attic; but Ἀθηναίας is a colloquial form not found in Athenian inscriptions before the fourth century. A Koan might readily make such a mistake, if he had heard Athenians talk about their goddess, but had not seen her name on stone. If the Koan inscription is contemporary with the Samian horoi, there is no longer any possibility that we are dealing with a cult of Athena Polias exclusive to Samos among the allies of Athens.

At this point we should mention the horoi of Athena from Aigina. They are not fully comparable, however, since they carry no cult epithet. Four examples are recorded (IG iv 29–32); at least three of them are of marble, in one case said to be Pentelic. They all read horos | τεμενος | Ἀθηναίας, except that no. 32 has the form Ἀθηναίας. The forms τεμενος and Ἀθηναίας are not Doric, and the wording is therefore not Aiginetan. Clearly, it is Attic. The horoi have usually been associated with the establishment of an Athenian colony following the expulsion of the Aiginetans in 431. But the script is not easy to reconcile with this date. No. 29 is fully Attic, with ʔ and Ρ; on the others, rho has the form Ρ, and sigma has four bars. In his note on IG iv 32, Fraenkel attributes the form Ἀθηναίας to an Ionic mason. The same influence could be responsible for the four-barred sigma. But it is to be noticed that no. 32 also bears the form Ρ, long since rejected by the Ionians: hence Fraenkel’s conjecture should be abandoned. The four-barred sigma could well be Attic, at any date from the mid-fifties onwards. It is at least equally possible that it is due to local Aiginetan influence, also responsible for Ρ, and that Ἀθηναίας represents an Aiginetan’s mistaken conjecture about the Attic dialect, which so often changed Doric alpha to eta. It may be conjectured, though in the absence of the distinctive title it cannot be proved, that the Aiginetan horoi reflect the establishment of the same cult of Athena as do those from Samos and Kos. Here, as in Samos, an effort was made to employ the

51 Ibid., no. 39, pl. 69; Herzog, Koische Forschungen (1899) 69 no. 36, pl. 21.
52 Jeffery, op. cit., 345 ff.
53 Cf. BMC Caria, pl. 34.12, Kamiros c. 475–50; Jeffery, op. cit., pl. 69 no. 41 l. 3. Halikarnassos c. 475.
54 Ibid., 350, 357, pl. 68 no. 30.
55 Ibid. 357 no. 27; JHS vi (1885) 372 f. and fig. The sign □ is used for both eta and the aspirate in Jeffery, op. cit., pl. 67 no. 5.
56 Thuc. ii 27.1: M. Fraenkel, IG iv ad loc.; cf. G. Busolt, Gr. Gesch. iii.2 (1904) 936 f. There was also on Aigina a tenemus of Apollo and Poseidon, jointly. The boundary stones (IG iv 33–8) are inscribed horos | τεμενος in Attic script and dialect, with the words Ἀπολλωνιος Ποσειδωνιος added at a later date in Ionic. I propose to discuss this tenemus in a separate paper.
57 Cf. Jeffery, op. cit., pl. 17 nos. 19, 21 (both c. 450 ?), for both letter-forms. It should be noticed that Ρ is an early form of rho in Aiginetan (cf. for instance IG iv 61 (Jeffery, op. cit. 113 no. 8), probably c. 550–500), and that Ρ and Σ occur together on the tombstone of Choiros, IG vii 71.
Athenian dialect and script (the latter perhaps only on no. 29); here, too, the effort was not wholly successful. Even if this interpretation is wrong, as it well may be, we must affirm that the inscriptions do not permit the date 431 to be attached to themselves. The alternative is to suppose that they record the seizure of land by Athens after the conquest of the island in 457. In this case, of course, the script (including four-barred sigma) would certainly be Attic.

We may summarise our arguments so far, in the form of five propositions.

1. The temene at Samos cannot have been consecrated by the Athenians in 439, for (a) the Eponymoi are the Eponymoi of Samos and the Ionians, not those of Athens, and their temenos at Samos was marked out by local masons working under local supervision; (b) the temenos of Ion goes with that of the Eponymoi in whatever explanation we seek; (c) the title 'Αθηνών μεδέονον is a synonym for Polias, of East Greek invention, and the horoi of Athena therefore bear East Greek wording; (d) the original horoi of Athena were cut by local Samian masons, and later replacements were both worded and cut by Samians.

2. The temene of Athena and the Eponymoi were probably instituted at the same date; for their horoi (apart from Athena's Ionic replacements) offer no stylistic basis for a distinction between them.

3. The horos from Kos proves that the cult of Athena 'Αθηνών μεδέονον is not peculiar to Samos among Athens' allies.

4. The title 'Αθηνών μεδέονον was recognised at Athens as a synonym for Polias to be used in certain circumstances at least as early as 424 (The Knights), and probably as early as 447/6, if it is rightly restored in the regulations for Kolophon, dated to that year.58

5. Since this title was adopted by Athens from East Greece, its invention there must be earlier than the date of its first occurrence in Athens.

To these five conclusions we must add that the headquarters of the cults of Ion and the Ionic Eponymoi must have been at Athens—as, of course, was that of Athena Polias. That this must be the meaning of 'Αθηνή—'at Athens'—seems clear from a horos τεμενον της Κεφαλής, from Keratea in Attika (IG ii2 2604): Keratea has been shown to be the ancient Kephalē,59 and Isaios, indeed (π 31), speaks of Aphroditē Κεφαλής.

The only hypothesis which will meet the case is of a series of common League cults voluntarily set up at the suggestion of the members of the Delian League, and having their headquarters at Athens. The inclusion of Ion and the Eponymoi is a reference to the tradition that all Ionia was settled from Athens at the end of the period of the Dorian invasions. An obvious suggestion would be that the cults were instituted at the same time as the League itself. But our evidence must at first be epigraphic and stylistic, an examination of the Samian horoi.

In the first place, since it is now demonstrated by non-epigraphical arguments, that the inscriptions do not belong to the year 439, their universal adherence to the three-barred sigma may be allowed to give a terminus ante quem in 446. The popularity of dotted delta can be followed on Attic vases.60 Beginning towards the end of the sixth century, it was

58 SEG X 17 (ATL ii D 15 l. 14) ἄθηνων μεδέονον (see Meritt, op. cit. 38 n. 36, for the reading). N. C. Comnaris, Klio xi (1962) 49, proposes a similar date for the Themistokles decree, on literary grounds.

59 RE 'Kephalē', following U. Koehrle, Ath. Mitt. x (1885) 110, who argued from funerary inscriptions, cf. IG ii2 6345 etc.

60 Cf. J. D. Beazley, AJA lli (1948) 336; P. Jacobsthal, GGA cxxv (1933) 10; P. Kretschmer, Gr. Vaseninschr. (1894) p. 96 sect. 74 and n. The form first appears at the end of the sixth century; cf. a b.f. olpe in the Villa Giulia belonging to the Leagros Group (ABV 378.251; Mingazzini, Vasi della Coll. Castellani pl. 85.1); also a r.f. neck amphora in Vienna, Oest. Mus. 319 (ARV 13.7). It was especially popular with Douris in his middle period, c. 495-80: ARV 279 ff. transcribes Α from 23 vases, Δ from six, but records the form on no vase of Douris' earlier or later periods. Fergusons of his work wrote ∆ORI, evidently recognising the delta as
much favoured during the period c. 495-480. It remained popular with one school only down to c. 430, but not later. It is found on a few of the early ostraka from the Athenian Agora,61 and occurs once on bronze during the fifties.62 Rho rounded and tailed begins in the time of Kritos and Nesiotes. As a common form it belongs to the fifties and forties, and last occurs in the final accounts for Pheidias’ statue of Athena Parthenos, 438/7.63 Ny with perpendicular strokes came into fashion in the fifties. I conclude that the stylistic criteria, taken together, favour a date in the fifties, and would permit a date in the early forties.

The tradition of Ionia’s Attic foundation may go back at least as far as Solon (Fr. 4.2); and it was certainly credited in Ionia as well as in Athens by c. 500 b.c.64 It was made the excuse for the founding of the Delian Confederacy (Thuc. i 95.1), and was particularly emphasised in the middle of the fifth century, having recently formed the subject of an epic by Panyassis.65 Presumably in the fifties, as part of the programme of dedications to which the statue of Athena Promachos belonged, the Athenians sent to Delphi a group of statues by Pheidias.66 There were two gods, Athena and Apollo for donor and recipient, and the victor of Marathon, Miltiades; there were the ten eponymous heroes of the Athenian tribes;67 and three more heroes: Theseus, who had been too much identified with the unification of Attica as a whole to be made hero of any one tribe; and also Kodros and (by emendation) Neileus, the main characters in the Ionian tradition. In 454/3 Athens was worried by the revolt of some Ionian cities, Miletos and Erythrai for certain, and possibly others besides. After the revolt, for a number of years from 453/2 onwards, Athens sought to regain and hold the doubtful loyalty of Miletos by supporting a government there which consisted wholly or partly of the Neileids, a clan which claimed to number the lineal descendants of Neileus, son of King Kodros of Athens, who had brought the first Milesians to Asia from Athens.68 There is evidence that the religious implications of the tradition

a peculiarity of his script: Cartellino Painter, ARV 297 f.; Triptolemos Painter, ibid. 241.27 and note on p. 239. The relevance of Douris to our horoi was observed by Preuner, op. Schede, Ath. Mitt. xlii (1919) 2. Among works of c. 490–480 by other painters we may notice a woman’s head vase in Rhodes (ARV 867.9); a cup in London by the Brygos Painter (E 65; ibid. 247.13); another in Boston by the Tyszkwiczewicz Painter (ibid. 185.1); a stamnos in Vienna by the Argos Painter (ibid. 176.1); and a pelike in Berlin by the same artist (ibid. 176.5). Rather later, c. 470, is a lekythos ‘related to the school of Douris’ belonging to M. Henri Seyrig (Beazley, AJA lxxii (1948) 336 ff., pl. 34). The form reappears on stamnoi of Polygnotos and his Group: Oxford 1916.68 (ARV 677.3); Gotha 51 (ARV 677.5); London E 454 (ibid. 678.8); London E 456 (ibid. 695.8); London E 450 (ibid. 689.1); Villa Giulia (ibid. 696.16); Boston 95.21 (ibid. 666.18). I owe much of this information to the kindess of Sir John Beazley, Mr Meiggs, and Professor E. Vanderpool.

63 Ig i 354 (on the date, Dinsmoor, ’Esp. ’Apy. 1937, 507). Some examples from the fifties and forties: Alliance with Segesta, 458/7 (IG i 20—IG i 29 has R, a misprint—A. E. Raubitschek, TAPA lxixv (1944) 10 and n. 3); Sigeion decree, 451/0 (SEG x 13; Hesp. v (1936) 360 ff. and figs.); sporadically and irregularly in some early tribute lists (heading of IV, 451/0; V ii 34, 450/49; VII iv 34, 448/7; not later); Brea, 447/6 (IG i 45; SEG x 34); here and there in the Chalkis decree, 446/5 (IG i 39; ATL ii pl. x); horoi of Hippodamos’ replanning of the Peiraueus, probably before 443 (AJA xxxi (1932) 254 ff.).

64 J. P. Barron, JHS lxxii (1962) 6 and n. 40.

65 Suidas, s.v. ‘Panyassia’; cf. F. Stoessel, RE. Hdt. i 146 may be an attack on Panyassia’s account; cf. also Hdt. ii 45 with Panyassia F 26 Kinkel, ap. Athen. 172d, on Bousiris. In both places, however, Herodotos may be attacking Pherekydes, not Panyassia.

66 Paus. x 10.1: s. sup., n. 16.

67 When Pausanias saw the group, it contained Antigonus, Demetrios, and Ptolemy, but only seven tribal heroes; presumably the newcomers had replaced them, retaining the original base.

were exploited c. 450, recently examined by Meritt and Wade-Gery.69 Athens laid a special duty upon her colonists: this is shown by three fourth-century inscriptions relating to Paros, Priene, and Kolophon, in which we find that these cities had the privilege of sending a cow and a panoply to Athens for each celebration of the Great Panathenaia on the ground that they were colonists of Athens.70 The same privilege was accorded to the new colony of Brea, perhaps in 447/6,71 and is mentioned in the decree of Kleiniias, probably of the preceding year,72 but had not been invented by the time of the Athenian regulations for Erythrai, 453/2.73 Meritt and Wade-Gery conclude that this privileged obligation for colonists of Athens was invented between 453/2 and 448/7. They believe, further, that storage of the panoplies was the original purpose of the Chalkotheke, a building whose foundation is independently dated c. 450.74 Finally, they suggest that the possibilities became clear to Athens after the transfer of the treasury of the League from Delos to Athens in 454/3, a Panathenaic year; but that the allies themselves were anxious to enjoy their right of sending a cow and a panoply, symbolising food and military assistance for the mother-city. It is clear that on historical, as well as epigraphical, grounds this general date would be most suitable for the voluntary establishment of the three cults which likewise recall the colonisation of Ionia.

The colonisation propaganda, as we may call it, was re-emphasised by Athens a generation later, when she was more than ever dependent on her allies for help against the Peloponnesians, and it is instructive to compare this evidence with that for the earlier period. As the first step, in 426/5 Athens reconstituted the old Ionian panegyris at Delos.74a Only a few months before the production of Aristophanes' Knights, with its use of the allies' name for Athena Polias, a decree of Thoudippos passed probably in the fourth prytany of 425/4 extends the obligation or privilege of sending cow and panoply to the Panathenaia to all cities who paid tribute to Athens.75 They are all to enjoy the special place of a colony in Athens' heart: πεμπτοντες [θε η α] τη Πολη [τις πιανετε] ωθεω την καθεστη εστι [θαπειρ] αυτου (ll. 57–8). After the passing of Thoudippos' decree, but before it first took effect in 422, Aristophanes perhaps referred to the glut of beef at the Panathenaia.76 A few years later, we have a decree of Adosios dated 418/7 providing for the fencing of the hieron of Kodros, Neileus, and Basil, and the letting of the temenos of Neileus and Basile77—apparently a distinct entity from the hieron, as Wycherley has recently emphasised.78 The decree is to be recorded on stone and set up (ll. 27 f.) ev τοι Neileοι παρα τα ικρα. The word ικρα commonly means 'constructional scaffolding' (LSJ). Austin here takes the phrase to mean that the inscription is to be set up beside a shed where some permanent set of scaffolding is housed.79 Wycherley rejects this, but himself prefers to think of some sort of grandstand.80 Surely the natural interpretation is that a shrine of Neileus is in building, but that so far the scaffolding is the only landmark visible on the site. Five years later, in 413/2, the imminent defection of the remaining allies after the Sicilian disaster caused Aristophanes to emphasise

69 JHS lxxvii 69–71.
71 IG ii 45 (Tod, GHI ii 44) ll. 11 f. On the date, see ATL iii 286 n. 49.
72 SEG x 341–3; date, ATL iii 281, 299.
73 IG ii 10, 11, 12/13a (Hill, Sources B 26); date, ATL ii 57, ii 254, following the suggestion of R. Meiggs, JHS liii (1943) 34–
76 Tod, GHI ii 66 (IG ii 63; ATL i 154 f., cf. ATL ii 40 ff., A 9) ll. 54–8.
77 Clouds 386 f. and schol.; cf. Meritt and Wade-Gery, op. cit. 70 and n. 10.
78 IG ii 94, surmounted by a relief of an old man seated and a younger man on horseback, presumably Kodros and Neileus. R. E. Wycherley, 'Neleion', BSA lv (1960) 60–6, referring to previous discussions.
80 R. P. Austin, JHS li (1931) 287 ff.
their status as colonies in the *Lysistrata* (582). Lastly, we have the *Ion* of Euripides, generally agreed to be a late play, but never conclusively dated. The purpose of the play is to make the allies proud of the empire. In the closing scene it is Athena, herself ἐπώνυμος ... σῆ ... χήθων (1555), who comes to proclaim to Ion (made an Athenian in the play, 8 ff.) that his four sons shall be Eponomoi of Athens, and that their descendants shall colonise the islands and both coasts of the Aegean, the source of Athens’ power, and shall be called Ionians (1575–88).

It is into this propaganda, at the time of its earlier use, that the Samian horoi of Athena, Ion, and the Eponomoi, should properly be fitted. At this juncture the removal of the treasury to the safekeeping of Athena Polias marked her adoption as the League’s chief patron in place of Delian Apollo: natural, then, that she should be honoured with a temenos, as well as the Athenian heroes of Ionia. It is not without significance that the only two genuinely original instances of the official use at Athens of the title Ἀθηνών μεδέων seen for Polias, on the inscriptions relating to the Eteokarpathians and to the Kolophonians respectively, are both in contexts concerning Athens’ allies, her real or putative colonists.

One last fact may suggest a more precise reconstruction. The cults were set up at the instance of East Greeks: we have argued that from their wording. But who were these East Greeks? The only place where we have evidence for the existence of all three cults is Samos; and indeed otherwise only Kos (and perhaps Aigina) provided evidence for any of them. Plutarch tells us, *Arist*. xxv 3, that the proposal to move the treasury from Delos to Athens was made by the Samians themselves. As newly appointed patron of the League, Athena Polias received an aparche of the annual tribute immediately from the outset of the new arrangements. The Samian motion in the League synod must therefore have provided for the payments to Athena as well as for the transfer of the funds to her keeping. The proposal to consecrate temene to her throughout the empire, together with similar honours for heroes who were alike forebears of Athenians and Ionians, may have been a third clause in the same motion, and may have included a direction to use the Attic dialect and script. The Samians no doubt obeyed their own motion at once; their allies partially, or not at all.

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83 The Ionic horoi of Athens in Samos may perhaps be associated with the renewed emphasis on the legend during the Peloponnesian War, or may be an assurance of solidarity when Samos was Athens’ base at the end of the war.

84 Kolophon, as an original Ionian colony; the Eteokarpathians, tributary members of the Delian League, adopted as colonists by Thoudippos’ decree: *ATL* i 157 l. 130 with 155 ll. 54–8.
85 According to Plutarch, the proposal was made to Aristeides, who was of course dead by 454. I follow *ATL* iii 262, against Gomme, *Comm.* i 370 n. 2, and also against R. H. Dundas, *CR* xlvi (1933) 69.
'Thirdly we love-sick youths introduce also a cottabus to take its stand for you here in the gymnasium of Bromius, as a punch-ball. All you who are present entwine your fingers in the thongs of the cups (i.e. the cup-handles which serve as thongs); and (?) before fixing your eyes on it (the target), you should measure by pacing with your eyes the air high above the couch, and estimate the area over which the wine-drops are to extend.'

These lines are quoted by Athenaeus (668c) in his celebrated account of the Sicilian and Athenian after-dinner game of cottabus, a pastime which has long been a matter of academic controversy. In several points text and interpretation are not clear. In general, it is an elaborate jest in which a cottabus party is described in vocabulary appropriate to the gymnasium and its pursuits, the dining-room itself being called the γυμνασίου Βρομίου. Instead of Hermes, the god of athleticism, it is Dionysus who is the presiding deity; instead of the punch-ball (κώρυκος) which would be prominent in the gymnasium, it is a cottabus-stand which the competitors propose to labour. In such a context I am surprised that

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1 In two honore (Garzya, loc. cit. infra, 201). Doubtless he is right in taking the dative of the person for whom the δυσέρωτες are pining, and for whose favours they are competing. For a discussion of the word δύσερωτες see R. M. Ogilvie, JHS lxxxii (1962) 107-8, who, however, (like LSJ) does not refer to this passage, which vies with Eur. HIPP. 193 as the earliest citation. I suggest that the notion of obsessive, but frustrated, love (the prevalent usage in the Anthology) is the basic meaning of the word.

2 The reading of VL κατακλησιν is not, I suppose, impossible—'the downward-sloping air', almost as vivid an expression as the ὀρθῶν αἰθέρα of Eur. BACCH. 1073—since the wine-drops had to be projected with such skill as to fall in rapid descent from a point above the plastix, like a howitzer shot. The height attained—reaching the αἰθήρ—is humorously exaggerated, as in Antiphanes, fr. 55, 10-20.

3 On cottabus in general I have found most useful O. Jahn, Philol. xxvi (1867), 201 ff.; K. Sartori, Das Kottabosspiel der alten Griechen; L. Becq de Fouquières, Les Jeux des Anciens 212 ff.; A. Higgins, Archaeologia li (1888) 383 ff.; B. A. Sparkes, Archaeology xii (1960) 202 ff.; on the Dionysius elegiac fragment also F. Buecheler, Neue Jahrb. f. Phil. cxi (1875) 125; A. Garzya, RFIC xxx (1952) 193 ff. I am also grateful to Mr. D. A. West of Edinburgh University for his comments in the preparation of this article.

4 So too the wine-drops in the game are called ἢραξείων τοξεύματι (Eur. fr. 562), Βρομίου γυμνασίου (Critias fr. 8. 10). Cf. Anth. Pal. xi 59. 1 (Macedonius) αὐθητήρες ἔκθετον.

5 I do not understand why Garzya (loc. cit., 203), referring to Jahn, is under the impression that it is the form of game δι' ὀξείδωρον which Dionysus is describing here. Aiming at a target is common to both the main types of cottabus, but only the familiar κατακλησιν κότταβος with its rod, plastix and lekane bears the appropriate visual resemblance to the κορυκος of the gymnasium. The verb ἔσταται also suggests the setting up of this apparatus (cf. Ar. fr. 209 ἐγονοκ' ἐγονο μηλικίαν ἔσταται καί μερρίνας). The comparison with a punch-ball here may explain a statement of Pollux (vi 109-10), repeated in schol. Ar. Pax 1242, but not, so far as I know, confirmed by vase-paintings nor at all probable in itself, that the cottabus-stand was suspended from the ceiling (see however, p. 51, n. 17). For his wording τὸ μὲν κότταβος ἔκρωμα ἀπὸ τοῦ ὀρθῶν... καὶ τὸ μὲν ἐκ τοῦ ὀρθῶν κρεμμύδιον ἔρημον ἐπικοπταίσαντα πούμαι τινα γόρον, ὡς καλείται λάτας, cf. this description of the κορυκος in Antyllus ap. Orib. vi 33.3. κρεμάται δὲ ἐν τοῖς γυμνασίοις ἄποθεν ἐξ ὀρθῶν. Curiously Pollux goes on to mention a σφαιρα as part of the impedimenta of the cottabus δι' ὀξείδωρον, and it is tempting to ascribe this error (?) also to some
the ϕαῖραι of I. 4 have been incorrectly explained, or even called into question. One need not suppose that cup-handles were ever called ϕαῖραι (or σφεῖραι in Jahn's emendation), but they are humorously likened, for the purpose not of drinking but playing cottabus, to the leather thongs (ιμάτες) which boxers twisted round their fingers and wrists before taking part in bouts both in contests, and in the gymnasia, and which are frequently illustrated in palaestra scenes on vases. We know that in a developed form, in which the four fingers were bound together above the first phalanx by a raised ridge or pad of hard leather which encircled the hand, these thongs were called ϕαῖραι, and that they were used particularly in preliminary training, protecting both the knuckles of the user and the person of his opponent from unnecessary damage, while at the same time allowing both to hit out freely as if this sparring were 'the real thing'. The position of such ϕαῖραι around the boxer's fingers is compared by Dionysius to that of the fingers crooked in the handles of the κυλικας as the young men 'square up' to the cottabus—punch-ball.

The following words καὶ πρὶν ἐκείνων ἰδεῖν are puzzling, and perhaps corrupt: the only possible meaning is that which I have set out in my translation, with ἐκείνων = the κόστασαν—κόρυκος target. Before aiming (or, as we might say, 'sighting') the thrower must estimate the distance between himself and the cottabus—the αἰθέρα κατὰ κλίψιν, and the trajectory required. Yet such a survey can hardly be said to exclude sight of the ultimate target (cf. Nonnus's description, 33. 92–3, of the heavenly cottabus contest in which Eros ἀπλανεῖ δημα παντάσα | εἰς σκοτών ἴκόντεσιν ἔκβιολον ἴκῳδα πέμπων), and the expression itself is more feeble than the allusive style in which, both before and after, Dionysius describes the game in athletic imagery. The analogy with boxing, however, seems now to be finished, since βηματικός is used of pacing out an area or distance, and would naturally refer in a gymnastic context to the preparation of the ground for a throw (e.g. of discus or javelin), or alternatively to the measurement of a throw which has been made. The perfect tense ἐκτάταται, if correct, implies the latter. For the preliminary measuring of an area for a contest in throwing one may compare Hom. II. iii 315 (cf. ib. 344) χῶρον μὲν πρῶτον διεμέτρεο, and—for a race-course—the two τέρματα which Achilles marks in II. xxiii 757. In view}

of Aristomenes (fr. 13 ap. Poll. iii 150)—if indeed this is the play mentioned in IG xiv 1097. 10 ἐξὶ Διοφάντῳ Διονέτῳ (see Dittmer, Fragments of Athenian Comic Didascaliae 45): otherwise the play could be much earlier in the long career of Aristomenes, who began exhibiting about 431 (Suidas). The theme of the play—'Dionysius in training' (in the palaestra)—suggests a humorous situation similar to that described by Dionysius Chalcus.

One might expect a preposition with ὃς in this sense; but cf. the charioteer in Hom. II. xxii 323 αἰεί τέρμα ὅροιν.

And contrast the unsuccessful javelin throw in Ach. Tat. ii 34 πρὶν ἀκριβῶς καταστάξασθαι τοῦ σκοπῶν.

There seems to be no literary evidence for how throws were measured. Vase-paintings sometimes show rods being used apparently for this purpose (cf. καρυὶ in Poll. iii 151), but Gardiner (Greek Athletic Sports and Festivals 475), referring to a r.f. vase in the Canino collection (fig. 173) suggests that the boy holding a javelin pictured in the interior of the cup 'seems to be measuring the ground with his feet, perhaps measuring the throw'.

of the use of ἐκτείνειν by Dionysius, one may also observe that Homer continues τοῖοι δὲ ἀπὸ νόσους τέτατο δρόμος, but the meaning ‘the running track was marked out’ has not found the approval of most commentators, who prefer the version ‘their running was strained’.

That an area was marked out restricting the general direction of a javelin throw (involving distance rather than aiming at a mark) is implied both for competitive purposes at the games and practice in the gymasia. For the former, cf. Pindar Pyth. i 44 ἔξω ἄγωνος βαλεῖν (and the metaphorical use in e.g. Lucian, Anach. 21), and schol. Nem. vii 71 τὸ ἀκόντιον παρὰ τὸ ὄρισμένον τέρμα βαλλόντως; for the latter, cf. Antipho ii 2.4 τὸ ἀκόντιον ἔξω τῶν θρόνων τῆς αὐτοῦ πορείας . . . ἐξενεχθεῖν. Here then, the cottabus player is said to measure the prescribed extent of his throw by eye, and we are reminded that in the ultimate refinement of the game special rooms were designed, as one might today construct a squash-court, to ensure that the competitors had a regulated distance over which to throw the λάταγες. In the last line of our fragment however, it seems that, since ἐκτέτασα could only refer to a cast already made, Buecheler’s ἐκτείνειν must be preferred.

The throwing of the wine drops in cottabus playing is regularly compared to the casting of some missile, either arrows (Critias fr. 1.2 λατάγων τόξα, Eur. fr. 562 Βακχίου τοξεύμασι) or a sling-shot (Antiphanes fr. 55.19), but most commonly a javelin. The propriety of this particular simile is enhanced by the way in which the javelin was normally held and thrown by means of the ἀγκόλη (Lat. amentum), a thong wound round the shaft at or near its middle, into which the index, or index and middle fingers were inserted. The rotary movement imparted by the unwinding of the thong, like the rifling of a gun, added both length and accuracy to the throw. The close similarity of the position of the finger(s) in the thong with that of the finger(s) in the handle of the κύλις in cottabus may be seen by the comparison of any vase-painting depicting cottabus scenes with the illustrations of the javelin grip in Gardiner’s article in JHS xxvii (1907) 257. The same verb ἐνείπεσ is used by Dionysius to describe the κύλις in his elegies is found of the javelin in Hesych. s.v. ἀγκολίσθαι: τὸ ἐνέιπε τοὺς δακτύλους τῇ ἄγκολῃ τοῦ ἀκοντίου. Indeed it seems to me probable that the frequent occurrence of ἀγκόλης, ἀγκολιστός, etc., in cottabus contexts, where it is virtually a technical term, derives from the finger position (which in Antiphanes fr. 55.15 is compared in another appropriate image to the fingering of the aulos) and wrist action, rather than from the bending of the arm, as it is usually taken. Most of the passages quoted by Athenaeus and the lexicographers are susceptible of either interpretation: Plato Com. fr. 47 ἀγκολισθάσαν δε οὐδέρα | τὴν χείρα πέμπτες εὐθριόμιο τῶν κόταθον (where, however, Athenaeus, 667b, paraphrases further from the play, saying that Heracles is directed μὴ αἰχμαλώτατον ἐχειν τὴν χειρα, not to hold his wrist stiff); Cratinus fr. 273 ἀτ' ἀγκόλης ἐπονομάζοντο αὖμα | ἠριά λάταγας (where Kock proposes ἀποκοντιζοντο); Bacchylides fr. 18

13 Although the scholiast is wrong about τέρμα here—see Gardiner, JHS xxvii (1907) 268.
14 Athen. 479d (=schol. Luc. Lex. §) πρὸς δὲ τούτους οἷς κατασκεύαστε κυκλοτερεῖς ἵνα πάντες εἰς τὸ μέσον τοῦ κοταθοῦ δεδέντες ἐξ αἰσθητῶς ἱερῶν καὶ τῶν ἄμεσον ἀγκολίσθατο περὶ τῆς νίκης. Cf. id. 668d, 792 f.
15 Non iaculatori sunt sed iaculatori (Buecheler).
16 In addition to the passages to be discussed, ἀκοντίζειν occurs in Nonnus 33.65 and 93, schol. Ar. Pax 1244. Cf. also Ach. Tat. iv 18 ἀκοντίζει κατὰ τὸ στοάτο τὸ τόμα καὶ τευχάτε τὸν αἰθέατο.
17 Headlam (CR xix [1905], 397) talks of ‘a loose wave of the elbow!’ Vase-paintings generally (but not invariably) show the arm bent. A quite different explanation of the term is given by A. Minto, Studi Etruschi xviii (1944) 83 ff., viz. that the Sicilian form of throw (now represented by a scene incised on a pebble found at Castiglione del Lago, which shows some features untypical of Athenian vase-paintings, including apparently a hanging target) was known as ἀτ’ ἀγκόλης from the position of the κύλις poised sul dorso del polso della mano. (Incidentally the target—if it is one—does not look remotely like a lamp, as he suggests.) W. Deonna, Un divertissement de table ‘à cloche pied’ (Coll. Latomus xl (1959) 20-1) offers some criticisms of Minto’s interpretation of the scene.
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... the meaning of ἄγκυλη and cognate words of the cottabus throw is Aeschylus's description of the missiles thrown at Odysseus in his Ὀστολογοί (fr. 179) as ἄγκυλητοι κόσσαβοι. The only other literary use of ἄγκυλητον (as a substantive) is also in Aeschylus (fr. 16), where it means 'a javelin thrown by the ἄγκυλη', ἄγκυλωτος being similarly used in Eur. B. 1205. Indeed every verb derived from ἄγκυλη\(^{20}\) is confined to a meaning connected with javelin or other thongs, with the exception of the explanatory notes of Athenaeus on the use of the term in cottabus, which I consider erroneous—διὰ τὸ ἀπαγκυλών (ἐπ.—schol. Ar. P. 1244) τὴν δεξιὰν χείρα ἐν τοῖς ἀποκοκτησμοῖς (667c, cf. 782c, Hesych. s.v. ἄγκυλη, etc.). Athenaeus is unreliable on these words, for in the latter passage, as Gulick correctly observes in his note, he undoubtedly gives the same wrong derivation of ἄγκυλητά and μεσάγκυλα of javelins, and his belief that ἄγκυλη is a kind of cup is also erroneous. Although the word may on occasions mean ἱκαμπή τοῦ ἄγκυλου (Hesych., etc.), this is not the reason for its constant use as a term in cottabus,\(^{21}\) which derives from the similarity of the grip and 'twisting' movement of the fingers\(^{22}\) to the javelin throw—and it must be remembered that the average Athenian young man had no lack of practice in both pursuits, a fact underlined by another comment of Athenaeus (479e, quoting Hesych. of Delphi) ὅστε ἐνοικοδημοποιεῖται κατὰ τὸ ἐπὶ τῷ καλῷ κοσσάβῳ τῶν ἐπὶ τῷ ἀκοπτικῇ μέγα φρονοῦντων.

It was not without reason therefore that Headlam, in the corrupt last line of the Ὀστολογοί fragment of Aeschylus referred to above, proposed to emend (—ὁς) ἐκτεμῶν [sic] to ὁς ἐκτεμῶν\(^{23}\)

... ἣν μὲν γὰρ αὐτῷ ἀκοπός ἀεὶ τούτῳ κάρα, τοῖς δὲ ἄγκυλητοις κοσσάβοις ἐπίσκοπος, ὡς ἐκτεμῶν, ἡμῶν χεὶρ ἐφίετο.

—although I should correct his translation 'bent-armed throws, as of javelins' to 'thongs-tossed throws'.

In view of the almost ubiquitous references or allusions to javelin, spear, or bow shots

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\(^{18}\) There is much to be said for the variant ἐντεῖνουσα, the reading of CE in the citation in Athen. 782e (ἀντεῖνα) is the reading of A at 667c: ἐντεῖνουσα πίθων would have a subsidiary meaning from archery, and combine with ἀπ' ἄγκυλη with precisely the same mixture of imagery as in Soph. OT 204–5 quoted above and Nonnus 33,92–3 τανάσσας . . . ἡκόντισεν (referring to a cottabus throw).

\(^{19}\) For ἄγκυλη δαίζων I had thought of διάγκυλοι, but the parallel use of ἐντεῖνασθα in Sophocles suggests the text is sound. Minto (loc. cit.) accepts Bergk's παιζων: Page, who takes δαίζων to be corrupt, mentions—lust<3>παιζων in his note ad loc.

\(^{20}\) Cf. LSJ s.v. ἄγκυλοι, ἄγκυλη, διάγκυλοι, ἐνάγκυλαι, ἔναγκυλαι, ἔνεγκυλαι, ἔνεγκυλα, ἔγκυλωω, ἐγκυλωω, ἐγκυλωω, ἐγκυλωω, ἐγκυλωω, προσγκυλώω.

\(^{21}\) An interesting subsidiary use of ἄγκυλη (Poll. iv 196) is as a medical term for a callous hardening μάλιστα ἐν δακτύλωι χειρῶν κατά τὸ ἐντός . . . ἐπικάμπτει δὲ τούς δακτύλους. Cf. Ar. H. 356b 17 ἐπικάμπτει τὸν δάκτυλον 'to bend the finger to an angle', precisely the characteristic finger-position in the thong of a javelin or the handle of a κύλις in cottabus. It may have been a common ailment on both accounts!

\(^{22}\) The use of συνεστραμμένου of cottabus and javelin throws alike is further evidence of the similarity of throwing action: Athen. 666c λατάνη δ' ἐστι τὸ ἐπολευμένον ἀπ' τοῦ ἐκθέοθεν ποτηρίου δ' συνεστραμμένη τῇ χεὶρι ἀνόιξεν ἑρέττων οἱ παίζοντες εἰς τὸ κοτάς; Hesych. s.v. ἄγκυλη χεὶρ: ἄγκυλομαζέ η καὶ συνεστραμμένη εἰς ἀποκοκτησμοῖς; Plat. Proto. 342e ἐφεξῆ ῥήμα . . . βραχύ καὶ συνεστραμμένον ὅσπερ δενός ἄκολοτης. Cf. also Eust. 203.22 (explaining ἄγκυλομαζέ) διὰ τὸ κατ' αὐτὸν ἄγκυλον καὶ συνεστραμμένον, and (with reference to ἐπικάμπτει in previous note), Hesych. s.v. ἄγκυλοι, ἐπικάμπτει; schol. Ar. P. 904 κομποι: συναιμεῖα.

\(^{23}\) CR xix (1905) 397–8, where he seeks to justify this meaning of ἐκτεμῶν on the analogy of ἀποστομάς, ἀποτομή, ἐκτεμῶς = ἀποστόμον πεντάδον. Cf. also his article in CR xiv (1900) 8.
in the small corpus of passages relating to cottabus, even those which are otherwise innocent of any particular association with athletics, I am brought to the conclusion that Dionysius, in likening the paraphernalia and activities of the palaestra to the game of cottabus, uses firstly imagery from boxing (κόρυφος, σφαίραι), and then passes to javelin-throwing, the connexion of thought being motivated, in the allusive and riddling manner which is a feature also of the other extant fragments of his verse, by the notion of inserting the fingers in the handles of the kylikes, which recalls to him not only the thongs of boxers, but of javelin throwers (frequently shown side by side in fifth century vase paintings of palaestra scenes). The words καὶ πρὶν ἐκείνον ἱδεῖν should therefore conceal some explicit reference to throwing. Of emendations proposed, Bergk's ἀπ' ὅλων ἱεῖν lacks one of the terms in the gymnasium-symposium comparison; Edmonds' ἐκεῖσε δικεῖν is possible—δικεῖν is an uncommon verb and properly used of a discus throw, but cf. Pindar, Isth. ii 35 μακρὰ διακήσαις ἀκοντίσαμι. I have thought of ἀκοντί' ἱεῖν or ἀκοντα δικεῖν which would give the appropriate sense, but are not too close to the MS. reading.

As almost every word in these lines is chosen carefully to suit the palaestra imagery, it is to be observed that the function of τρίτων in the first line alone remains unmotivated. Edmonds' 'third kind of cottabus' is improbable, and although only the wider context of the complete poem could explain the point, my guess is that Dionysius has been travestying other gymnastic pursuits in similar fashion. A glance at Aristophanes Pax 894 ff. will show how use may be made of Greek athletic terminology (less decorously, however, than in the elegant riddles of Dionysius) to describe a party for νεανία μεθυσκόταβοι.

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PALLADAS AND THE NIKAI

PALLADAS' attitude to Christianity has been much discussed in recent years. It is not of purely academic interest whether he was a pagan or Christian, for Christianity did not remain 'the beggars' religion', and it is the conversion of just such a figure as Palladas, a schoolmaster steeped in the classics but scornful alike of the traditional gods and the Platonism that had taken their place for the intellectual pagan of his day, that marks a vital stage in the christianisation of the Roman Empire. For nowhere did the traces of paganism linger longer than in the University circles of Alexandria. However the arguments adduced by P. Waltz to show that it is 'infiniment probable que Palladas était chrétien' carry but little conviction, and it seems much more likely that Palladas was and remained a pagan. The following poem, though overlooked or misinterpreted in most recent discussions, can perhaps be made to cast a little more light on the matter:

Νίκαι πάρεσμεν, αἱ γελώσαι παρθένοι,
νίκας φέρουσαι τῇ φιλοχρήστῳ πόλει.
ἐγραμμα ἡμᾶς οἱ φιλούντες τὴν πόλιν,
πρέσποντα Νίκας ἐπιποιοῦντες σχήματα. (xvi 282)

φιλοχρήστω Planudes (Marcianus 481): φιλοχρήστω Lascaris (editio princeps, 1494), edd.plerique.

'Here we are, Victories, the laughing maidens, bearing victories for the city that loves Christ (א). Men painted us who loved the city, carving the symbols that are proper for Victories.'

First a textual point. Did Palladas write φιλοχρήστω or φιλοχρήστω? There are, in fact, some slight external grounds for preferring φιλοχρήστω. Johannes Lascaris printed it in his editio princeps, and it has been conjectured that he brought with him from Constantinople Themistius, xi 292. ix 400, usually taken as showing that Palladas drank in philosophy at the feet of the beautiful and learned Hypatia, has been shown by Luck (op. cit. (n. 1) 452–7), in spite of the objections raised by Irmischer (Eirene-Kongress Plovdiv, 1962; not yet published), to be neither by Palladas nor about Hypatia.

4 Compare the stir caused by conversion of the schoolmaster/philosopher Marius Victorinus in Rome (Augustine, Conf. viii 11): a century after Palladas the conversion of another Alexandrian grammarian, Horapollon, was regarded by his ex-fellow-pagans as desertion (Suidas s.v. 'Εραπόλλων).


7 See especially Luck, op. cit. (n. 1) 457 f.

8 And Cod. Brit. Add. 16409, a direct apograph of the Marcianus, and written only a few months after it (Douglas Young, Parola del Passato x (1955) 197 f.).
and used for his edition a manuscript independent of and probably earlier than Planudes' Anthology. It is hard to think of a word more liable to corruption at the hand of a Byzantine scribe whose emperor was regularly hailed as φιλόχριστος than φιλόχριστος, and the fact that the pious and prudish Planudes wrote φιλοχριστός should not therefore inspire much confidence. It is of more significance that Lascaris, in spite of the fact that he also was a Byzantine, preferred what must have seemed to him the lectio difficilior. Yet the principal objection modern scholars, working from the wholly inadequate and misleading article in Liddell-Scott-Jones, have raised against φιλοχριστός, is that the word is not found before the sixth century A.D. This is quite without foundation, for it is in fact extremely common in patristic and hagiographic writings from the fourth century on. Hence Irmischer, after pointing out that the two words would by Palladas' day have been quite indistinguishable in pronunciation, and that φιλοχριστός might naturally be expected to have given way completely in the Tempora Christiana to a phonetic twin such as φιλόχριστος, has recently championed φιλοχριστός. His argument is sound enough and he is probably right, but we should not ignore the possibility that if Palladas was 'ein Gegner des Christentums', he might have felt himself at liberty to use φιλοχριστός regardless of its twin, perhaps even knowing that to the ear it was indistinguishable from φιλόχριστος. It must be remembered that the ancients read aloud, and no one familiar with Palladas will deny that the pun is the most striking single characteristic of his style; at ix 169.4 he indulges in just such a pun on the phonetic identity of λογός/λόμας, and perhaps another on καυσόταφο/κενοτάφο at viii 684.4. We know that pagans regularly said Christus for Christus, and the pun itself is abundantly attested in early Apologetic, even by inscriptions. In any event Palladas did not, like many of his pagan contemporaries, ignore Christianity completely in his writings, and whichever of the two words he did in fact write, in view both of their phonetic identity and the crisis the struggle between Christianity and paganism had reached

9 Cf. H. Beckby, Anthologia Graeca i (1957) 75: 'Merkwürdigerweise stimmt aber diese editio princeps keineswegs mit dem Marcianus völlig überein. Es sind nicht nur in der Anordnung der Gedichte, sondern auch in der Textgestaltung gewisse Unterschiede vorhanden, die den Marcianus als direkte Quelle ausschliessen'.

10 E.g., Luck, op. cit. (n. 1) 438 and n. 21. Since any occurrence of a word like προφήτης will inevitably be in a Christian context, it is hard to understand or justify the decision of LSJ to exclude Christian writers but include Christian inscriptions, thereby giving the quite false impression that the word is not found at all before the age of Justinian.

11 A number of examples are cited by J. Irmischer, Studien zum Neuen Testament und Patristik: Erich Klostermann zum 90 Geburstag dargebracht (Berlin, 1961) 323 f. Doubtless that the new Patristic Lexicon reaches the letter ϕ it will reveal many more instances, but for the moment add to Irmischer's list Gregory Nazianzen contra Julianum i 24 (PG lv 552), and nine occurrences in the probably fifth-century life of St. Porphyry of Gaza by Marcus Diaconus (69.2; 70.12; 75.3; 75.8; 76.14; 79.4; 84.6; 93.13; 103.6). Many instances from papyri collected by Preisigke, Wörterb. d. griech. Pap. p. 202; see also below, p. 56.


13 Keydell, op. cit. (n. 1) 1.

14 A fairly complete list of Palladas' puns is to be found at RE xviii 3.167 (W. Peek), and W. Zerwes, Palladas von Alexandri, Diss. Tübingen, 1956 (typescript) 368-9. On reading aloud see J. Balogh, Philologus lxxxii (1927) 84 f., 202 f.

15 As noted by Irmischer, Wissenschaftliche Zeitschrift d. Humboldt-Universität, Gesellsch. u. sprachwiss. Reihe vi (1956/7) 165.

16 Suggested by Teresa Bonanno, Orpheus v (1958) 140 n. 66.


20 Libanius, for example studiously avoids the words Χριστός and Χριστιανός throughout eleven fat Teubner volumes, calling Christ himself διψύχος ἢ Παλαιστίνος (or. xviii 178). Themistius quotes the Old Testament as λόγος τῶν Ἀσσυρίων (G. Downey, Studia Patristica v pt. 111 (1962) 480 f.). Even classicizing Christian writers like Procopius and Agathias tend to avoid specifically Christian terminology: see Alan and Averil Cameron, 'Christianity and Tradition in the Historiography of the Late Empire,' CQ xiv (1964).
in Alexandria during the period in which he was writing. It can hardly, I think, be doubted that he intended, or at least expected, his reader to hear φιλόχριστος. But what was the reader to understand by the poem?

Waltz argued that only a Christian could have used a word such as φιλόχριστος, and Palladas must, therefore, have been a Christian. Keydell also, after contending that Palladas was ‘ein Gegner des Christentums’, conceded that φιλόχριστος, if correct, would imply that he had later been converted to Christianity. This assumption goes back at least to Reiske, but is plainly a non-sequitur: words like φιλοδίκαιον and φιλοβιβαζόμενοι were not normally used in a complimentary sense, and what matters is the context in which the word is used, not the bare fact of its use. Irmscher argued that the context of xvi 282 shows Palladas to have been a pagan: he suspects that the Nikai, pagan goddesses, who bring victory to the Christ-loving city laughing, are laughing from scorn or ridicule. He concludes that an ‘areligiöser Skeptiker’ might have written thus, but never a Christian. I would agree with Irmscher that the poem is full of ‘sarkastischer Ironie’, but not that his own argument proves it. For Himerius Oratio xix 3: Νίκη, Διός τοῦ μεγάλου πατρός, ἐνπατρείᾳ καὶ φιλογέλωσι, τότοις γάρ σετ οἰς ὑμνοσαίι ἀγάλλει ἣ πούρας (cf. Menander 616 = Epit. fr. 11 K) shows that γελώσαση, a stock poetic epithet for a Nike, cannot be pressed here. When Heracles is described by Palladas as μετανόων although his statue has just been thrown down by the Christians (ix 441), that is a different matter. If Palladas had wished to make such a place here he would have used quite a different epithet, as when he calls Nike σταυρός in xi 386 (see below, p. 58). An observation of Keydell’s points the way to the true solution: ‘da wir nicht wissen welche Stadt gemeint ist, ist Sicherheit nicht zu erreichen’. If we did know which city Palladas had in mind we should be in a very much better position to interpret the poem aright.

Most scholars, Reiske, Jacobs, Luck, Bonanno, and now Irmscher have simply assumed that the city in question was Constantinople. Ducange even cited the poem to illustrate a reference in the Chronicon Paschale (i p. 494 Dindorf) to some Nikai in Constantinople. But although Constantinople may have been ‘praeceteris ἡ φιλόχριστος πόλις’ oddly enough φιλόχριστος is not found once as an epithet of Constantinople in all the occurrences of the word listed by Irmscher (Studien ... 325–8): he notes that the word is far less commonly used of places than persons, and can find no example of a city being so described before Damascus in the eighth century. It may well be no more than a coincidence that the epithet is not attested for Constantinople before this—but it is interesting to note that φιλόχριστος is in fact found several times as an epithet of Alexandria. ‘Ἀλεξάνδρεια ἡ λαμπτρά καὶ μεγάλη καὶ φιλόχριστος in Sophronius (PG 87 col. 3596A), φιλόχριστος καὶ μεγαλοπολίς in Leontius Neapolitanus (Vita S. Ioann. Eleem. ed. Gelzer p. 7), where the reading is confirmed by the Latin version of Anastasius, ‘amica Christi magna civitas’ (PL 73 col. 342), and Acta S. Epimachi, 2, in Acta SS. Oct. XIII pp. 712 f. and 717b. None of the passages cited by those who believe that xvi 282 and various other poems were written in Constantinople show knowledge of people or events that Palladas could not have gained in Alexandria, much less prove that he wrote them in Constantinople. Bowra has shown that we need

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21 See F. Homes Dudden, Life and Times of St. Ambrose ii (1935) 404–8 and below p. 57.
24 See his note on Constantine Porphyrogenitus De Caer. 1 (ii p. 4 Bonn).
26 See the discussion of the poem by Bowra, op. cit. (n. 1) 259 f.
27 Constantinopolis Christiana ii (1680) 174 f.
28 Jacobs, Animad. in Anth. Graec. ii 3 (1801) 244.
29 Cited from A. Calderini, Dizionario dei nomi geografici e toponomi dell’ Egitto greco-romano i (1935) 60.
30 Byz. Zeit. liii (1960) 2 f. Irmscher defends the lemma (see next note) but without successfully tackling the point that the motif of statues being melted down fits better into the context of late fourth century Alexandria (see p. 57 below) than fifth century Constantinople: cf. E. Demongeot, De l’unité à la division de l’empire romain, 395–410 (1930) 78, ‘ces troubles sont inconnus à Constantinople, ville récente et dépouvrue de temples vénérables’. 
not believe the *lemma* to ix 528, which seems to refer the poem to a palace founded in Constantinople in at the earliest the 420s, when, on the most plausible chronology of his life, 30 Palladas would have been just over 100 years old. But even if the evidence of the *lemma* is accepted, and it is conceded that the poem was written in Constantinople—by no means a necessary consequence—this proves nothing about Palladas' whereabouts some thirty years before during the temple-wrecking activities of Theophilus, the fanatic patriarch of Alexandria, in 391—the natural context for xvi 282 (see below p. 59). It is clear that Palladas was in Alexandria during those turbulent days, for one of his favourite themes is the ignominious fate of the pagan statues overthrown there by Theophilus and his bands of rioting monks (e.g. ix 441, ix 773, x 90–1). Indeed he refers several times to Theophilus himself. 31 I see no good reason to suppose that the temple of Tyche whose conversion into a tavern caused such amusement to Palladas (ix 180–3), is the same Tychaion that suffered, according to Johannes Lydus (De Mens. iv 32), a similar fate in Constantinople at a date quite unspecified, some time between Pompey and Justinian. 32 The Tychaion of Alexandria, as we learn from a rhetorical exercise wrongly ascribed to Libanius (Förster viii p. 550), was a most imposing building, surrounded, moreover, by a very considerable number of statues of all the principal pagan gods. It is hardly likely that Theophilus would have destroyed all the other pagan temples and statues in Alexandria and left this one alone unscathed—or that its degradation would have escaped the notice, and pen, of a man so preoccupied with Tyche as Palladas. 33 The patriarch George had already threatened to destroy it thirty years earlier in 361, 34 long before Theodosius' edicts forbidding worship at pagan altars and temples in 391, so enthusiastically carried out by Theophilus. 35 Neither can I agree with Irmser and Bonanno that the lampoon addressed to Themistius (xi 292) was penned in Constantinople. It may have been, as the *lemma* of Planudes tells us, aimed εἰς Θεμιστίου γενόμενον ὑπαρχον Κωνσταντινωπόλεως but one did not have to go to Constantinople to find out who the praefectus urbi there was. It is usually supposed that in Oratio xxxiv 2 Themistius answers Palladas' attack: I would maintain, on the contrary, that it is Palladas who is answering Themistius. I find it hard to believe that a man of Themistius' standing would have deigned to reply to the sneers of an insignificant Alexandrian *grammaticus*, 36 whereas it would have been very much in character for Palladas to seize on the somewhat sophistic arguments with which Themistius sought to justify his abandonment of the philosopher’s cloak for public office. If this is so, then there are certainly no grounds for insisting that Palladas must have read the speech in Constantinople, much less for concluding with Bonanno that he was 'un appassionato partecipe della vita politica di quella città'. Of course it would be absurd to maintain that in a life of at least 72 years (x 97) Palladas had never been to Constantinople, but in view of the frequency of references in his poems to Alexandria and things Egyptian (e.g. ix 174, 378, xi 306), and the frequency of *lemma* of the sort εἰς Αἰγύπτιον ῥήτορα (xi 204)—which if only Byzantine guesses, do at

31 x 90–1, 91–1 and ix 175–5, discussed in my article cited in n. 1.
32 Zerwes, *op. cit.* (n. 14) 269 f, shares my opinion that Palladas is referring to the Tychaion of Alexandria. It is perhaps worth noticing that according to Ps. Libanius (I.4.) the statue of Alexander that surmounted the Tychaion was flanked by Nikai. Palladas' Nikai? But ἐντυπωσθείτε, as Mr. P. M. Fraser has pointed out to me, suggests that Palladas' Nikai were bas-reliefs (*cf.* Plotinus v 8.6).
33 See Bowra, Palladas on Tyche, *CQ* x3 (1960) 118 f.
34 Ammianus Marcellinus 22.11.7, a reference to be added, together with Zacharias Scholasticus, *Life of Severus* ed. Kugener, *Patrol. Orient.* ii (1907) 33 to Calderini's entry, *op. cit.* (n. 29) s.v. 'Tychaion'.
35 *Cod. Theod.* xvi 10.10 and 11: even the Christian Socrates (Hist Eccles. v 16) says that 'Theophilus ταύτης τῆς ἐξονίας δρασάμενος παροῦσι εὐγένετο καθυστέρα τὰ τῶν Ἐλλήνων μνημεία. 36 I am glad to note that Zerwes, *op. cit.* (n. 14) 203 f., shares this opinion as well: I was not able to consult his dissertation till my article was virtually complete.
least show that Byzantine editors of the Anthology naturally looked to Alexandria for Palladas' subject-matter—it is hard to resist the conclusion that the greater part of his literary activity was spent in Alexandria. There is however one other poem, xi 386, which should be discussed in connection with xvi 282, first because it also is about a Nike, second because when understood aright it will almost certainly be adduced as a reference to Constantinople. Naturally if Palladas could be shown to have written of one Nike in Constantinople, there would be good reason in spite of the preceding arguments for locating the Nikai of xvi 282 in that city. L. A. Stella has already suggested that the poem was written during a sojourn in Constantinople, following Jacobs' identification of the Patriicus of 1. 4 with a quaestor bearing that name at Constantinople in 390.

\[\begin{align*}
\text{Στυγνὴν τὴν Νίκην τις ἱδὼν κατὰ τὴν πόλιν ἐχθὲς,} \\
\text{ἐἶπε, θεὰ Νίκη, τίπτε πέπονθάς ἀρα;} \\
\text{Ἡ δ' ἀποδορομομένη καὶ μεμφομένη κρίσαν ἐπευ} \\
\text{οὐκ ἔγγος σὺ μόνος; Πατρικίων δὲδομαι.} \\
\text{Ἡν ἀρα καὶ Νίκη πολυώδυνος, ἵν παρὰ θεσμὸν} \\
\text{Πατρικίων ναυτίς ἠμπασεν ὡς ἄνεμον.}
\end{align*}\]

(xi 386)

In view of the fact that this Nike is specifically described as στυγνή and πολυώδυνος in direct contrast with the γελώσαν παρῆναι of xvi 282, it is tempting to look for some connexion between the two poems. Paton (Loeb ed. iv p. 256 n.) says that 'a statue of Victory had been adjudged to this Patriicus'. If this were so one might take the poem as another dealing with the fate of a pagan statue, on this occasion passing under dubious circumstances into the possession of a certain Patriicus. Eunapius and Libanius both allege that the Christians stole the statues they pulled down,\(^{37}\) and they are borne out by Christian sources: Augustine, for example, warns the faithful to be careful when destroying pagan temples 'ut appareat nos piatete ista destruere, non avaritia'.\(^{38}\) Lausus, the cubicularius of Arcadius, amassed a collection that included the Cnidian Aphrodite of Praxiteles, the Samian Hera of Lysippus and the Zeus of Pheidias (Cedrenus p. 564 Bekker). But why then is the Nike μεμφομένη κρίσαν? Κρίσας implies not theft but some sort of official verdict. It can, I think, be shown that Jacobs, though unable to adduce any support for his conjecture, was correct in thinking of a circus victory. First κρίσας is an obvious word to use in connection with a race or contest (cf. Soph. Electra 684, SIG\(^{8}\) 867 with n. 13): second, for the meaning of ἀρπᾶζω in 1. 6, cf. CIL vi 10084.11. The famous charioteer Diocles 'occupavit et vicit' 815 times, 'eripuit et vicit' 502 times; Pontius Epaphroditus (another charioteer, ib. l. 25) 'eripuit et vicit' only 468 times. This, as Friedlaender observed (Sittengesch. ii\(^{8}\) 519), shows that 'eripuit et vicit' was considered the most honourable sort of victory: 'occupavit et vicit' seems to mean 'won easily' (cf. Pliny NH viii 160), and it is fairly certain that 'eripuit et vicit' means 'won at the very last moment', when the team running second 'snatched' the victory from the leader 'by a short head'. Is not ἀρπᾶζω the Greek equivalent of the Latin technical term eripio?\(^{39}\) The picture is of Patriicus in second place snatching at an opening in the same way as a becalmed sailor snatches at the first breath of wind. Patriicus is obviously, therefore, neither quaestor (Jacobs), sailor (Paton), nor astrologer (scholion)\(^{40}\), but a

\(^{37}\) Eunapius Vit Soph. 472, Libanius or. xxx 8, Förster iii 91–2.

\(^{38}\) Ep. 47.3; cf. also Marcus Diaconus, Life of St Porphyry § 65.

\(^{39}\) The διβάπτων (diversium), another technical term of chariot racing, explained by Constantine Porphyrogenitus De Caer. i 69 f., i p. 336 Bonn, is alluded to, but not named at xvi 337: cf. Friedlaender ii\(^{8}\) 525 f.

\(^{40}\) My interpretation entails the rejection of a scholion printed in the editio princeps according to which Patriicus was an astrologer whose floruit fell under Jovian (363–4). It does not seem to have been noticed that this scholion was taken from Suidas' entry s.v. 'Πατρικίων'; and its application to the Patriicus of our poem is, therefore, no more than at earliest a tenth-century guess (the date of Suidas), and more likely due to Lascaris himself. Although
charioteer. There had evidently been some dispute (παρὰ θεσμῶν) about the verdict (κρίσιν) on this ‘photo-finish’. Patricius was obviously not the favourite, hence Nike is represented as ‘downcast’ at his victory, just as she is said to have been ‘in love’ with Porphyrius, a very successful and popular charioteer under Justin I (xvi 337.2). Now there are scores of epigrams in the fifteenth and sixteenth Books of the Anthology on the charioteers whose statues adorned the Hippodrome in Constantinople, but the Hippodrome of Alexandria was every bit as popular. Dio of Prusa describes Alexandria as a πόλις μαυρόμενη ὑπ’ ὀδός καὶ δρόμων ἐπικόων (Or. xxxii 41), Philostratus speaks of the Alexandrians flocking to the Hippodrome (Vita Apoll. v 26), and Procopius of Gaza reproaches a young Alexandrian for spending all his time at the races (ep. cxvii p. 581 Hercher). Procopius of Caesarea, when describing the notorious circus factions of Constantinople, frequently states that the same sort of thing happened in all the cities of the empire (Anecd. 8.1, Bell. Pers. i 24.6). It seems, therefore, only natural to conclude that the Alexandrian Palladas was alluding in xi 386 to the chariot races of Alexandria. Similarly there is no good reason to suppose that the Nikai of xvi 282 adorned any other city than Alexandria.

I suggest, therefore, that in xvi 282 Palladas is ringing yet another variation on his favourite theme of the treatment meted out to the pagan statues in 391 by the unscrupulous Patriarch of Alexandria—with the added piquancy that these Nikai have actually been yoked into the service of Christ. The irony lies not in the one word γέλωσις (see above, p. 56), but in the contrast between the victory the Nikai now bring to the city that loves Christ (Christ, that is, instead of the old θεοί πολιούχοι ‘defeated’ by Theodosius) anti-pagan edicts of 391, and the victories they were designed to celebrate (presumably those of Alexander or the Ptolemies), fashioned by men who loved the city (and, by implication, not Christ). The Nikai seem, perhaps, to have suffered some slight defacement in connexion with their enforced change of allegiance, if we may deduce from the emphasis plainly laid on the fact that the σχῆμα engraved by οἱ φιλούντες τῷ πόλιν were πρέποντα Νικαί, that some typically pagan characteristic had been christianised (such as the substitution of a halo for a laurel-wreath, or a crucifix for a palm branch) with the result that they were not any longer πρέποντα Νικαί. In this they were at any rate better off than Eros, who ended his days melted down into a frying-pan (ix 773)—the usual fate of pagan statues at the time (Socrates, Hist. Eccl. v 16)—but less fortunate than some other ‘converted’ Olympians, who were allowed to survive the pogrom quite undamaged:

Χριστιανοὶ γεγούμενες, ’Ολομπία δόματʼ εἴχοντες
ἐνθάδε ναυτῶν ἀσμένες · οὔδε γὰρ αὐτοῖς
χώνῃ φόλλειν ἀγοῦσα σφετέριον ἐν πυρὶ θήσει.

(ix 528)

We find a parallel to Palladas’ sentiments in the comment of the diehard pagan Eunapius on this ‘victory’ won by Theophilus and his monks over the statues and temples of the old gods: τοὺς ἀναθήματος ἐπολέμησαν (sc. the monks), ἀναβαλόντων καὶ ἁμαρτημάτων, τοὺς τὰς ἐφαμαν νεκρηκέναι (Vit. Soph. 472). It should not surprise us that the more moderate Palladas, though not, perhaps, a very regular temple-goer, should nevertheless resent a ‘victory’ that consisted in the wanton destruction of the great monuments of a

Chalcondyles’ edito princeps of Suidas was not published till 1499, five years after Lascaris’ edition of the Anthology, there is no reason to doubt that Lascaris had access to Suidas before then, for he actually owned one if not two manuscripts of the Lexicon himself (Suidas ed. Adler v (1938) 223, 261). Similarly the seemingly authoritative scholion on the eight poems dealing with Gessius (vii 681–8) is lifted from Suidas’ entry s.v. ‘Τέσσερις’ (with one sigma), and chronological considerations alone show that he is quite certainly the wrong Gessius (see Re vii 1324).

41 Cf. J. W. E. Pearce, Roman Imperial Coinage ix: Valentinian I–Theodosius I (1951) xli: ‘the die-cutter of a Theodosian Virtus Romanorum TRPS in my possession has inserted a cross in the body of the little Victory’. 
glorious past by bigoted unwashed illiterates. He explicitly counted himself among the "Ελληνες—a word that at least since Julian’s day had borne the quite precise connotation ‘pagan’—and it may well be that the old Hellenic conception of the πόλις and its patron gods still meant something to him, as it did to many who, like him, had little time for most of the external trappings of a decadent paganism. It has recently been observed of Libanius, Palladas’ contemporary and a fervent champion of the pagan cause, that 'il concevait volontiers tous les dieux du Panthéon hellénique sous leur aspect poliade . . . il n’est point douteux que son attachement a la religion hellénique ne soit directement issu de sa passion pour l’idée de cité'.

Countless references to θεοὶ πολιοῦχοι might also be cited from the works of Julian, whose hatred of Christianity stemmed not least from his passionate belief in the concept of national gods—a concept naturally irreconcilable with the new Christian idea of the one ecumenical faith, ‘ubiique patria, ubiique lex et religio mea’ (Orosius Adv. Pag. v 2.1). When in Antioch he revered Calliope, Zeus and Apollo, the πολιοῦχοι of Antioch (Misop. 357C, 360D, 366B), when in Athens he paid his respects to Athene (ep. ad Athen. 275A, cf. 280D). Palladas’ fellow-countryman Synesius, ‘hellène et chrétien’, proclaimed himself φιλόπολις (ep. ciii p. 700 Hercher), and among the grammarians and philosophers of fifth-century Alexandria we find a remarkable resurgence of patriotism that harks back to pre-hellenic times. ‘L’avènement du Christianisme fut une défaite pour le parti patriote égyptien. La preuve de ce fait, c’est que ces derniers païens ont toujours à la bouche le mot de πατρία pour désigner leur foi.’

J. Maspero cites many examples from Heraiscus and Horapollon—it is not without significance that the latter wrote a πατρία Ἀλεξανδρείας (Photius, Bibl. 280)—and an interesting example occurs in Sozomen’s description of an incident of which Palladas was probably himself an eye-witness, ‘Theophilus’ sack of the Sarapeum of Alexandria in 391: Οἱ άληθεῖς τῶν φιλοσόφων οὐχί περί πάντων, καὶ τεῖχον χρήσι μὴ άμελεὶν τῶν πατρίων (Hist. Eccl. vii 15). And according to the Zosimus Theophilus destroyed τῶν ζώων πατρίων (v 23.3). It is only natural that we should find traces of a similar patriotism in Palladas. Compare, for example, xi 291:

\[
\text{τί οὐφελήσαι τὴν πόλιν οἰκίσας γράφων,} \\
\text{χρυσόν τουσοῦτον λαμβάνων βλασφημίας,} \\
\text{πολίων λαμβοῦν ὡς ἔλαιον ἔμπορος;} \\
\]

What good to the city is a man who prostitutes his Muse, who peddles his verses like a hawker of olive-oil? See also especially ix 501, a reflection on the collapse of paganism in Alexandria:

\[
\text{Τὴν πόλιν οἱ νέκυες πρώτερον ζῶσαν κατέλευσαν.} \\
\text{ὁμις δὲ ζώντες τὴν πόλιν ἐκφέρομεν.}
\]

42 Palladas, like most of his pagan, and not a few of his Christian, contemporaries, did not entertain a very high opinion of the monks: cf. xi 384 εἰ μναχοὶ καὶ τάστωρε; . . . ὡς πλῆθος μναχῶν πνευσάμενοι μόνοδα. Libanius complained that they ate ‘more than elephants’ (or. xxx 8), and Eunapius likened them to pigs (Vit. Soph. 472).

43 P. Petit, Libanius et la Vie Municipale à Antioche (1955) 192 f., with many references.

44 H.−A. Naville, Julien l’Apostat et sa philosophie du polythéisme (1877) 65 f., and Bidez, Vie de l’empereur Julien (1930) 307 f. The favours Julian showed to the Jews were inspired by the fact that he, like Gibbon, thought that ‘the Jews were a people which followed, the Christians a sect which deserted, the religion of their fathers’.

45 On belief in Athene Poliouchos in Palladas’ day see the pagan Zosimus’ story that it was she who dissuaded Alaric from sacking Athens (Hist Nov. v 6): charming, but false—the Goths did sack Athens.

46 Chr. Lacombrade, Synésios de Cyrène, Hellène et Chrétien (1951) 100 n. 3, and see Harnack’s remarks on the arch-pagan Porphyry, Hibbert Journal x (1911) 68.

47 J. Maspero, op. cit. (n. 5) 188. It is often claimed that Egyptian Nationalism asserted itself in the form of Christianity, against all the evidence: see Maspero op. cit. 188 f., and especially A. H. M. Jones, ‘Were ancient heresies national or social movements in disguise?’ JTS x3 (1959) 286 f.
By νέκυς Palladas means the pagan gods. To insult the pagans Christians regularly described their gods as νεκροί, dead men, and here, as in x 90.7 and 97.2, Palladas ironically follows them. The city that was alive only while the old gods dwelt in it they have now abandoned to its doom, and Palladas is left behind to officiate at its funeral (ἐκφέρομεν). The emphasis laid on the word πολύ, especially in this connection, is most striking. Striking also is the contrast in xvi 282 between τῷ φιλοχρηστῷ πόλει and οἱ φιλοθεῖνες τίνων πολύ. A most interesting parallel is provided by Julian, who had noted with some bitterness in Antioch the phenomenon that now struck Palladas in Alexandria: Χριστόν δ’ ἀγαπῶντες ἔχετε πολιούχον ἀντὶ τοῦ Διός (Misop. 357C). Keydell points out that Sarapis is the only god Palladas mentions who ‘in das menschliche Leben eingreif’, and it may well be, as Geffcken deduced from ix 174 and 378, that Palladas was a devotee of Sarapis, albeit perhaps a rather luke-warm devotee. Worship of Sarapis was still strong in Alexandria in the late fourth century: Libanius tells us in his pro Templis (§ 35, Förster iii 105) that the authorities allowed it to continue because (he claims) they knew that the flooding of the Nile depended on it (cf. also Rufinus, Hist. Eccl. 11.30). Isis worship is attested in Alexandria until well into the fifth century, particularly, again, among grammarians, and when Heraiclus died, Asclepiades, the father of Horapollon, mumified him by wrapping τὰς Ὀσπιδίς ἐπὶ τῶν σώματι περιβάλον (Suidas s.v. Ἡραίκλας). When Theophilus advanced to destroy the Sarapeum, many pagans stoutly resisted, and turned the building into a fortress, making periodic sallies to drag back inside any Christians they could lay hold of to torture and even crucify them (Rufinus, Hist. Eccl. 11.22). Under these circumstances there might have been an added point in Palladas’ designation of Alexandria as φιλοχρηστός, for, as Julian had twice sharply reminded the Alexandrians, the πολιούχος of their city was Sarapis (ep. 111, p. 188 Bidez, ep. 60, p. 69): for Libanius also Alexandria was ἰ τῶν Σαραπίδων (I.c., cf. ep. 1183.2). There was indeed a cult of Σάραπις Πολεών in Alexandria (OGIS 708), and an inscription found there has revealed to us the word φιλοσάραπης (Sammelb. gr. Urk. 4275). If then Palladas was one of the evidently large number of Alexandrians who still remained at least nominally faithful to their old πολιούχος Sarapis, the φιλοχρηστός πόλει of xvi 282 might be construed as an ironic allusion to his successor. For on the site of the Sarapeum destroyed by Theophilus a Christian church was erected (Sozomen, Hist. Eccl. vii 15).

Though traces lingered on, especially in Academic circles, for a century or more, paganism in Alexandria was now officially dead. But, like Heracles, who told Palladas in a dream that καυρῷ δουλεύων καὶ θεός ὢν ἐμάθον (ix 441.6), the laughing maidens survived to herald the victory of a new conqueror. It is interesting to note that it was around an altar of Victoria that a last stand was in these very years being made for the toleration of paganism in the Western half of the Empire.

48 Theoph. ad Autol. i 9, Justin Apol. i 9, Sib. Or. viii 47, Clement Protr. iii 45-5, Eusebius, Praep. Ec. ii 5-6. I deal with this topic more fully in my article cited in n. 1.
50 Ausgang d. grieß.-röm. Heidentums (1929) 175.
51 Zacharias Scholasticus, Life of Severus, ed. Kügener, Patrol. Orient. ii (1907) 18 f., 22, 29, vi 60, ascribed by both the Palatinus and Planudes to Palladas, is a dedication to Isis: but I have given reasons for doubting the attribution in an article to appear in Proc. Camb. Phil. Soc. n.s. x (1964).
52 Cf. ix 378.12, where Sarapis, having saved a murderer's life, warns him in a dream that he is being reserved for the cross (σταυρῷ δ’ ἀθικ φυλασσόμενος). The poem was probably written between the destruction of the Sarapeum and the erection of the church on its site (M. Rubenssohn, Phil. Woch. xxxiii/iv [1903] 1028–9). Might not Sarapis be alluding by σταυρῷ to the church rising on the site of his temple, and warning the murderer that he cannot expect such lenient treatment now that the ‘tenant’ has changed? The play on the two meanings of σταυρός would be exactly in character for Palladas: Bowra indeed, CR x2 (1960) 93, has conjectured that he does in fact employ this very pun at vii 685.4.
had fashioned the Nikai in Alexandria, Symmachus, in his famous plea for the restoration of the altar of Victoria to the Senate house in Rome, regarded the behaviour of those who had removed it as tantamount to treachery.\textsuperscript{53} Both were now in the minority: most men agreed with St. Augustine, who made Victoria give way before the Angel of God.\textsuperscript{54}

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\textsuperscript{53} \textit{Relatio} iii 3, Seeck p. 281.8, and for the πολιούχος motif, cf. p. 281.36 ‘varios custodes urbibus cultus mens divina distribuit’, and the words of his friend Macrobius, ‘constat omnes urbes in alicuius dei esse tutela’ (\textit{Sat.} iii 9).

\textsuperscript{54} \textit{Civ. Dei} iv 14, 17. See Pearce, \textit{op. cit.} (n. 41) for the survival of Victoria as a coin type: ‘accepted by both sides, by the pagans as the goddess to whom Rome owed her greatness, by the Christians as an angel from God’. Cf. also A. Baudrillart, \textit{Les Divinités de la Victoire en Grèce et en Italie d’après les textes et les monument figurés} (1898) 61–6.

I am grateful to Mr P. M. Fraser and Mr E. C. Yorke for reading an earlier draft of this article.
FLUTES AND ELEGIAC COUPLES

My purpose in this article is to call attention to the inconclusive nature of the evidence for the view that the proper accompaniment of early Greek elegiac poetry was the flute. 1 The elegiac couplet has been aptly called ‘a variation upon the heroic hexameter in the direction of lyric poetry’. 2 But how far did it go in this direction? In particular, did early elegiac poetry have a close connexion with music? A distinction is commonly drawn between poetry written kath’ stixon, for example the epic hexameter and the iambic trimeter, which was spoken or at most intoned, and poetry in lyric metres which was sung to musical accompaniment: to which group does elegiac poetry belong?

Passages in Plutarch and Pausanias and hints in the lexicographers and in the early elegists themselves have suggested to some scholars that there was a connexion between elegiac poetry and the flute. 3 Plutarch indeed wrote (Mus. 8) ἐν ἀρχῇ γὰρ ἐλεγεία μεμελοποιημένα οἱ αὐλόδοι ἱδον, and Pausanias is equally explicit (x 7.5): ἤ γὰρ αὐλωδία μέλη τῇ ἴν αὐλῶν τὰ συκυριστότατα καὶ ἐλεγεία προσφαύμενα τοῖς αὐλοῖς. So Bowra (whose Early Greek Elegists is the only general study of the subject in English) writes (5–6), ‘In practice the elegiac couplet seems to have been a song sung to the accompaniment of the flute, just as lyric poetry was sung to the lyre.’ Archilochus’ couplets (8–9) ‘were probably improvised and sung in the intervals of fighting, when someone had a flute and the poet was called upon for a song’. Minnemus (24) ‘used the elegiac as a medium also for narrative and historical or mythological subjects. That these, too, were sung to the flute need not surprise us; for in the seventh century the Greeks had no history in the modern sense, and its place was taken by narrative poetry.’ And on Tyrtaeus (42), ‘The Spartans marched into battle to the sound of the flute, and we may assume that the poems of Tyrtaeus were composed to be sung on the march.’ The collection of poems called the Theognidea consists of elegiac pieces ‘composed to be sung to the flute over the wine, and the book is a songbook’ (141).

Thus Bowra vividly describes the performances of the early elegiasts as he imagines them, and he sets forth clearly the conclusions reached or accepted by most writers on the subject, 4 that elegiac poetry was sung to the flute, and that this distinguishes it from epic poetry on the one hand and lyric poetry on the other.

If this view is correct, we must be quite clear about its consequences. The writers of personal lyric poetry—Alcaeus, Sappho, Ibycus and Anacreon—were able to accompany their own poems, and as they had filled these poems with their own emotions the admission of a second party to the performance would have been intolerable; but we are asked to believe that the elegiac poet, unable to sing and play the flute simultaneously, relied on a flute-playing colleague for the performance of his poems; that Archilochus, for example, thought that flute accompaniment was suitable for such personal poems as his

άσπιδα μὲν Σαῦ ν θις ἀγάλληται (fr. 6)

1 I am grateful to Dr R. Crowhurst, Professor J. A. Davison and Miss R. M. Harriott for their valuable comments on this article.
2 W. R. Hardie, Res Metrica 49, quoted by C. M. Bowra, Early Greek Elegists 3.
3 I ought, of course, to call the instrument an ‘aulos’, since it had a reed-mouthpiece and was therefore not a flute, but I prefer to use an English word which has many of the correct associations.
4 E.g. RE s.v. ‘Elegie’; M. Wegner, Das Musikleben der Griechen 83; Schmid-Stählin, Geschichte der griechischen Literatur i 329 ff.; J. M. Edmonds, Elegy and Iambus i. 16; D. L. Page, ‘The Elegiacs in Euripides’ Andromache’ in Greek Poetry and Life 266 ff.; Paul Friedländer (with Herbert B. Hoffleit), Epigrammata: Greek Inscriptions in Verse 66. Helmut Huchzemeyer in his dissertation Aulos und Kithara in der griechischen Musik (Münster, 1931) goes further and says (29 ff.) that the iambic verse of Archilochus, Semonides of Amorgos, Hipponax and Ananus was also accompanied by the flute.
or

when he might have accompanied them himself on the lyre or even spoken them unaccompanied. Considerable demands, too, must have been made on the flautist’s stamina by some of the longer pieces such as Mimnermus’ narrative and historical poems or the 76 lines of Solon’s longest surviving piece (fr. 13).⁵

The difficulties caused by the theory of flute-accompaniment are serious enough to warrant an examination of the evidence, and we must look first at any hints we find in the early elegists themselves.

Archilochus fr. 123,⁶ ἄδων ὑπ’ αὐλητήρος, does not help much. We have no idea who was singing or what his song was, and the fragment is no part of an elegiac couplet.

Theognis 239 ff. is more useful, and this time we know the context: the poet tells Cyrmus that he has given him wings to carry him over all the world:

θοίνης δὲ καὶ εἰλαπάνης παρέσσῃ
ἐν πάσαις πολλῶν κείμενοι ἐν στόμαισιν,
καὶ σε σὺν αὐλάκουσι λευκοβόγγοι νέοι ἀνδρεῖς
eὐκόσιμοι ἐρατοὶ καλὰ τε καὶ λεγέα
ἀσονταί.

The song which celebrates Cyrmus can only be Theognis’ elegiac poetry, and it is to be accompanied by flutes. But the language suggests that the performances will be formal, perhaps even that they will be given at festivals:⁷ it is significant that ‘many men’ will be talking of Cyrmus and that the song is to be performed not by the poet himself and a flautist but by young men and flutes. We need not suppose that Theognis’ poems were normally performed under such conditions.

Other passages quoted from Theognis are not so helpful. The value of 533, slight enough, is diminished when 534 is quoted with it:

χαίρω δ’ εὖ πίνων καὶ ὑπ’ αὐλητήρος ἀείδων,⁸
χαίρω δ’ εὐβάγγην χεριὶ λύρην ὀξέων.

In 825–6,

πῶς ὑμῖν τέτληκεν ὑπ’ αὐλητήρος ἀείδειν
θυμὸς;

we are not told what the roisterers were singing to the flautist’s accompaniment. In 943–4,

ἐγγύθεν αὐλητήρος ἀείσομαι ὁδε καταστάσ
δεξιὸς ἀδανίτως θεοίσῃ ἐπευχόμενος,

we need not explain ἐπευχόμενος with Edmonds as ‘pres. part. instead of aor. with fut. indic. as often in Gk.’ and make the music part of the dinner party. The music is part of a carefully planned religious observance.⁹ The couplet 1041–2,

δεύρῳ σὺν αὐλητήρι · παρὰ κλαίοντι γελώντες
πίνομεν, κείνου κήδεις τερπόμενοι,

suggests only that the flute-player is to create the atmosphere for the unsympathetic merriment. The revelry of youth is the context of 1065 also, ἐστι δὲ κομάζοντα μετ’ αὐλητήρος

⁵ For the performance of Solon’s Salamis see below.
⁶ These passages are mentioned as evidence by Bowra, 6, n. 4, 6; Schmid-Stählin, i 354 n. 5; Huchzermeyer, 38–40; Wegner, 202 s.v. ‘Elegie’.
⁷ Cf. Richmond Lattimore’s translation, Greek Lyrics 29.
⁸ Pierson. If ἀκοννίον, the MSS. reading, is kept, the passage is of even less value.
⁹ For flute and lyre (though without voice) in a religious context in Theognis, cf. 761–3, φόρμιες δ’αδ φθάνονθ’ ἱερῶν μέλος ἐδὲ καὶ αὐλός · ἦνείς δέ αἰνοδύς θεοίσῃ ἀρεσάμενοι πίνομεν.
Bowra might have referred to 941, oúde τὸν αὐλητήν προφανοίζομαι, where the hoarse singer blames not the flautist but a hangover for his poor performance, though again we do not know what was being performed. Lastly we must consider 1055-8,

\[\text{άλλα λόγοι μὲν τούτον ἔσομεν, αὐτὰρ ἐμοὶ σὺ αἰσθάνεται, καὶ Μουσῶν μην σύμμεθυ' ἀμφότεροι. \] αὐτά γὰρ τάδ' ἔδωκαν ἐχεῖς κεχαραμένα δόρα

soi καὶ ἐμοί, μελέχω, ἥμαρτο σὺ ἀμφιπερικτικός.

The words Μουσῶν μην σύμμεθυ' ἀμφότεροι, 'let us both remember the Muses', probably mean only that Theognis will listen while his friend plays. 'These lovely gifts' in the second couplet must of course mean Theognis' poetry and the friend's music, but once again the poet stops short of saying that the flute-player is to accompany him. The poet is clearly the listener in 531-2,

\[\text{αἰεὶ μοι φίλων ἵττορ ἱαίνεται, ὅπποτέ' ἀκοῦσω}

aiλῶν φθεγγομένων ἵμερόσφαιρά ὁπα.

In case the frequency of flutes in Theognis should itself be regarded as significant we ought to recall 789-91,

\[\text{μὴστὶ μοι μελέθημα νεῶτερον ἄλλο φανεῖν}

αὐτ' ἀρετὴς σοφίης τ', ἀλλὰ τάδ' αἰεῖν ἐχον}

τερποῖμην φόρμαμαι καὶ ἄρχειμω καὶ ἀνδηβή.

The poet's craft lies in his lyre-playing, his dancing and his song. Lyre and flute are mentioned together in 761 and 975.

Persistent references to Mimnermus as a flautist may be significant, as his poetry seems to have been limited to elegiac verse. But we scarcely know enough of his fluting to use this as evidence of a close connexion between flutes and elegies. Hermesianax says of him,

\[\text{ποιμί μ' ἐπι πολλάκι λωτῷ}

κημιωθεῖσ' κόμοις εἰχε σὺν Ἐξαμύῃ,

and again we find the flute and the κόμοι together. Plutarch quotes Hipponax as saying that Mimnermus played the flute-nome called Cradias, and continues with the sentence, ἐν ἀρχῇ γὰρ ἔλεγεν μεμελοποιημένα οἱ αἰλῳδοὶ ἦσον, which is examined below.

Tyrtaeus, too, is said to have been ἐλεγευστοίος καὶ αὐλητής (Suidas s.v. 'Τυρτάιος'), but this does not help much. We are told by Athenaeus (xiv 630 I') that the Spartans marching to battle kept step by reciting Tyrtaeus' poems, but these poems are less likely to have been elegiac couplets than marching-songs (ἐμβατηρία μέλη) like the anapaests quoted by Dio Chrysostomus (Diehl, Anth. Lyr. Gr. ii 197, Carm. Pomp. 18; Page, Poetae Melici Graeci 856; Oxford Book of Greek Verse no. 98: the scholiast on Dio attributes the lines to Tyrtaeus; cf. Pausanias iv 15.6). Thucydides v 70 says that the Spartans went into battle at Mantinea βραδέος καὶ ὧπο αὐλητῶν, and that the flute-players were used to keep a steady rhythm (cf. Cicero, TD ii 16.37; Plutarch, Lycurgus 22.3). No ancient writer says that the Spartans sang elegiac couplets or anything else to flute accompaniment when on the march.

There is one occasion on which we are given full details of the performance of an elegiac poem, and there is a conspicuous absence of a flautist: in Solon 8 Plutarch tells how Solon

10 Edmonds explained the line as a reference to the piper's absence; but the inadequacy of the accompanist has often been the excuse for an inadequate performance.

11 Hermesianax vii 37, Strabo xiv 643, Plutarch Mui. 8.

12 Hermann. MSS. κημιωθεῖς.
delivered his famous poem Salamis: 'he secretly composed elegiac lines and learned them off by heart; then he put on a cap and bounded out suddenly into the agora. When a great crowd had gathered, he mounted the herald’s stone and recited the elegy (ἐν ὀδη ἀντικρισία τῆς ἑλεγείας) which begins

αὐτὸς κηρύξ ἡλικίαν ἀφ’ ἀμφετέρως Σαλαμῖνος,
κόσμου ἐπόλεων ὀδην ἀντ’ ἀγορῆς θέμενος.

The words ἐν ὀδη, as Edmonds says (op. cit., 114), probably refer to formal recitation like that of a rhapsode; and when Plutarch goes on to describe the effect of the performance on its audience, we notice that he says Pisistratus urged the citizens to obey ‘the speaker’ (τῷ λέγοντι). But even if Solon did sing the poem, it seems that although he had clearly given much thought to this performance, he found nothing amiss in the absence of flute accompaniment.

A fifth-century cup in the British Museum shows a reveller singing to lyre accompaniment the words φαῖνεν ἀληθῆ ταῦτα, which look like the introduction to a maxim and may well have been in elegiac metre. Elsewhere on the cup a man sings to the flute the words ὅ δὲ τῆς θυρίδος, a variation on Praxilla’s ὅ δὲ ἔνων θυρίδων (fr. 3). Jacobsthal concluded that the painter had confused the accompanists, but this is a desperate conclusion when we cannot even say for certain that we are dealing with an elegiac fragment.

Another cup, dated c. 490 B.C. and found at Tanagra, shows a man singing the words ὅ παλιν κάλλιστε, which form the beginning of Theognis 1365, although of course they may also have been the beginning of another song; but the cup shows no flautist: the singer holds κροτάλα in his hand ready for later use.

A third cup, the work of Duris in his late period, i.e. somewhere c. 470, does show a flautist accompanying a singer who sings the words οὖ δέναι οὐ. If this is a version of Theognis 695 (οὐ δέναι σοι, θυμεί, παρασχείμεν ἄρμενα πάντα) or of Theognis 367 or 939, both of which begin with the words οὐ δέναι, then we do have evidence that in the early fifth century Theognis’ poetry was sometimes sung to flute accompaniment; but the evidence would prove no more than that.

Euripides used the expressions ἀλυρων ἑλεγον (Hel. 185) and ἀλυρως ἑλεγος (IT 146), but they have no relevance to either flutes or the elegiac couplet. ἑλεγος and ἑλεγοι in Euripides, Aristophanes and later writers meant a lament in any metre or in none, and the ‘lyre-less laments’ are no more than ‘sad laments’. ἀλυρως, ἀναλως, ἀκιθαρις and ἀφορίμικος can all be found in this sense in the tragedians and elsewhere.

We come to the passages in Plutarch and Pausanias which bear on the subject. Plutarch near the beginning of his De Musica lists the αἰλῳδικοί νόμοι and writes (8) καί

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Footnotes:

13 P. Jacobsthal, Göttinger Vasen, A.G.G., N.F. 14 (1912) 62 with pl. 22. 81-2; R. Lullies, Zur boiotisch rotfigurigen Vasenmalerei, Athenische Mitteilungen 65 (1940) 6 ff. with pl. 3. The cup is Boeotian, and Jacobsthal argued that the painting was a copy of an Attic vase of the school of Duris. But the lettering was incised after the cup was fired and need not have been the work of the artist, who in any case was not a copyist. Altogether the cup is poor evidence for anything.

14 Preludes to elegiac γνώμαι are usually more confident in tone than this; cf. καὶ τὸν Δημοδόκον, καὶ τὸν Φωκυλιδόν, μνήμα τοῦ Ἴππαρχε. Could φαῖνεν ἀληθῆ ταῦτα not have been the beginning of one of Praxilla’s dactylic or dactylo-trochaic lines?

15 CVA Athens i III 1 pl. 3. 1, 3 (NM 1357).

16 Munich (Museum antiker Kleinkunst) 2646.

17 Paul Kretschmer, Die Griechischen Vaseninschriften 87, does not link the inscription with Theognis, but supplies πλέον πάνειν with οὐ δέναι οὐ.

18 The evidence is collected by Page, loc. cit., 206-9, but he uses it differently. The word does not occur in earlier writers except for Echmbrotus’ inscription; see below.


20 Perhaps we should say pseudo-Plutarch with F. Lasserre: see his edition, especially 104.
FLUTES AND ELEGIC COUPLETS

άλλος δ’ ἐστὶν ἰχαίας νόμοις καλούμενος Κράδιας, ὃν φησιν Ἰππώναξ Μίμερμον αὐλήσαι. ἐν ἀρχῇ γὰρ ἑλεγεία μεμελοποιημένα οἱ αὐλιδοὶ ἤδων ποίησις δὲ ἐνει δῆλοι ἦ τῶν Παναθηναίων γραφὴ ἣ περὶ τοῦ μουσικοῦ ἀγώνος. Plutarch clearly felt that there was a close connexion between elegiac verse and flutes, and in case his reader was surprised that Minnemus, whom he would know only as a poet, should play the flute, he generalises: ‘in the beginning the flute-singers used to sing elegiac poetry set to music’; and he names his evidence, the Panatheniac inscription about the music competition. Now, if the inscription was relevant to early elegiac poetry (i.e. to writers of the seventh and sixth centuries B.C.) and if Plutarch used it correctly, it proves only that elegiac poetry was sung to the flute at the Panatheniac festival. We need not make the assumption that all writers of elegiac poetry were musical composers who set their poetry to flute accompaniment.

The Pausanias passage (x 7.5) also refers to a festival, the reorganised Pythian festival of 582 B.C., in which ἀλλὰ ἔθεσαν οἱ Ἀμφικτύονες καλαρωδίας μὲν καθὰ καὶ εἰς ἀρχής, προσέθεσαν δὲ καὶ αὐλιδῶν ἀγώναμα καὶ αὐλίδων. The winner in the αὐλιδῶν section was Echembrotus from Arcadia. But at the next festival in 578 B.C. they abolished this competition, καταγώνισε νυν εἶναι τὸ ἀκούσμα ἐνθημον· ἦ γὰρ αὐλιδῶν μέλη τῇ ἄνθρωπῳ ἀλήθεια καὶ ὑπνοία προσφέρομεν τοῖς αὐλίδοις. (‘The flute-singing consisted of flute-tones of the gloomiest kind and elegiac poetry sung to the flutes.’) μαρτυρεῖ δὲ μοι καὶ τοῦ Ἐχεμβρότου τὸ ἀνάθημα, τρίτους χαλκοὺς ἀνατεθεῖ τῷ Ἡρακλεὶ τῷ ἐν Θήρᾳ· ἐπίγραμμα δὲ ἐν τρίτος ἐρχεται Ἐχεμβρότος Ἀρκάδης ἐγκαὶ τῷ Ἡρακλεῖ νικήσας τὸς ἀγάλματος Ἀμφικτύονων ἐν ἀδίδῳς. Echembrotus, then, described his act as ‘singing songs and laments to the Greeks’. As far as we know, the term ἑλεγοι was used exclusively to mean ‘lamentations’ till the time of Callimachus, who used it (fr. 7) to describe his elegiac couplets, as did Apollonides (AP x 19) nearer Pausanias’ own day. No fragment of Echembrotus’ verse survives unless his inscription can be forced into metre, and we have no knowledge of him from any other source. It seems likely that Pausanias guessed wrongly that ἑλεγοι referred to elegiac couplets, possibly with the Callimachus and Apollonides passages in mind and even Ovid’s famous ‘flebilis Elegiae’. Pausanias seems to have misused his evidence, but even if he did not, we again have a reference to competitive performances at a festival, as in the Plutarch passage.

Other scraps of evidence need little attention: when Plutarch (Mus. 3) mentions Clonas, inventor of the αὐλιδικὸς νόμος, and calls him ἑλεγεῖον τε καὶ ἐποίην ποιητῆς, he gives us no information that we can profitably use except that here we have another example of a Peloponnesian of early date who made his mark in the local annals as both musician and writer. Yet another was Polyneustus, who though a Colophonian by birth helped Sacadas and others to found the second musical era at Sparta: he, too, was an elegiac poet, according to Plutarch (Mus. 3). Sacadas of Argos had won the flute-playing contest at Delphi in 582 B.C., and Plutarch (Mus. 8) calls him ποιητὴς μελῶν τε καὶ ἑλεγεῖον μεμελοποιημένων. Sacadas and his school are said to have been ποιηταὶ ἑλεγεῖον (Mus. 9). No scrap remains from any of these writers, and again we cannot even say for certain whether these ἑλεγεῖα were in fact elegiac couplets or dirges, ἑλεγοι, miscalled.

Athenaeus has one or two relevant remarks: ‘Chamaeleon in his book on Stesichorus says that not only Homer’s works but also those of Hesiod and Archilochus and even of

21 See J. A. Davison, ‘Notes on the Panathenaica’, JHS lxviii (1958) 23-42. Panathenaic amphora of c. 560 and c. 540 show that αὐλιδῶν was recognised at the festival at that period.

22 For an uncomplimentary assessment of the writer see The New Oxford History of Music, i 379, where Isobel Henderson calls the pseudo-Plutarchian De Musica ‘an unintelligent source of late antiquity’.

23 It is possible, too, that Plutarch confused the terms ἑλεγοι and ἑλεγεῖα in his source, as I believe Pausanias did: see below.

24 After ἑλεγεῖα some MSS have ἰηρίον, some καὶ ἰηρίον, which must be glosses.


26 Am. iii 9-3. Page, 209-10, lists passages which connect elegiac verse with threnody.

27 Cf. Mus. 5.
Mimnermus and Phocylides were sung to music (μελωδηθέναι)’ (xv 620c). But no one believes that Homer or Hesiod was actually set to music except as an oddity, and we need not believe it of the others. Besides, if there is any value in quoting these passages, one can point to a later speaker at the same party (xv 632d) who mentions Xenophanes, Solon, Theognis, Phocylides and Periander of Corinth, the elegiac poet, καὶ τῶν λουπῶν οἱ μὴ προσάγοντες πρὸς τὰ ποιήματα μελωδίαν, ‘the others who do not set their poems to music’.

Finally, the entries in Suidas under the headings Ὄλυμπος and ἔλεγος are no more helpful. With Olympus, Mysian flute-player and ποιητὴς μελῶν καὶ ἔλεγεών, we are deep in the mists of musical and literary prehistory; no one else vouches for his ἔλεγεια. And when we find the entry ἔλεγος ὀθρῆνος, ἀπὸ τοῦ ἐ ἐ λέγειν, ἃ οἱ πρὸς αὐτὸν ἀδύνατον θρῆνοι τὸν γὰρ αὐτὸν πένθιμον ὑπελείψαι, we need pay no more attention to the second item than to those between which it is sandwiched. Besides, the entry makes no mention of ἔλεγεια, and so confusion is deepened.

The ancient evidence was not likely to help much. References to the accompanying instrument are no more to be found in Theognis than in Sappho or Sophocles; and assertions in late historians, geographers and lexicographers are of extremely doubtful value to the history of early music. We must keep probabilities in our minds, and ask whether the first writers of elegiac poetry thought their new creation so different from epic poetry that it merited a completely different type of presentation; whether their poetry was such that they felt it needed the support of a second performer; and whether the loss of spontaneity in such a performance was negligible. I feel certain that Archilochus and the others answered ‘No’ to these questions if they thought of them at all. We need not reject outright the possibility that for festival performances and other formal occasions the flute was used to give a more impressive presentation. We may believe that Andromache sang her elegiac couplets in Euripides’ Andromache (103 ff.) to the accompaniment of one of the theatre flautists. But we need not imagine Archilochus rehearsing with a flute-player ‘in the intervals of fighting’, or saddle Theognis with flute-accompaniment when he entertained the dinner party. To see the relationship between elegiac poetry and the flute as similar to that between lyric poetry and the lyre is to see a happy parallelism where none exists.

David A. Campbell.

28 For a recent discussion of Homeric performances see Sir Maurice Bowra’s contribution to A Companion to Homer, ed. Wace and Stubbings, 22 ff., with the remarks on 22 about Hesiod.
PERICLES AND DRACONTIDES*

In his discourse on the causes of the Peloponnesian War (Pericles 31–32), Plutarch devotes an inordinate time to what he calls ‘the worst charge [against Pericles], but that having the greatest number of supporters’. The elements of this charge may be outlined briefly:

1. Pheidias was indicted for embezzling the precious materials used in the construction of the great statue of Athena Parthenos. The informer was a certain Menon, a fellow workman, who was subsequently given immunity and tax-free status by a decree of the assembly proposed by Glycon.

2. At the same time, Pericles’ consort Aspasia was indicted and his friend and teacher Anaxagoras was attacked indirectly through a law against religious non-conformity brought by Diopeithes.

3. While the people were still in this mood, Dracontides had a decree passed, requiring that Pericles’ accounts be deposited with the council and that the dicas try any resulting cases on the acropolis with ballots specially sanctified at the altar. This last clause was stricken from the bill by Hagnon, who specified that any resulting suits were to be tried by a jury of 1,500.

4. Because of all these attacks, Pericles resolved to start the war, using the Megarian decree as provocation.

Plutarch reports here the popular fancy—that Pericles started a foreign war to avoid domestic embarrassments. The development of this tradition is a well-known chapter in the history of Greek literature, but as it is fundamental to this discussion, a brief review is called for.

In 425, Aristophanes (Acharnians 515–39) made the first of several alarming disclosures: Pericles started the war because the Megarians had made a personnel raid upon an establishment run by Aspasia. More significantly, in March, 421, the comic poet confided to his audience (Peace 603–14) that Pericles had started the war because he was frightened by the fate of the embezzler Pheidias. Whatever the allusions intended by comedy, these stories gathered momentum, became the nucleus for gossip, and later, it is often assumed, for oligarchic tracts.

All the stories about Pericles seem to have been taken at face value by the historian Ephorus, whose massive work was published sometime in the middle of the fourth century and enjoyed an unrivalled popularity and authority throughout antiquity. Ephorus began his inquiry into the causes of the war with an anecdote: the young Alcibiades asked Pericles why he appeared distraught. Pericles replied that the Athenians had demanded an audit of the money with which he had been entrusted and that he was unable to give it. Alcibiades

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*This paper continues an investigation of Plutarch’s ‘documentary’ sources for fifth-century Athenian history, and of historical problems arising from his use of such evidence. The present writer’s first discussion of this theme, ‘Some Documents in Plutarch’s Lives’, appeared in Classica et Mediaevalia xxi (1961) 182–94.

1 The biographer reveals elsewhere that he knew the stories to be inventions by the comic poets, De Hdt. Mal. 6. 856A. The theme had also been developed by Cratinus in the Dionysalexander (the argument has survived in P. Oxy. iv no. 663).


3 Generally thought to be a parody on the beginning of Herodotus’ history; see Gomme, HCT i 450.


5 Jacoby, FGrH IIIb Suppl., 490.
then suggested Pericles rather find a way not to give an accounting. Following this, Pericles was further burdened with the indictments of Pheidias and Anaxagoras; wishing to divert the people's attention, he started the war, using the Megarian decree as a pretext.⁶

Due largely, one assumes, to Ephorus' influence, this interpretation of the cause of the war became canonised by popular history.⁷ Inevitably, any evidence of attacks upon Pericles and his friends that came to light during later periods of scholarship will have been falsely dated to the period immediately preceding the Peloponnesian War. The popular tradition was further strengthened, for example, and chronology further obscured by some adventurous scholar who speculated that the accounts demanded from Pericles were not connected with the Pheidias indictment, but had to do with the Propylaea, which was nearing completion at the beginning of the war. It may have been Ephorus himself who first calculated a high cost for the Propylaea, perhaps the sum of 2012 talents later given by the periegete Heliodorus (Harpocr. s.v. 'Propylaea'; FGrH 373 F 1).⁸ Demetrius of Phalerum, fifty or so years later, has some sharp things to say about Pericles spending so much money on the Propylaea (in Cic., De Off. ii 60; FGrH 228 F 8). Finally, Valerius Maximus (iii 1, ext. 1) repeats Ephorus' anecdote about Alcibiades and Pericles, but in this version, what Pericles is advised to avoid explaining is the enormous sum spent on the Propylaea.⁹

The tradition that Pericles kindled the war to avoid prosecution would have imposed not only on ancient writers but on modern scholars as well, except for the fact that the unusually astute scholiast to Peace 605 took time to look up the date of Pheidias' prosecution in Philochorus' Attis and found it under an archon year six entries away from the archon year in which the Spartans declared war over the Megarian decree.¹⁰ Most modern critics now agree that the popular dating of the Pheidias indictment to the eve of the war cannot stand against the authority of Philochorus, whose date of 438/7 is supported by the tradition of the chronographers (Eusebius, Chron., 440/39, var. 439/8), and by the recent discovery that Pheidias was active at Olympia in the late 430's.¹¹

It is apparent that Plutarch, in reporting the 'worst charge' against Pericles, is following, for the most part, the Ephoran tradition. On the other hand, he has added to this tradition evidence that seems to be based ultimately on documentary authority. Somewhere in his reading the biographer has come upon the decree of Glycon rewarding the informer Menon,

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⁶ Ephorus (FGrH 70 F 196) cited by Diodorus xii 38-41. Jacoby, FGrH II C, 92-3, would attribute the anecdote in Diod. xii 38.2-4 to a source other than Ephorus; see, however, the arguments of the editors of ATL iii 121-5 and 122, n. 12.

⁷ See, for instance, Suda s.v. 'Pheidias'; Aristodemus, 16 (FGrH 104 F 1). Arist, knew all the stories, but preferred (19) Thucydides' explanation: Spartan fear of Athenian greatness (23.5).

⁸ Diod. xii 40.2: '4000 talents had been spent on the building of the Propylaea and the siege of Potidaea.' Thuc. (ii 70.2) says 2,000 talents were spent on the siege. For an explanation of Ephorus' arithmetic see ATL iii 123-4. Heliodorus' figure of 2012 T is generally admitted to be far too large; see ATL iii 124, n. 15; Gomme, HCT ii 22-23.

⁹ Meyer believed Val. Max. to be repeating Ephorus in more detail than Diodorus; Forschungen ii 330-1.

¹⁰ Philochorus, FGrH 328 F 121. O. Lendle, 'Philochoros über den Prozess des Pheidias', Hermes lxxiii (1955) 284-303, has reinterpreted the text of the scholion, allowing Philochorus to date the Pheidias prosecution to 432/1, but most of his arguments have been anticipated by Jacoby and others: see following note.

¹¹ W. B. Dinsmoor, AJA xvii (1913) 70-1, held that the statue was finished in 438/7, but that Pheidias continued working in Athens and was prosecuted later. But why did Pheidias' co-worker Menon wait six years to inform? That the indictment immediately followed the dedication of the statue seems required by the evidence: Meyer, Forschungen ii 301: 'Wir haben kein Recht, das Datum des Philochoros über die Weihe des Bildes zu verwerten, und Rechenschaftsablage und Process sind davon nicht zu trennen'. This view is strongly supported by Adecok, CAH v 480; Jacoby, FGrH III B Suppl., 496.

Emil Kunze now reports from the excavation of Pheidias' workshop at Olympia that moulds used by the sculptor in creating the chryselephantine Zeus there have been found in a context dateable to the last third of the century: Neue deutsche Ausgrabungen im Mittelmeergebict und im vorderen Orient (Berlin, 1959) 278-94.
the indictment of Aspasia, the prohibition of religious unorthodoxy by Diopeithes and the decree of Dracontides calling for an audit. The decrees of Glycon and Dracontides are fundamental, I believe, to an investigation of the attacks upon Pericles' fiscal probity.

Many modern scholars believe that Plutarch's source for these documents was Craterus, the first ancient writer to collect such evidence in a methodical way. The biographer read widely, however, in the political writings of the Aristotelian school, and his source is just as likely to have been Theophrastus or Demetrius of Phalerum. In any case, whoever first discussed the original documents—probably at the end of the fourth century when such specialised investigation became popular—will have been little more enlightened as to the dates and circumstances than Plutarch, four centuries later. Our knowledge of Athenian government publications leads us to assume that the language of the inscriptions or tablets was terse to the point of obscurity, stating only the barest facts of each case. The decree of Glycon would have said little more than that since Menon was a good man, having informed the Demos of the theft of Pheidias, he should be tax-free and the generals should take care that he not be harmed. By the same reasoning, the decree of Dracontides will have provided only for the depositing of the accounts with the council and for the unusual trial procedure.

Neither of these documents will have given any indication of date or ultimate disposition. Plutarch's source, however, or perhaps the biographer himself, will have inferred both date and circumstances from Ephorus and the popular tradition, reasoning that since Pericles was in deep trouble on the eve of the war, and since these documents are evidence of his troubles, they must belong to the same period.

I have attempted so far to explain how these decrees came to be dated to the eve of the war, in 432/1. The decree of Glycon, rewarding Pheidias' accuser, can be moved back to 438/7, on the good authority of Philochorus. Modern attempts to place the decree of Dracontides, on the other hand, have in my opinion gone astray.

The consensus may be illustrated by the following statement of Sir Frank Adcock:

"The decree of Dracontides must have meant the suspension or deposition of Pericles from the office of general. It must therefore be connected with the only deposition and trial of Pericles of which we have respectable evidence, viz. his deposition and trial in the autumn of 430 B.C." ¹⁴

The first objection to this theory comes from the nature of the decree itself. The existence of a decree calling for an accounting does not prove various further assumptions: that such an accounting was made, that Pericles' accounts were found wanting, that he was tried, and that he was found guilty. In fact, the real reasons for Pericles' punishment in autumn, 430, could not be more plainly stated than they have been by Thucydides: the Athenians, despondent because of the war and the plague, began sending embassies to Sparta. Pericles called an assembly and attempted to dissuade them with a masterful reaffirmation of his strategy; the Athenians came to agree with him, but being for once unanimous in their resentment, did not stop before they had punished him with a fine (ii 59–65.4).

Although there is no direct evidence, one suspects the Athenian assembly needed no legal pretext to dismiss a general. ¹⁵ An impressive characteristic of the Demos is its demand for swift service; it little mattered whether the mills of Attic justice ground coarse or fine, etc.

¹² Meyer, 
¹³ Jacoby, FGrH IIIb, 88 (to Idomenes, 338 F 9); E. Meinhardt, Perikles bei Plutarch (Frankfurt, 1957) 61.
¹⁴ Jacoby, Athos 209.
¹⁵ CAH v 479; Jacoby, FGrH IIIb, 88: 'Ich habe keinen zweifel dass Plutarch mit der δική [i.e. of 430] eben dieses psphisma meint.'

¹⁶ The classical example is the light-hearted exchange of generals—Cleon for Nicias—in 424 (Thuc. iv 27.4–28) concerning which Plutarch says (Nic. 8) Nicias αὐτοῦ ἀπεχθαρτώτητα. Can a regular ἀπεχθαρτωτικα have been much more complex?
as long as they ground fast. It is improbable, moreover, that the Demos would elect to use a procedure as long and involved as that envisaged by the decree of Dracontides to punish a man who all sources agree was a model of financial integrity.16

I believe the decree of Dracontides is rather to be considered as part of a general attack upon the handling of the accounts of the great gold and ivory statue of Athena Parthenos, the attack that began with the prosecution of Pheidias in 438/7. This is, after all, what Plutarch implies, and while the biographer’s grouping of events has no independent value, the following arguments may be brought in support. (1) Philochorus states that Pericles was one of the epistateai for the statue,17 and other sources are unanimous in stating that Pericles came under suspicion because he was Pheidias’ superior—a situation that ended after the dedication of the statue in 438/7. (2) The only natural reaction after the trial and exile of the sculptor would have been a demand, such as this, for an investigation of the board of epistateai to ascertain what, if any, additional misappropriation had taken place. (3) The decree called for all suits to be tried on the acropolis with ballots taken from the altar; this mumbo-jumbo is more easily understood if the suspected crime was the theft of sacred materials from the Goddess herself—hierosylia, as Ephorus (Diod. xii 39.2) says. (4) Finally, Hagnon seems to have offered his amendment at the same assembly in which the decree was brought forward; this amendment will then have been attached to the main bill by the usual formula. In autumn, 430, on the other hand, Hagnon was absent at Potidæa, and scholars favouring the later date are forced to postulate an additional decree brought by him upon his return.18

I offer the following reconstruction of the events of 438/7: Pheidias was indicted, was either exiled or escaped condemnation by flight.19 With the sculptor’s guilt thus seemingly established, Dracontides immediately demanded that Pheidias’ superiors be investigated and that any suits arising be tried under the extraordinary religious sanctions already mentioned. This provision was amended out of the decree by Pericles’ friend Hagnon.20 The implication here is that using sacred ballots taken from the altar of Athena would make superstitious jurymen almost duty-bound to find Pericles guilty. We should assume that Pericles’ reputation for integrity was as unassailable at this time as it had been in 446, when he could write off a sum of ten talents as having been used for reasons of state,21 and that no suits did arise from the investigation. The fact that Hagnon was sent off to establish the colony of Amphipolis the next year—an assignment that brought him great honour22—would seem to indicate that by this time the Periclean faction had regained its predominance in Athenian politics.

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16 That the impeachment was hasty and poorly remembered is indicated by the confusion over the name of the prosecutor in Plut., Per. 35.5; according to Idomeneus, it was Cleon; Theophrastus said Simmias; Heracleides Ponticus, Lecrateides. Plutarch evidently found the account of Theophrastus most convincing, as Simmias is so identified again in Pers. Ger. Reip. 10, 85c.
17 Philochorus (F 121) as quoted by the scholiast says only Περικλῆος ἐπιστατοῦτος, which seems to be his usual wording in the interests of brevity (cf. F 37 on the Lyceum); see also Aristodemus (FGrH 104) 16.1: οἱ τὸν Περικλῆα, etc. Pericles, of course, was only one of a board of epistateai. The building accounts (IG ii 354–62; SEG x 261) gave the names of the treasurers, but only the secretary of the epistateai.
18 Adcock, CAH v 478.
19 The scholiast to Plut. 605 makes it clear that Philochorus said Περικλῆος ἐπιστατοῦτος, which seems to be his usual wording in the interests of brevity (cf. F 37 on the Lyceum); see also Aristodemus (FGrH 104) 16.1: οἱ τὸν Περικλῆα, etc. Pericles, of course, was only one of a board of epistateai. The building accounts (IG ii 354–62; SEG x 261) gave the names of the treasurers, but only the secretary of the epistateai.
20 Hagnon was himself attacked by comedy as having enriched himself during a magistracy; Cratinus, Ploutoi, in D. L. Page, Greek Literary Papyri 200. For his identification with the democratic faction, see the remark of Critias, Xen., Hell. ii 3.39.
21 Thuc. ii 21.1; Plut. Per. 22.3, 23.1 (according to Ephorus, FGrH 70 F 193, the sum was 20 talents).
22 Thuc. iv 102.3–4; v 11.1.
THE archaic inscription from Samos recording a dedication to Hera by two Perinthians (SEG xii 391) is a most interesting document from many points of view. But in this paper I shall confine myself to a single topic, the crux of ll. 3–4.

Klaffenbach, the first editor, frankly admitted that he could not master the problems presented by the letters on the stone (3 f.) O.KHIHIOI. He was naturally reluctant to postulate dittography. The text is short and well inscribed, but that is not all. For when one has assumed the engraver’s error one is still left with a phrase qualifying the names of the two dedicators, oikeiot Perinthis, for which Klaffenbach could see no satisfactory explanation. He therefore inclined to the possibility that there is a word for some form of craftsman concealed in these letters, and the dedication is comparable to the thete dedications by potters from the Athenian acropolis. The lack of any word for a craft at all similar to the letters on the stone gives this suggestion the appearance of a last resort.

Robert thought that dittography should be assumed and oikeiot explained as referring to the relationship as kinsmen which existed between the two dedicators but which was not evident from their names and patronymics. This explanation did not convince Klaffenbach, nor is it supported by other examples. And, as Guarducci rightly objected, clearer ways of expressing their relationship, if this is what the dedicators intended, could surely have been found.

A new approach to the problem was offered by Guarducci. Undeterred by Klaffenbach’s statement ‘es ist zwar verführerisch, wenn wir auf einer samischen Inschrift vor Perinthioi ein mit oik. beginnendes Wort finden, darin ein Hinweis auf das Verhältnis der beiden Städte zueinander zu vermuthen, also eine Bezeichnung wie oikostai, oikostropes o.ä., aber die erhaltenen Buchstaben schliessen doch offenbar jede Möglichkeit der Bildung eines entsprechenden Wortes aus’, she proposed to read a word, oikeiot, which she regarded as expressing the relationship of the Perinthians as colonists of Samos. She sought epigraphical justification for her reading in certain lines observable in the photograph, but even without these was prepared to argue that an engraver’s error of i for γ is a lesser assumption than dittography. No one would deny this if a satisfactory reading resulted. The proposed word oikeiot would be a ἀπαξ λεγόμενον, which Guarducci forms from oikeiot (translated colonist) on the analogy of the word oikeiot (oikeiot), a body of slaves, formed from oikeiot. The two dedicators are, on this interpretation, described as colonising Perinthians, or, if we give the adjective the force of a noun, Perithian colonists. Apart from the dubious method by which an engraver’s error is assumed in order to produce a ἀπαξ λεγόμενον, the question remains: why should an unparalleled adjective be used when there were common nouns available meaning colonist and settler? It seems unanswerable, and Robert treated the suggestion with scorn.

A brief mention of the problem of ll. 3–4 by L. H. Jeffery in Local Scripts of Archaic Greece is vitiated by the assumption that oikeiot can mean colonist, and a misrepresentation of Klaffenbach’s views by which he is made to agree with Guarducci.

1 Mitteilungen des deutschen archäologischen Instituts vi (1953) 15–20. His text is reproduced as SEG xii 391.
2 On all this see Klaffenbach, 17 f.
3 See Klaffenbach, ibid., and REG lxii (1959) 225 f.
4 Loc. cit.
5 p. 25; see next note.
7 Loc. cit.
8 See 365 with n. 4. Cf. ‘This would mean that the phrase oiknijiou means not “the settlers” (i.e. original colonists) but simply “the colonists”’. Her
Of these treatments only Guarducci’s attempts to correct Klaffenbach’s actual reading. If one studies the stone itself one is bound to conclude that Klaffenbach has rendered the letters on the stone entirely correctly. The apparently suggestive cuts round the second omikron in o[1]κειωσι there are shallower than those of the letters and irregular, and though the gap of c. 0.05 m. between the last two iota looks wide it is in fact not significantly larger than that usually left for omikron. Apart from these cuts the letters on the stone are absolutely clear.

Since there is no justification from the stone for any different reading, there is no escape from a choice of evils: either we assume dittography or an engraver’s mistake. The great obstacle to the latter assumption is shown by Guarducci’s attempt. No small or single change will produce a satisfactory reading. If we are to make either a word for settlers or founders or for Klaffenbach’s suggested craft we have to postulate such considerable changes that we go far beyond any probable engraver’s mistake. It is not surprising that Robert reiterated his view that it is best to correct a ‘simple dittographic’. This is certainly the easiest assumption epigraphically.

But the failure till now to find a satisfactory interpretation shows that the problem is not to be settled merely by looking at the stone and a lexicon. The circumstances of the dedication should have weight in the choice of an explanation. Perinthos was a colony of Samos, and so Guarducci was surely right to look for a reference to this relationship in the word qualifying the two Perinthian dedicators, even if her actual solution is unacceptable. She very justly compared the dedication with other known instances of dedications by colonies to the deities of the metropolis. It also seems an attractive explanation of the unusual statement of the cost of the dedication that the two men were chosen to make the offering on the part of the state and the sum mentioned was the size of dedication required of Perinthos. If this is right it meets Robert’s objection that the analogy of other colonial dedications is inadmissible because this dedication is by individuals. But even if it is wrong, dedications by individual colonists to the gods of the metropolis are known elsewhere. It seems to me that unless one assumes that the Perinthian dedicators were consciously making an offering as colonists to the great deity of their mother city, one is setting one’s face against both analogy and probability.

A satisfactory solution will therefore combine the simplest epigraphical assumption with the most likely sense, viz. read o[1]κειωσι and understand it as referring to the colonial relationship.

The only meaning of oikeios likely to concern us here is ‘kin, related’ (section II.1 of the article in LSJ). That the word could bear the meaning kin in a general sense can own tentative suggestion that the two dedicators were ‘οικεῖοι in the sense of Perinthian officials responsible for the gifts in a Perinthian oikos or Treasury’ seems too far from the attested meanings of oikeios to be a serious possibility.

To do this one needs now to visit the ‘apotheké’ by the main church in Tigna (officially Pythagoreion).

9 See PLATE II.

10 REG lxii (1959) 225 §320. I take the liberty of correcting Robert’s actual words ‘simple haplographie’.

11 See Guarducci, 26 f., for examples.

12 See Diehl Anth. Lyric. Graec. ii (1925), Simonides no. 114, a dedicatory epigram probably not by Simonides (see M. Boas, De Epigrammatis Simonideis (Groningen, 1905) 119), is hardly an exact parallel, as the sum mentioned is presumably the sculptor’s fee, not the cost of the dedication:

Ἀρτέμιδος τοῦ ἄγαλμα—διηκόσια γὰρ ὁ μυθὸς
dραχμαί τιν Παρθανί, τῶν ἔποιησα τράγος—
ἀσκητὸς δ’ ἔποιησεν Ἀθηναίως παλάμης
ἀξίων Ἀρκεσίας, νίκος Ἀριστοκράτου.

13 I also agree with her that this would explain why the two men dedicate together even though they were apparently not related.

14 Loc. cit.

15 E.g. that of Deinomenes to Athena Lindia; see Lindian Chronicle xxvii in Blinkenberg, Lindos ii.

16 Whatever the date of the inscription, though the argument is stronger if it is dated within a generation of the colony’s foundation in c. 600, i.e. 580–570. This is where Klaffenbach placed it (16 f.), but Jeffery offers a much later date, c. 525 (371, no. 35), on arguments which do not seem to me compelling; see 365.
already be seen in Herodotus’ order (viii 39.1) παίδος . . . καὶ ἀδελφοὺς καὶ ὀἰκεῖοι καὶ φίλους, and this character made it possible in Hellenistic times for ὀἰκεῖος to mean kin of a wide and indeterminate kind. The statement of the Epidamnians in their decree passed in reply to the representations of the people of Magnesia on the Meander, παρεκάλουν δὲ καὶ ἄμε ὄντα ὀἰκεῖοι καὶ φίλους [2]ποδέχασθαι τὰν τε θ'ναταν κτλ.18 (Ditt. Syll.3 560.20 f.), is a sufficient and by no means isolated example.19 But such a wide use can also appear earlier; cf. Thuc. iv 64-3, where Hermocrates is made to say at the conference of Gela: οὐδὲν γὰρ αἰσχρὸν ὀἰκεῖοι ὀικεῖοι ἡγοσθαί, ἡ Δωρίδα τινὰ Δωρίων ἡ Χαλκιδεὰ τῶν ἔργανον, τὸ δὲ ἔμπιπτεν γείτονας ὄντας καὶ νεοκλικοὺς μᾶς χωρίς κτλ. In general we may conclude that ὀἰκεῖος would be a satisfactory word for Perinthians to use to describe their relationship to Samos.

However, the two Perinthians, if they wanted to express this, chose to do so elliptically. Where the reference of ὀἰκεῖος is obvious it is frequently not expressed, and in archaic inscriptions it was not thought necessary to state the self-evident, but these general truths are hardly enough in themselves. An analogy may help. The ὅρκον τῶν ὀἰκιστήρων of Cyrene20 contains the phrase Αἳ μὲν δὲ καὶ κατέχουσιν τῶν ὀικισίων οἱ ἄποικοι, τῶν ὀικείων τοῦ καταπλεόντας στὴρεος ἡ Λιβύν καὶ πολιτήμας καὶ τιμᾶν πεδέχετε καὶ γάς τὰς ἀδεσπότω ἀπολογισάμενες (30–35).21 Here too the reference of ὀἰκεῖος is not stated and different interpretations have been put on the word by modern scholars.22 But the ancients at least had no doubts. The ancient foundation decree is given as justification for the Cyrenæan action in conferring citizenship on all the Theræans; the only part of it which could justify such an action is this phrase; so it was clearly understood to mean all the Theræans. In my opinion we cannot say for certain whether a given part of the wording of the ὅρκον is authentic, so it is not possible to decide whether ὀἰκεῖοι is part of the original wording of the seventh-century decree, though this does not seem impossible.23 In the fourth century at least the word was regarded as sufficient in itself to mean kinsmen of the colonists, i.e. all citizens of the metropolis. This suggests that ὀἰκεῖος had a clearly recognized meaning in the context of the relations between a colony and its mother city: the citizens of each were ὀἰκεῖοι of the other. Hence two Perinthis dedicators who set up a stele in the precinct of the great goddess of their metropolis could call themselves ‘Perithian kinsmen’ without further elaboration and so express their relation as colonists to Samos.

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19 Robert makes the general comment that in Hellenistic decrees ὀἰκεῖος is sometimes equivalent to σμαριον, but not always (BCH lli (1928) 171 n. 5).
20 SEG ix 3. I discuss this document in JHS lxxxi (1960) 94 ff.
21 The text is that of Oliverio, Ric. di Fil. vi (1928) 225. The words τῶν ὀἰκείων were printed by him in large type in order to indicate that he read them with difficulty (cf. 186). Mr P. M. Fraser has kindly informed me (by letter) that he could not see the letters ἄποικοι, τῶν ὀἰκείων on the stone, which is now fixed in a dark part of the museum. However, Oliverio (223) describes how he waited for the sun to strike the stone at favourable angles to reveal each letter in turn, and it is noteworthy that even his photograph (pl. 12) reveals two letters quite clearly (ων of ὀἰκείων) which were not visible to Mr Fraser, and possibly others (e.g. π of ἄποικοι). There is, therefore, sufficient reason for confidence in Oliverio’s reading of this passage. It is not among those attacked by Ferri, Historia iii (1939) 389–396.
22 E.g. Oliverio and Chamoux took it as equivalent to all the Theræans (Riv. di Fil. vi (1928) 227. and Cyrene sous la monarchie des Battides (Paris, 1952) 107), while Meiggs’ translation suggests to an English reader a more narrow sense of relationship (‘any kinsman’; see Bury, History of Greece’s 362).
23 I discuss the question of the decree’s authenticity at length in the article cited above, n. 20. Cf. also Jeffery, Historia x (1961) 139 ff., whose conclusions, reached independently, are on the whole similar. I take this opportunity to acknowledge the justness of Parke’s criticism of my treatment of the word ἀυτοματέω (JHS lxxii (1962) 145).
HOARDS, SMALL CHANGE AND THE ORIGIN OF COINAGE

Numismatists necessarily deal in minute detail, often so minute that it passes unnoticed by those used to a larger scale; yet as much historical knowledge may depend on such minutiae as on the far larger details of sculpture or architecture. This difference of scale, however, is dangerous because it tends to increase the gulf between numismatists, who seem to be pursuing ever smaller details, and historians, who want to be told how coinage may affect the larger picture with which they are concerned. The following pages are an attempt to assemble and evaluate some of the very incomplete, but annually increasing, evidence, which bears on the function and use of coinage in the Greek world in the sixth and fifth centuries B.C.¹ The treatment will be divided into three sections:

1. Evidence for the movement of coins from their various areas of origin.
2. Evidence for the incidence of fractional coinage.
3. The implications of 1 and 2 for an understanding of the function of early Greek coinage.

1. THE MOVEMENT OF COINS FROM THEIR AREAS OF ORIGIN

Evidence for the areas over which different coinages circulated is provided mainly by the distribution of hoards, and the incidence of various coinages in them. Unfortunately the record of hoards is deplorably incomplete, for very few are found in controlled excavations; most turn up unexpectedly, often in remote places, as a result of entirely non-archaeological disturbance of the soil. Such finds are usually quickly dispersed and come on to the market piecemeal; thereafter, if their discovery is noted at all, they can be only partly reconstructed. For our present purpose, however, the exact composition of any particular hoard is unimportant, for we are concerned only with general trends, which can be sufficiently deduced even from the deficient evidence at our disposal.

Nor need we concern ourselves with any but major mints, for however interesting small or isolated issues may be for other reasons—artistic, political or historical, such issues are unlikely to set an economic trend, and do not usually appear in significant quantity in our evidence for distribution. We can, therefore, concentrate on those mints which have a substantial and more or less continuous output of a major denomination;² such mints are relatively few in the sixth and fifth centuries B.C. The sequence of treatment will be by areas from West to East (Italy and Sicily, Central Greece, Northern Greece, Asia Minor, the Near East).

THE WEST

Italy. In the sixth century there are five major mints, Sybaris (until 510), Metapontum, Croton, Caulonia and Poseidonia; in the fifth century three more became prominent, Taras, Thurium (from c. 440) and Velia. Rhegium, though on the Italian side of the Straits, belongs to the Sicilian rather than the Italian economic sphere. Hoards from the area

¹ The substance of this article, though in somewhat different form, was given as a paper to the Hellenic Society on January 10, 1903. I have benefited from a number of interesting and useful points which were raised in the subsequent discussion.
² By this term I mean staters (pieces of two or three drachmae), usually weighing about 7–8.6 gm according to standard, and tetradrachms weighing about 17 gm. Small change, intended for local use, is not relevant in this section, since it does not travel widely; it will be examined in Section 2.
are numerous, though many are recorded only summarily. The picture they give throughout the period is a consistent one—coins from a number of cities are found together in quantity in the same hoard. This can be illustrated by a table giving the principal mints represented in several of the better recorded hoards.

<table>
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<th>City</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
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<tr>
<td>Sybaris</td>
<td>c. 103</td>
<td>4</td>
<td>13</td>
<td>—</td>
<td>9</td>
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<tr>
<td>Metapontum</td>
<td>c. 133</td>
<td>71</td>
<td>86</td>
<td>10</td>
<td>58</td>
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<tr>
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<td>c. 80+</td>
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<td>92</td>
<td>51</td>
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<tr>
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<td>45</td>
<td>49</td>
<td>19</td>
<td>43</td>
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<tr>
<td>Poseidonia</td>
<td>4</td>
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<td>24</td>
<td>16</td>
<td>143</td>
</tr>
<tr>
<td>Taras</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>2</td>
</tr>
<tr>
<td>Thurium</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>22</td>
</tr>
<tr>
<td>Velia</td>
<td>c. 95</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>2</td>
</tr>
</tbody>
</table>

1. Taranto, Noe 1052, buried c. 490.
2. Curinga, Noe 285, buried c. 470.
3. Calabria, Noe 180, buried c. 460.
5. Unpublished hoard, buried late fifth century.

From these figures it is clear that staters minted in S. Italy circulated without restriction within the region. On the other hand, these issues are never found in neighbouring Sicily or elsewhere outside S. Italy. The S. Italians, therefore, did not employ their coinage in external trade; since, however, there appear to have been no local sources of silver sufficient to supply the S. Italian mints, the metal had to be acquired from abroad either as loot or in exchange for products or services.

**Sicily.** Here the story is much the same, except that Sicilian coinage seems to start somewhat later than S. Italian; only Selinus, Himera, Acragas and Zankle had substantial outputs in the sixth century, though none probably compared in volume at that time to the major S. Italian mints. In the fifth century the principal mints were Syracuse, Gela, Acragas and Messana, with Catana and Leontini in the second rank. From about 480 the Sicilian cities employed a common weight standard, so that coins from different mints were readily interchangeable. This is reflected in the mixed composition of hoards throughout the century.

<table>
<thead>
<tr>
<th>City</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Syracuse</td>
<td>A few</td>
<td>133</td>
<td>97</td>
</tr>
<tr>
<td>Gela</td>
<td>260+</td>
<td>95</td>
<td>39</td>
</tr>
<tr>
<td>Acragas</td>
<td>400+</td>
<td>91</td>
<td>26</td>
</tr>
<tr>
<td>Messana</td>
<td>2</td>
<td>20</td>
<td>49</td>
</tr>
<tr>
<td>Catana</td>
<td>—</td>
<td>2</td>
<td>22</td>
</tr>
<tr>
<td>Leontini</td>
<td>—</td>
<td>27</td>
<td>1</td>
</tr>
</tbody>
</table>

2. Selinunte, Noe 953, buried c. 455.
3. Ognina, Noe 749, buried c. 400.

Of Sicilian coins outside Sicily the record is slightly more impressive than it was in the case of S. Italy, but still amounts to very little. Three Italian hoards each containing a

---

3 Reference to hoards will be to Noe, 'Bibliography of Greek Coin Hoards' (2nd edn.), *NNM* 78, hereafter referred to as Noe.

4 Poseidonia and Velia are exceptions, because they lie outside the main area of S. Italian colonisation, and because they both employ a weight standard different from that of the majority.

5 The representation of Sybaris declines progressively the later a hoard is buried after 510.
substantial Sicilian element probably all come from the area of Reggio, and thus belong to the Sicilian rather than to the Italian economic sphere. Outside this area there are a few Sicilian coins in the Taranto hoard (Selinus, c. 5; Himera, c. 4), an accumulation, which is unique in S. Italy for the number and variety of coins from outside the area, and a single tetradrachm of Gela in Noe 507. In addition, a number of S. Italian coins are known to have been overstruck on Sicilian issues, mostly didrachms of Gela and Acragas; the total number of such overstrikes so far known is about forty-five. I have argued elsewhere that the presence of such Sicilian coins, virtually unsupported, as it is, by additional specimens in S. Italian hoards, cannot be taken as evidence for widespread use of Sicilian coins in S. Italy; such overstrikes are rather a means of disposing of batches of obsolete or otherwise unwanted coins. Farther afield the record of Sicilian coins is extremely meagre, and is confined to isolated examples in the extremely mixed Near Eastern hoards, which will be discussed below (p. 84).

From this evidence it appears that though the considerable coinages of Sicily and S. Italy circulated freely within their respective areas, they played no significant part in international trade. Sicily, like S. Italy, has no internal source of silver, so that in both areas the same factors presumably operated to retain for internal purposes the coins struck from precious metal which had to be obtained from abroad.

**CENTRAL GREECE**

*Aegina.* Familiar evidence for the reputation of the Aeginetans as traders need not be repeated here; it is sufficient to say that the sixth century was their golden age, and that this coincides with the greatest activity of Aegina as a mint. Certainly after 480 she was fighting a losing battle against political and economic domination by Athens, and the quantity of her coinage was much reduced, until it was brought to an end either by the Coinage Decree or by the expulsion of the Aeginetans in 431.

Specimens of the sixth-century coinage are found over a wide area, as is, indeed, inevitable where the output was so great; yet over most of this area turtles provide only a very small portion of any find in which they occur. In the West they, like so many other imported coinages, make their sole appearance in the Taranto hoard (Noe 1052) with about sixteen examples; clearly they formed no significant part of western currency. In the east Mediterranean specimens occur in most hoards, but hardly ever in such numbers as to form a substantial proportion, as the following table shows:

<table>
<thead>
<tr>
<th>(a) Near East</th>
<th>Total in hoard</th>
<th>Aegina</th>
</tr>
</thead>
<tbody>
<tr>
<td>Noe 806 (Persepolis)</td>
<td>8</td>
<td>1</td>
</tr>
<tr>
<td>S. Anatolia (c. 480)</td>
<td>38</td>
<td>11</td>
</tr>
<tr>
<td>Lebanon (c. 430)</td>
<td>76</td>
<td>11</td>
</tr>
</tbody>
</table>

*6 Noe 180 (Calabria); Noe 854 (Reggio); Noe 857 (Reggio).*

*7 Noe 1052: probably more than one hoard; this does not affect the present argument, since all the material is thought to have been found in S. Italy (cf. *NC* 1962, 421 ff.).

*8 From near Taranto; now fully published by Noe, The Coinage of Caulonia (ANS Numismatic Studies no. 9) 59 ff.*

*9 Most of these are recorded in Museum Notes viii 13 ff. and *NC* 1960, 66 ff.*

*10 *NC* 1960, 71 ff.*

*11 The only instances appear to be Noe 729 (Naucratis), one Syracuse; Noe 730 (Naucratis), one each of Syracuse, Leontini and Messana; S. Anatolian hoard (NC 1961, 107 ff.), one Samians at Zankle.

*12 They also had a long life for they continue to be found in hoards buried at least down to the middle of the fourth century; cf. Noe 718 (buried c. 350, not after 338", as Noe), which still contained many sixth-century turtles.*

*13 See also Schmidt, *Persepolis* ii 113.*

*14 *NC* 1961, 107 ff.*

*15 To be published by Monsieur P. Strauss of Basel.*
HOARDS, SMALL CHANGE AND THE ORIGIN OF COINAGE

(b) Egypt (before c. 480)
Noe 722 (Myt-Rahineh) ... 23
Noe 323 (Demanhur) ... 165
Noe 888 (Sakha) ... 72+
Fayum$^{16}$ ... 15
Noe 143 (Benha) ... 71+

(c) Egypt (after c. 480)
Noe 1178 (Zagazig) ... 84
Noe 729 (Naucratis) ... 15
Noe 730 (Naucratis) ... 84

The only hoard in which they account for more than a quarter is that from S. Anatolia, which is also the nearest to the area of use of the Aeginetan standard, extending as far as Ialyssos on Rhodes. Farther afield the representation of Aegina is more meagre still, even in Egypt, where the Aeginetan share in the settlement at Naukratis (Her. ii 178) might have been expected to be reflected in higher proportions of Aeginetan coins in Delta hoards.

In one area only do Aeginetan coins provide a notably larger proportion of the hoards in which they occur, and that is in the more immediate neighbourhood of Aegina itself. Here, and here only, do we find hoards in which half or more of the contents are Aeginetan:

<table>
<thead>
<tr>
<th>Location</th>
<th>Total in hoard</th>
<th>Aegina</th>
</tr>
</thead>
<tbody>
<tr>
<td>Matala, Crete (c. 520)$^{17}$</td>
<td>70</td>
<td>69</td>
</tr>
<tr>
<td>Noe 920 (Thera-Santorin)</td>
<td>760</td>
<td>541</td>
</tr>
<tr>
<td>Noe 671 (Melos)</td>
<td>(Similar to Noe 920)</td>
<td></td>
</tr>
<tr>
<td>Noe 289 (Cyclades)</td>
<td>144</td>
<td>114</td>
</tr>
<tr>
<td>Isthmia votive deposit (c. 475)$^{18}$</td>
<td>135</td>
<td>61</td>
</tr>
</tbody>
</table>

On this evidence the triangle formed by Corinth, Rhodes and Crete could be regarded as a currency area (like S. Italy or Sicily), within which Aeginetan coinage was to be found in quantity, and outside which it occurs in numbers no greater than those of many other coinages. The chief difference would be that, within the area, Aeginetan coinage enjoyed a far greater predominance than did any one coinage in S. Italy or Sicily. A possible reason for this might be that the S. Aegean area had its own local source of silver in the island of Siphnos (Her. iii 57), the output of which may have come in great part into Aeginetan hands through the channels of trade,$^{19}$ whereas the distant and possibly various sources of S. Italy and Sicily were less susceptible to control by any single city.

Corinth. The role of her coinage in the sixth and fifth centuries has been much exaggerated to the extent of creating a ‘mirage Corinthien’. Corinth has been seen as the dominant commercial power in S. Italy, where the local coin standard has been interpreted as ‘reduced Corinthian’ derived from the weight of worn Corinthian coins; on the evidence of a few overstrikes, moreover, the exported coinage of Corinth has been supposed to have been the source of much of the silver used in S. Italy. Such views seem to me untenable and I have argued against them at length elsewhere;$^{20}$ here I will only repeat that the mint of Corinth, using about sixty-five obverse dies in the sixth century and a hundred in the fifth, simply did not have a large enough output to dominate the West, or to provide,

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$^{16}$ Scaby's Coin and Medal Bulletin, January 1960, pl. 3.
$^{17}$ This hoard is preserved in the Ashmolean Museum, Oxford.
$^{18}$ Hesperia xxiv (1955) 135 ff.; the deposit includes a number of fractional denominations.
$^{19}$ There is no evidence for political control of Siphnos by Aegina.
$^{20}$ NC 1960, 71 ff.
through her coinage, a significant proportion of the West's needs in silver. Nor are such views supported by western finds of Corinthian coins, of which the list is extremely short; it is confined to the Taranto hoard (Noe 1052) which contained thirteen Corinthian staters, and a recent find from near Terina of a single Corinthian stater with forty-three staters of Sybaris. In addition, about thirty-five western overstrikes (nearly all from S. Italy) have been observed on Corinthian coins. Such evidence is quite insufficient to support claims that Corinthian coinage enjoyed any special position in the West.

The record of Corinthian coins from the Levant and the Near East is hardly more impressive:

(a) Levant

<table>
<thead>
<tr>
<th>Coinage</th>
<th>S. Anatolia (c. 480)</th>
<th>Noe 978 (Side)</th>
<th>Total in hoard</th>
<th>Corinth</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>38</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>18+</td>
<td>1</td>
</tr>
</tbody>
</table>

(b) Egypt

<table>
<thead>
<tr>
<th>Coinage</th>
<th>Noe 722 (Myt-Rahineh)</th>
<th>Noe 323 (Demanhur)</th>
<th>Noe 888 (Sakha)</th>
<th>Noe 500 (Egypt)</th>
<th>Noe 1178 (Zagazig)</th>
<th>Total in hoard</th>
<th>Corinth</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>23</td>
<td>165</td>
<td>72+</td>
<td>24</td>
<td>84</td>
<td>23</td>
<td>4</td>
</tr>
</tbody>
</table>

These are clearly no more than stray coins which drifted with much miscellaneous silver into Near Eastern hoards.

Nearer Corinth herself the record is still meagre, but, so far as it goes, it does suggest that most Corinthian coins did not at this time travel far from their place of origin. Apart from a few pieces found in excavations at Corinth, there are fifty-two Corinthian coins in the Isthmia deposit, and a late-fifth-century hoard of Corinthian coins of unknown size was found near Corinth about 1500. Though the evidence for Corinth is less satisfactory than for most other areas so far examined, it does appear that Corinth too did not disperse her coinage far afield, but retained it for local use.

Athens. As a result of much recent discussion about the early coinage of Athens, there now seems to be fairly general agreement that the familiar owl coinage did not start before a date well inside the last quarter of the sixth century, and that it was preceded by issues with changing types known collectively as 'Wappenmünzen'. The pattern of distribution of these latter is one with which we have become familiar in the areas already examined; the great majority of examples have come to light in Attica or Euboea, while finds further afield contain only isolated specimens. Clearly the 'Wappenmünzen' circulated mainly in Attica and only occasionally drifted outside.

So far, from all the areas examined we have extracted a consistent picture of coinages tending to stay in their areas of origin, and only rarely being found outside them. With the owls of Athens, however, there is a radical and apparently immediate change; for, while they are not unnaturally still found in numbers within Attica (e.g., Noe 96 from the

21 Cf. G. K. Jenkins in Centennial Publication of the American Numismatic Society, 368 ff.; only from the mid-fourth century onwards are Corinthian coins found in considerable numbers in the West.
22 AJA lxv (1961) 381.
23 Cf. note 9 above; for the rather more numerous S. Italian overstrikes on Sicilian coins, see p. 78 above.
24 See above, n. 14.
25 Preserved in the Corinth Museum.
26 See above, n. 18.
27 Cf. BMQ xix 13.
29 The list of internal finds remains as drawn up by Seltman, Athens, its History and Coinage 146 ff., sects. I-III and VII, with addition of three stray finds from the Agora excavations; farther afield we have Taranto (Noe 1052) 2 exx., Benha (Noe 143) 1 ex., Sakha (Noe 888) 2 exx. and Fayum (n. 16 above) 1 ex.
Acropolis), they also occur in substantial numbers far afield, both westwards and eastwards. Finds of owls from outside the Central Greek area can be tabulated for the sixth and fifth centuries as follows:

<table>
<thead>
<tr>
<th>(a) The West</th>
<th>Total in hoard</th>
<th>Athens</th>
<th>Approximate date of burial</th>
</tr>
</thead>
<tbody>
<tr>
<td>Noe 1052 (Taranto)</td>
<td>c. 100&lt;sup&gt;30&lt;/sup&gt;</td>
<td>5</td>
<td>510</td>
</tr>
<tr>
<td>Noe 685 (Messina)</td>
<td>35</td>
<td>20</td>
<td>490</td>
</tr>
<tr>
<td>Gela 1957&lt;sup&gt;31&lt;/sup&gt;</td>
<td>c. 1,000</td>
<td>c. 200</td>
<td>485</td>
</tr>
<tr>
<td>Noe 667 (Mazzaro)</td>
<td>338</td>
<td>6</td>
<td>480</td>
</tr>
<tr>
<td>Noe 854 (Reggio)</td>
<td>?</td>
<td>?</td>
<td>?</td>
</tr>
<tr>
<td>Noe 931 (Schiso)</td>
<td>2,000</td>
<td>a few</td>
<td>410</td>
</tr>
<tr>
<td>Noe 949 (Selinunte)</td>
<td>48</td>
<td>4</td>
<td>409</td>
</tr>
<tr>
<td>Motya (stray find)</td>
<td>—</td>
<td>1</td>
<td>—</td>
</tr>
<tr>
<td>Messana and Rhegium (overstruck on Athens)</td>
<td>—</td>
<td>a few</td>
<td>450</td>
</tr>
</tbody>
</table>

| (b) Levant              |                |        |                            |
| S. Anatolia<sup>32</sup> | 38             | 19     | 480                        |
| Noe 605 (Latakia)       | 16+            | 14+    | 480                        |
| Lycia<sup>33</sup>      | ?              | ?      | 440                        |
| Lebanon<sup>34</sup>    | 76             | 36     | 430                        |
| Noe 978 (Side)          | 18+            | 1      | 410                        |
| Noe 297 (Dali, Cyprus)  | 60+            | 7      | 410                        |
| Celenderis<sup>35</sup> | 2,000?         | 100+   | 400                        |
| Jerusalem (stray find)  | —              | 1      | —                          |
| Samos (overstruck on Athens)<sup>36</sup> | — | 1 | 450 |
| Citium (Azbaal overstruck on Athens)<sup>37</sup> | — | 1 | 425 |

| (c) Egypt               |                |        |                            |
| Noe 143 (Benha)         | 66+            | 9      | 490                        |
| Noe 1178 (Zagazig)      | 84             | 34     | 470                        |
| Noe 729 (Naucratis)     | 15             | 6      | 430                        |
| Noe 730 (Naucratis)     | 84             | 70     | 400?                       |
| Cyrene (overstruck on Athens)<sup>38</sup> | — | 1 | 500 |

In the above table one or two points require comment. In the West Athenian coins are common enough before 480, but, thereafter, they are not present in mid-century hoards,<sup>39</sup> and begin to reappear in small numbers only at the end of the century. In the Near East, on the other hand, they are present both before and after 480, with a tendency in Egypt, where alone there is sufficient evidence, to form a progressively larger proportion in the hoards, as the century progresses. The conclusion of this trend is to be seen in the numerous Near Eastern hoards buried in the early decades of the fourth century, which

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<sup>30</sup> Imported coins only.
<sup>31</sup> See above, p. 77
<sup>32</sup> See above, p. 80
<sup>33</sup> Owls are known to have been found recently together with mid-century Lycian issues; several Lycian coin-types are derived from Athenian owls.
<sup>34</sup> See above, p. 78
<sup>35</sup> NC 1962, 1 ff.
<sup>36</sup> BMC Ionia, 338, no. 87.
<sup>37</sup> NC 1945, 78 f.
<sup>38</sup> BMC Cyrenaica 1, no. 1.
<sup>39</sup> Noe 611, 953 and 1161 are typical in this respect.
contain high proportions of owls that were both minted and, presumably, conveyed to the East during the fifth century:

<table>
<thead>
<tr>
<th>Region</th>
<th>Total in hoard</th>
<th>Athens</th>
</tr>
</thead>
<tbody>
<tr>
<td>Karaman, Lycaonia</td>
<td>70</td>
<td>69</td>
</tr>
<tr>
<td>Noe 252 (Cilicia)</td>
<td>141</td>
<td>38</td>
</tr>
<tr>
<td>Noe 647 (Marash, Commagene)</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>Al Mina, Syria I</td>
<td>35</td>
<td>18</td>
</tr>
<tr>
<td>Al Mina, Syria II</td>
<td>43</td>
<td>26</td>
</tr>
<tr>
<td>Mesopotamia</td>
<td>25</td>
<td>6</td>
</tr>
<tr>
<td>Malayer, Persia</td>
<td>322</td>
<td>164</td>
</tr>
<tr>
<td>Kabul, Afghanistan</td>
<td>72</td>
<td>34</td>
</tr>
<tr>
<td>Noe 144 (Benihasan, Egypt)</td>
<td>85</td>
<td>64</td>
</tr>
<tr>
<td>Noe 673 (Memphis, Egypt)</td>
<td>39</td>
<td>39</td>
</tr>
<tr>
<td>Noe 1082 (Tel el-Athrib, Egypt)</td>
<td>700</td>
<td>700</td>
</tr>
<tr>
<td>Tell el-Mashkuta, Egypt</td>
<td>8–10,000</td>
<td>8–10,000</td>
</tr>
</tbody>
</table>

There is thus clear evidence that Athenian owls were used in a way different from that of any of the other coinages we have so far examined; unlike them they were regularly dispersed in large numbers to areas far removed from their place of origin. On the evidence of dates of burial this pattern was already well established in the decade 490–80, and probably began even earlier, since coins buried at this time may well represent imports of the preceding decade; by the end of the fifth century owls seem to have become the coins most commonly employed for storing surplus wealth in the east Mediterranean. Dispersal of coins on this scale obviously could not take place without a plentiful and secure supply of silver, and this Athens had in her own mines at Laurium. That there was some realisation of the potentiality of these mines for foreign trade is suggested by the introduction of the owl coinage itself; for the replacement of the changing and inexplicit types of the ‘Wappenmünzen’ by the unchanging owls, which declare so very plainly their Athenian origin, seems to be directed at non-Athenian users. To answer the question whether this idea of dispersing silver in the form of coin rather than retaining it within the area of minting originated in Athens, we must examine the distribution of the coins of the Thraco-Macedonian area.

**Thrace and Macedon**

Within this extensive region there were several areas where silver was mined, and numerous mints, among which the most important were Thasos, Acanthus, Abdera, Aenus, Mende, Lete and the Macedonian tribes; we should, therefore, expect to meet with some evidence for the dispersal of coins outside the area. The movement, however, was not all southward into the Greek world, but partly, at least, northwards as well; two hoards of coins of the Derrones are known, one from S. Yugoslavia (Noe 495) and one from near Pasardjik in Bulgaria, a country which has also produced three apparently early hoards of Thasian coins. The hoards from the Greek world, however, are more interesting,
because in them it is sometimes possible to compare the representation of N. Greek coins with that of coins from other areas or mints.

In the West, with the usual exception of the Taranto hoard, North Greek coins are not common, and are confined to a few examples of Acanthus, and to isolated overstrikes. The very different picture in the Near East can again be shown by a table, drawn from the better recorded hoards, in which the North Greek issues are given a block figure for the area, and compared with the figures for Athens, which is the only other mint to disperse her coinage in quantity:

<table>
<thead>
<tr>
<th>Hoard</th>
<th>Total</th>
<th>N. Greek</th>
<th>Owls</th>
<th>Approximate date</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Ras Shamra</td>
<td>39</td>
<td>33</td>
<td></td>
<td>525</td>
</tr>
<tr>
<td>2. Noe 323 (Demanhur)</td>
<td>165</td>
<td>55</td>
<td></td>
<td>510</td>
</tr>
<tr>
<td>3. Noe 1052 (Taranto)</td>
<td>c. 100</td>
<td>41</td>
<td>5</td>
<td>510</td>
</tr>
<tr>
<td>4. Noe 143 (Benha)</td>
<td>66</td>
<td>31</td>
<td>9</td>
<td>490</td>
</tr>
<tr>
<td>5. S. Anatolia</td>
<td>39</td>
<td></td>
<td>2</td>
<td>480</td>
</tr>
<tr>
<td>6. Noe 1178 (Zagazig)</td>
<td>84</td>
<td>23</td>
<td>13</td>
<td>470</td>
</tr>
<tr>
<td>7. Lebanon</td>
<td>76</td>
<td>3</td>
<td>36</td>
<td>430</td>
</tr>
<tr>
<td>8. Noe 729 (Naukratis)</td>
<td>15</td>
<td></td>
<td>6</td>
<td>430</td>
</tr>
<tr>
<td>9. Noe 739 (Naukratis)</td>
<td>84</td>
<td>2</td>
<td>70</td>
<td>400</td>
</tr>
</tbody>
</table>

From hoards 1–4 it emerges clearly that N. Greek coins were being dispersed in quantity long before owls, and that, therefore, the idea of dispersing locally mined silver in the form of coins came to Athens from North Greece. We may hazard a guess that the Pisistratids, who spent their exile in Macedonia just at the time when this practice was beginning, brought the idea back with them to Athens. Hoards 5–9 show that the appearance of high proportions of owls in hoards was accompanied first by a sharp drop in number and later by an almost total disappearance of N. Greek issues from Near Eastern hoards. We cannot, however, deduce from this that the production of the Thraco-Macedonian mints was greatly curtailed in the fifth century. There were, of course, changes in the pattern: the mints of Potidaea and Lete do not seem to have survived the Persian wars, the coinage of Thasos appears to have ended with the Athenian siege of 463/2, and after the death of Alexander I small change only was minted in Macedonia; on the other hand, Aenus started only after 480, and the principal output of Maroneia was in the fifth rather than the sixth century; Abdera, Acanthus and Mende had substantial issues throughout the period. The fact is that though we can say that N. Greek coins did not travel to the Near East in quantity during the fifth century, our evidence does not yet tell us where, if anywhere, they did go to.

**Asia Minor**

This area need not detain us long, for it produced only one coinage of more than local importance, the Persian siglos. Yet, as Schluumberger has clearly shown on the evidence of hoards, even this was not the 'coinage of the Persian empire', but a strictly regional derive from a single very large hoard found on the site of Mende itself (Noe 522); also the only known hoard to contain a substantial number of coins of Acanthus comes from the neighbourhood of Acanthus itself (Rev. Belge 1949, 42).

58 L'argent grec dans l'empire Achéménide 12 ff.; to his material may now be added Noe, 'Two hoards of Persian sigloi' (NNM 136) and a hoard from the American excavations at Gordium.
currency on the Greek model for use among the Greek subjects of Persia in Western Asia Minor; from this area only come hoards composed of sigloi in substantial numbers, while elsewhere in the Persian empire sigloi are no more numerous than many imported Greek issues; in the numerous hoards from Egypt they are virtually unknown.

THE NEAR EAST

To complete our survey of the movement of Greek coins a word must be said about the Near East, for large numbers of Greek coins found their way to this area.\(^{57}\) By the end of the fifth century such finds are composed very largely of Athenian owls, but the composition of earlier hoards is extremely mixed without any single mint or area predominating. The older interpretations of such accumulations, as representing the stock of silversmiths or loot from the Persian Wars, are inadequate to explain a phenomenon which extends over the whole Persian empire from Syria to Afghanistan. These hoards are now seen to be accumulations of wealth of a type to be expected in an area which did not use coinage; they are collections of precious metal, in which ingots or fragments of jewellery or plate are normally present; coins are included, not because they have a value as coins, but because they are another source of precious metal. This is born out by the fact that such coins have often been mutilated to a degree which far exceeds the needs of a routine test for purity; their character as coins has been effectively destroyed, and only their value as bullion remains.

These Near Eastern hoards, with their mutilated coins and their miscellaneous scraps of precious metal, differ from most Greek hoards in that the latter are tied by their coins to the area of issue, whereas there is no such restriction on the movement of the former. Though they are of great interest because they have restored to us many coins otherwise unknown, they tell us little about the Greek world. They simply show that Greek coins were attracted to the Near East as scrap silver; their very mixed character provides no information about how they got there or who brought them.

CONCLUSION

For most Greek coinages our survey has produced no evidence of their occurrence outside their home area in more than isolated specimens. This result can be reinforced by a number of instances in which a very large proportion of the now surviving specimens of an issue or group of issues is known to have come from a find made in the neighbourhood of the issuing mint. Outstanding examples of this phenomenon are:

1. All the later-fifth-century Melian stater known today come from a single find made on Melos itself (Noe 672).
2. All but a very few of the surviving sixth- and fifth-century tetradrachms of Mende derive from a single hoard found in the neighbourhood of Mende (Noe 522).
3. Most of our archaic Parian drachmae come from a hoard found on Paros about 1941.\(^{58}\)
4. Chalcidian League tetradrachms are known mainly from hoards found in the excavations of Olynthus (Noe 757, 758, 760, 763, 764).
5. Finds of Cypriot coins in quantity are confined to Cyprus itself (Noe 297, 601).
6. A recent find in Lycia has brought to light a whole range of hitherto unknown issues of fourth-century Lycian dynasts (NC 1959, 32 ff.; Rev. Num. 1961, 18 ff.), and, in general, the very numerous Lycian issues rarely appear in non-Lycian finds.
7. Before about 1855 the now very common fifth-century drachmae of Cnidus were

\(^{57}\) Schlumberger, op. cit. in n. 56, p. 6 ff.
\(^{58}\) Lederer, Neue Beiträge zur antiken Münzkunde 44.
virtually unknown; about this time a very large hoard seems to have been discovered, probably at or near Cnidus.59

No doubt such a list could be considerably extended if our record of hoards were more complete.

To this general picture of the immobility of Greek coinage we have found two exceptions, both associated with silver-producing areas. First from the Thraco-Macedonian area and later from Athens silver was dispersed in the form of coins, which are consequently found in quantity far from their areas of origin.

2. The Incidence of Fractional Coinages

Any attempt to determine the way in which early coinage was originally used must depend, at least in part, on the extent to which the smaller denominations were minted. Unless there was a supply of low value coins sufficient for the needs of daily retail trade, coinage cannot have been used in anything like the way with which we are familiar today. There are, however, certain difficulties in the way of obtaining a clear view of the extent of such coinages. The first is that the great majority of ancient coins reach us through hoards, which represent deliberate accumulations of substantial sums. Since such accumulations are normally made in units of high value rather than in small change, there is a possibility that for some mints our knowledge of the fractions may be deficient, even though it is virtually complete for the larger denominations. This possibility is reinforced by the modern tendency to make detailed studies of the major denominations only, so that the full range even of the existing fractions may not be readily available to students. Nevertheless, despite these hazards, estimates can be made for a number of the more important mints, and conclusions drawn from these will probably be valid for most of their humbler neighbours. Our survey will follow the same sequence of areas as before.

The West

Italy. The four principal S. Italian mints show certain differences in their pattern of minor denominations. Sybaris, by the time of her destruction in 510, had a supply of at least drachms and obols in addition to the stater; neither of these denominations seem particularly rare, but in the absence of a detailed study of the mint no figures for dies are available. Metapontum, likewise, had drachms and obols, and in the fifth century triobols and diobols were added as well; a recent find, dating from about 430, has added a number of hitherto unknown types for the obol.60 Yet even at these two mints at which the smaller denominations are comparatively well represented, they seem rare in comparison with the number of surviving staters.

In contrast Caulonia and Croton have comparatively few fractions. Croton, the more important mint, but as yet not studied in detail, has a very considerable output of staters, which appear to be accompanied by practically no fractions down to about 480; thereafter (the dumpy incuse phase) there appear a few drachmae, a number of triobols, a very few diobols, and very rarely an obol.61 Caulonia has been studied in detail,62 so that some figures can be offered in terms of obverse dies used:

<table>
<thead>
<tr>
<th></th>
<th>Staters</th>
<th>Drachms</th>
<th>Smaller</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Incuse coinage (c. 530–475)</td>
<td>33</td>
<td>5</td>
<td>3</td>
</tr>
<tr>
<td>2. Double relief (c. 475–388)</td>
<td>73</td>
<td>11</td>
<td>1263</td>
</tr>
</tbody>
</table>

59 Information from Dr H. A. Cahn, Basel.
60 Sch. Münzb. 1956, 45 ff. and 1957, 73 ff.
62 Noe, The Coinage of Caulonia (ANS Numismatic Studies no. 9).

63 A bronze coinage was also used during this period, but as only some four specimens are known, it does not materially affect the picture.
The subsidiary coinage thus appears to have been on a very small scale compared to the staters.

From such a study we can also gain an idea whether our surviving sample is reasonably representative. If, for example, the sixteen obverse dies known to have been used for drachms at Caulonia were now represented by only one coin each, there would clearly be a strong likelihood that a new find of drachms would contain at least several obverse dies hitherto unknown, and that therefore the output of drachms may perhaps have been very considerably larger than our surviving evidence suggests. If, however, ten coins survive from each die, the chances that further finds will reveal many new dies is much reduced.\(^{64}\) The actual number of specimens recorded by Noe for each drachma die is as follows:

\[
\begin{align*}
\text{Period 1:} & & 1, & & 2, & & 4, & & 6, & & 2. \\
\text{Period 2:} & & 2, & & 1, & & 8, & & 11, & & 6, & & 1, & & 2, & & 9, & & 2, & & 1, & & 1.
\end{align*}
\]

These figures suggest that further finds may include some hitherto unknown dies, but that these are not likely to be very numerous. On the other hand, most of the denominations below the drachma survive in only one or two specimens per die, so that the chance of new dies appearing is here much greater.

**Sicily.** Syracuse is the obvious starting-point, for it was the most productive mint in the island, and has also been the subject of very detailed study.\(^{65}\) For the late sixth century and the first three-quarters of the fifth no less than 244 obverse dies for tetradrachms are recorded but only seven each for didrachms and drachms. These last two are represented by the following numbers of specimens:

\[
\begin{align*}
\text{diddrachms:} & & 4, & & 3, & & 55, & & 35, & & 1, & & 1, & & 2. \\
\text{drachms:} & & 13, & & 15, & & 1, & & 12, & & 25, & & 7, & & 2.
\end{align*}
\]

These make it unlikely that many more dies for these two denominations remain to be discovered.

Below the drachma there is a scatter of small denominations, principally obols, litrae and half-litrae, but little sign of regular or systematic supply. The evidence appears to justify the following statements:

1. **Before about 480,** though over one hundred dies had been used for tetradrachms, these had been accompanied by only four dies for didrachms, one for drachms, and none at all for any smaller denomination.
2. **In the years round 480** the obol was minted in considerable quantity (more than seventy dies being used), but at no other time in the period does this denomination occur.
3. Similarly the litra was struck in quantity only between about 470 and 460.
4. **Finally,** towards the end of the period there are a certain number of half-litrae, perhaps supplemented by bronze.

The coinage of Gela from about 490 to its destruction by the Carthaginians in 405 has been studied by G. K. Jenkins.\(^{66}\) Down to 481 thirty dies for didrachms are known, and thereafter sixty-five for tetradrachms. The period of the didrachms is wholly devoid of fractional coinage, as was the case at Syracuse. Later there are two heavy issues of litrae, one in the years around 460 (about sixty obv. dies), and the second in the years around 430 (about fifty-four obv. dies). At other times virtually no small change was being minted.

\(^{64}\) The chief disturbing factor is likely to be an uneven incidence of hoards within a period, so that whereas one part of it may be well covered, another may lack a representative hoard.

\(^{65}\) Boehringer, *Die Münzen von Syrakus* (1929).

\(^{66}\) I am grateful to him for allowing me to use here his unpublished figures for the frequency of dies.
nor is there any suggestion that the available litrae can be subdivided into small issues, which could be distributed over a long period.

In the absence of a detailed study, no figures are available for Acragas, but once again the supply of fractions seems both small and irregular. A number of mints, such as Camarina and Catana, from time to time produce sizeable issues of fractions, but nowhere does there seem to be a continuing output of small denominations in the range and quantity required to sustain anything like general use in retail trade.

CENTRAL GREECE

In this area the supply of fractions seems to have been much greater than in the West. For Aegina, no figures are available, but both drachms and obols of early date are commonly met with, and, in addition, triobols and hemiobols are numerous in the fifth century. At Athens, likewise, some at least of the ‘Wappenmünzen’ issues are well supplied with fractions down to the quarter-obol; the wheel and Gorgoneion issues are particularly common, and the scantier representation of some of the other types is probably due simply to lack of evidence. The very large succeeding issues of archaic owls seem to be almost devoid of fractions, but, as has been seen above, they were intended mainly for foreign trade, in which the smaller denominations had no place. But by the end of the fifth century, literary and epigraphic allusions combine with the surviving coins to show that Athens possessed an exceptionally full range of silver fractions down to the minute eighth of an obol, and that such coins were used in daily transactions at least in the city of Athens itself.

At Corinth, on the other hand, the record of fractional coinage is much less complete. All periods have some fractions, but some periods seem to have very few indeed, even when there was a substantial output of staters. In the archaic period fractions are relatively common, but there are few that can be attributed to the decades 480–430. In the latter part of the century little coinage was apparently being produced at Corinth, but the considerable output of the first half of the fourth century seems to have been almost wholly in staters. A striking parallel is afforded by Ambracia, from which not a single fraction is known from its beginning, about 480, down to its capture by Philip of Macedon in 338; the entire output of the mint was devoted to Corinthian type staters.

THRACE AND MACEDONIA

We have seen that this was an area which produced silver, and which exported it to the E. Mediterranean and elsewhere in some quantity in the archaic period; for such a purpose large denominations were required, and these are, in fact, conspicuous in the area; nowhere there is a full range and quantity of fractions. For example, Aenus, between 475 and 450, coined virtually only tetradrachms and diobols, both in some quantity, and from about 435 to the end of the century the output of diobols was continuous and sometimes very considerable. Yet no single coin of a lower value than the diobol is known. If Athens required a coin as small as an eighth of an obol, one wonders what was used at Aenus for values below two obols.

At Abdera from 540–411 the range of denominations is greater, but the number of dies still rather meagere for the smaller values:

<table>
<thead>
<tr>
<th>8 dr.</th>
<th>4 dr.</th>
<th>2 dr.</th>
<th>dr.</th>
<th>4 ob.</th>
<th>3 ob.</th>
<th>ob.</th>
<th>½ ob.</th>
</tr>
</thead>
<tbody>
<tr>
<td>30</td>
<td>118</td>
<td>1</td>
<td>16</td>
<td>6</td>
<td>11</td>
<td>14</td>
<td>2</td>
</tr>
</tbody>
</table>

67 See BMC Attica, etc., 126 ff. for denominations at different periods; the dates there given are too high.
68 Seltman, Aenus, 151 ff.
69 May, Aenus 8 and 70.
70 The diobol at Aenus was slightly lighter than at Athens.
71 These figures are taken from an as yet unpublished study by the late J. M. F. May.
There is in any case a great disproportion between the number of tetradrachm dies and those of the smaller denominations. In Macedonia both Alexander I and Perdiccas minted considerable quantities of tetrobols, but this was the lowest significant denomination, for very few smaller coins (which seem to be obols) are known. At other mints in the area a similar lack of small change is to be seen, as at Thasos, Mende and Acanthus.

**Asia Minor**

Here the Persian siglos, as the successor of the Lydian silver of Croesus, was the standard silver unit, but fractions of the siglos are extremely uncommon. These lower denominations, however, were supplied locally by copious issues, mainly of quarter-obols, from a number of mints such as Colophon, Teos, Ephesus, Erythrae and others which are not certainly identified. At Colophon the denominations were clearly marked with monograms indicating their values as half, third or quarter-obols. This tradition of the use of very small fractions was clearly derived from the electrum coinages of the sixth century, in which fractions as small as one ninety-sixth of a stater were commonly minted.

This brief survey is, admittedly, based on evidence which at some points is still incomplete; yet it is hard to avoid the conclusion that few parts of the Greek world in the sixth and fifth centuries B.C. were equipped with a sufficient range and volume of low value coins to cater for the needs of daily retail trade. In some cases the denominations exist, but in quantity so small that they can have made little impression on the total currency; in other cases there is a considerable output of a denomination such as the tetrobol or diobol, but nothing to represent still lower values. Only at Athens, Aegina and in Ionia does there seem to be anything like a continuing and plentiful supply of the smallest denominations.

**3. The Original Function of Early Greek Coinage**

The first part of this paper showed that most Greek coinages tended to stay within the areas in which they were minted; to this general statement there were two exceptions, the Thraco-Macedonian mints and Athens, both silver producers, who had surplus metal to dispose of. Neither of these exceptions operated at the very beginning of coinage, but only when the idea of affixing distinctive types to weighed lumps of metal was already well established. It would therefore seem that, since most coinages were not exported, and since those that were exported were not among the earliest coinages, the original intention in striking coins was not to facilitate foreign trade, or to provide merchants with a means of purchasing goods or materials not available locally. This is not to deny that bars or ingots of silver formed an article of trade, for the possession of silver mines was a stroke of rare good fortune; the majority had to obtain their silver from abroad by trade or service or loot. But it would seem that once silver was converted from bullion into coin, it was virtually withdrawn from all but local trade (unless it were Thraco-Macedonian or Athenian coin). The problem remains, why did the Greeks require coinage, in addition to the undoubted trade in bullion; if coinage was thought to facilitate foreign trade, why were the Persians (except for their localised siglos), the Etruscans, the Carthaginians and the Phoenicians so slow to adopt it? We must conclude that coinage was not devised to meet the needs of foreign trade, and that, in so far as it came to do so, this was a secondary development.

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72 Raymond, 'Macedonian Regal Coinage to 413 B.C.' (NNM 126). A light and a heavy tetrobol were struck; the former was of impure silver and may have been intended for local circulation only (Kraay, *The Composition of Greek Silver Coins* 20 ff.).

73 Kraay, 'Monnaies provenant du site de Colophon', *Rev. suisse de Numismatique* xlii 5 ff.
Next must be examined the possibility that coinage was intended to facilitate internal trade.\textsuperscript{74} That some such trade took place within areas is proved by the fact, for example, that coins of Acras may be found in the territory of Syracuse, but the idea that coinage served at all widely for daily retail transactions, as it does today, must be rejected on several grounds. First, the survey in part two of this paper has shown that at this date few of even the most important Greek states possessed a regular supply of small denominations adequate for this purpose. Second, very many places, especially in the sixth century, had no coinage of any sort. And third, coinage originated, not among the silver-using states of mainland Greece and the West, but with the electrum issues of Asia Minor; these, even in their smallest fractions, must certainly have represented values much higher than those required for retail trade. The use of coinage in retail trade which, even at the end of the fifth century, was confined to a few of the more economically advanced states, cannot be regarded as its original purpose.

The view that coinage was originally designed to serve the needs of either local or foreign trade, is contradicted by the evidence of circulation and of the incidence of fractions, and, indeed, it is unlikely that in the seventh and sixth centuries B.C. traders were anywhere either so influential or so organised as to be able to secure the public and official adoption of a device designed primarily to serve their interests. For the official nature of coinage is one of its most striking features; wherever it can be identified, the design placed on a coin is the badge of the political authority which issued it. Admittedly, many early types, in the absence of legends, are so inexplicit that they defy attribution, but this constitutes no reason for postulating the activity of private bankers or merchants. If, then, coins are struck by the ruler or governing body of a state, and bear that state's or ruler's distinctive badge, the original purpose of coinage will surely be in some way to the profit or convenience of the governing body or individual.

In the seventh and still more in the sixth century life became very much more complex in the Greek world than it had been previously. Largely through the foundation of colonies the area actually occupied by Greeks became enormously greater, and relationships between different parts of the area much more elaborate. Raw materials and manufactured products were increasingly traded, and in many places a new level of wealth was attained. Evidence of this is to be found in both the public and the private sphere, in the quality of dedications and funeral stelae, no less than in the growing splendour of the temples. In such a world the functions of government were also becoming more complicated, and the number and kind of occasions on which official payments had to be made or received were rapidly increasing. Among receipts may be mentioned the harbour dues which are said to have formed the main revenue of the Bacchiadids and Cypselids at Corinth,\textsuperscript{75} the fines and penalties which are commonly threatened in laws,\textsuperscript{76} and any other taxes.\textsuperscript{77} Payments will have included those occasions on which a surplus was divided among the citizens, as at Siphnos and Athens,\textsuperscript{78} the pay of mercenaries or soldiers,\textsuperscript{79} salaries paid to experts,\textsuperscript{80} and expenditure on public works; the latter category includes both payments for materials and to craftsmen and labourers.

\textsuperscript{74} The term 'internal' must be understood to cover both transactions within a city and those between cities within the same area, such as S. Italy or Sicily.

\textsuperscript{75} Strabo 378; Aristotle, fr. 611, 20.

\textsuperscript{76} The so-called Constitution of Chios (BSA li (1956) 157) and the Eretrian laws (IG xii 9, 1273-4) are sixth-century examples.

\textsuperscript{77} E.g., the 5 per cent duty levied by Athens on all imports and exports in 413 (Thuc. vii 28.4); some taxes were levied in kind (Ath. Pol. 15.4).

\textsuperscript{78} Her. iii 571; vii 144. Thasos also enjoyed a considerable income from mines under her control (Her. vi 46).

\textsuperscript{79} In a stimulating article in Historia vii (1958) 257, R. M. Cook suggests that this was indeed the original purpose of coinage; I have applied here to a wider area many of the points which he makes in relation to Lydian electrum.

\textsuperscript{80} Her. iii 131.
Before the invention, or in the absence of coinage all these transactions could, in principle, be performed with weighed quantities of precious metal, but the more numerous such transactions became, the more cumbrous this method of payment. It had the further disadvantage that though the 'government' could know what it was paying out, it could not so easily determine the quality of what it received. This difficulty was most easily overcome if the 'government' could insist on payment in units which it had itself created, and of which the quality was therefore known. The adoption of such a system would have to be a deliberate act in each community. The first step would be to convert existing bullion into coin of determined weight and purity, and then to enact that official transactions should be conducted only with such χρήματα δόκιμα.\textsuperscript{81}

After these preliminaries the coins could be put into circulation through the various types of payment mentioned above; in addition, the supply could be increased by individuals, who required the coins for any purpose, bringing their own bullion to the mint to be converted into coin. So far coins will have been only a matter of convenience to the 'government', but it is perhaps at this point that the motive of profit enters as well, for the service of converting bullion into coin will not have been performed free of charge; and the charge made may easily exceed the actual cost of the operation. The economic result of this would be that the value of a piece of coined metal would exceed that of a similar weight of uncoined metal, not only by the cost of coining, but by the amount of the 'government' profit as well.

That current coined metal did enjoy a premium can be seen from the story of Hippias of Athens in the \textit{Oeconomica}.\textsuperscript{82} 'he declared the existing coinage to be ἄδοκιμον, he fixed a price for it and ordered it to be brought to him'. By declaring it no longer legal tender, he cancelled the premium enjoyed by the existing coinage, thus reducing its value to that of bullion, at which price he acquired it. Normally the next step would have been to recoin with a distinctive type the bullion thus acquired, and to reissue it with a fresh premium. Hippias, however, went one better, and reissued the old coinage unchanged, thus levying a second premium, without incurring the cost of recoinage; the fact that any who dared to ignore his original order automatically found themselves possessed of the new coinage without having paid the premium, presumably ensured that this particular device should not be tried too often.

We can also now explain why so few coins are found outside their areas of issue. The premium arises from the necessity of using a coin for certain purposes in a particular place; where that necessity no longer exists, the premium will disappear, and the coin will be worth no more than its bullion value. This will not prevent coins going abroad, for it may well be that the coins of A will be in some demand at B, in order that the traders of B may pay their harbour-dues at A; but, it will tend to keep current coins in the area in which their value is greatest, or to attract them back to it, if they have been exported. In cases in which the coinage did go abroad in quantity, as from Athens, we must suppose that the premium was either non-existent or was kept very low, because here the object was to encourage the dispersal of a locally plentiful commodity, rather than to retain for local use an expensive import.\textsuperscript{83}

I have argued that coins were not invented for the purpose of any sort of trade; once invented, however, they will have become liable to be traded like any other object to which value is attached, but beyond this they will have had little effect on the way in which trade was carried on. It is often claimed that coins obviated the need for weighing in each transaction, and this is no doubt true of those places which produced a range and quantity

\textsuperscript{81} This is the phrase used in the Eretrian laws (n. 76 above); compare the use of ἄδοκιμον in the account of Hippias' so-called reform in \textit{Oec.} 1347a 8.

\textsuperscript{82} 1347a 8.

\textsuperscript{83} At both Athens and Siphnos the mines were owned by the state; ample resources were thus available to cover the costs of minting.
of small denominations sufficient to supply the needs of retail trade; but we have seen that such places were very few in the period under discussion. Elsewhere, irregularity of weight, variety of standard, the hazards of wear, clipping or plating must surely have required the continuation of weighing in almost every transaction. Reputation for purity and regular weight may have made one coinage more acceptable than another, but acceptance at face value in private trade is unlikely to precede a similar acceptance in official transactions, and few 'governments' are likely to have taken the coinage of their neighbours on trust. In fact, weighing can hardly have been widely dispensed with in major transactions before the age of extensive kingdoms or empires, throughout which royal or imperial coinages were legal tender.

It may be objected that for much of what I have said about the original purpose of coinage, there is really very little evidence. This, however, is true of any interpretation which can be put forward; our only evidence is the coinage itself, its characteristics and behaviour, illuminated by occasional literary or epigraphic references. My contention is that Greek coinage in its earliest phases has often been treated as though it were essentially like modern coinage and capable of covering much the same range of transactions, whereas the lack of smaller denominations makes this impossible. At the same time a concentration of attention on the few coins which travelled abroad rather than on the great majority which stayed at home, has exaggerated the part which foreign trade is supposed to have played in the origin of coinage, and obscured its domestic functioning for the convenience and profit of the issuing authority. Only in a rare case such as Athens, where the source of silver was within the states' territory, could coinage be deliberately destined for foreign trade, once internal needs were supplied; at Athens the realisation of this additional purpose seems to be marked by the replacement of the inexplicit and changing types of the 'Wappenmünzen' by the unchanging owl coinage, which so clearly proclaims its origin.

Ashmolean Museum, Oxford.

Colin M. Kraay.
IN GREEK PHILOSOPHY

In a previous article (JHS lxxxii (1962) 56 ff.) I examined some of the theories and explanations which appear in Greek philosophy and medicine in the period down to Aristotle, in which reference is made to right and left or certain other pairs of opposites (light and darkness, male and female, up and down, front and back), and I argued that several of these theories are influenced by the symbolic associations which these opposites possessed for the ancient Greeks. In the present paper I wish to consider the use of the two pairs of opposites which are most prominent of all in early Greek speculative thought, the hot and the cold, and the dry and the wet. My discussion is divided into two parts. In the first I shall examine the question of the origin of the use of these opposites in Greek philosophy. How far back can we trace their use in various fields of speculative thought, and what was the significance of their introduction into cosmology in particular? And then in the second part of my paper I shall consider to what extent theories based on these opposites may have been influenced by assumptions concerning the values of the opposed terms. Are these opposites, too, like right and left, or male and female, sometimes conceived as consisting of on the one hand a positive, or superior pole, and on the other a negative, or inferior one? How far do we find that arbitrary correlations were made between these and other pairs of terms, that is to say correlations that correspond to preconceived notions of value, rather than to any empirically verifiable data?

I

On the problem of the origin of the use of these opposites in various types of speculative theories, it is as well to begin by reviewing the incontrovertible evidence for the post-Parmenidean period before turning to the much less certain and more difficult evidence for earlier writers. The first extant philosophical texts in which the hot and the cold, the dry and the wet are explicitly described as being present at the first stage of cosmological development are certain fragments of Anaxagoras. Fr. 4, for instance, specifically mentions these opposites, along with the bright and the dark, ‘much earth’ and ‘innumerable seeds’, as being present in the original mixture when ‘all things were together’, and the same three pairs of opposites, together with the rare and the dense, are described in fr. 12 as separating off from one another in the περιχώρησις initiated by Nous. The first extant text in which we find a physical theory based on the hot and the cold, the dry and the wet as the four primary elements or components of other things appears to be in the Hippocratic treatise On the Nature of Man, ch. 3 (L vi 36 17 ff.) where the author states that on death each of the components of a man’s body returns to its own nature, τὸ τε ὑγρὸν πρὸς τὸ ὑγρὸν καὶ τὸ ἔριδον πρὸς τὸ ἔριδον καὶ τὸ θερμὸν πρὸς τὸ θερμὸν καὶ τὸ ψυχρὸν πρὸς τὸ ψυχρὸν. τοιαύτη δὲ καὶ τῶν

1 The reference to Melissus in ch. 1 (34 6) provides a terminus post quem for chs. 1–8 of this composite treatise, and to judge from the report in Anon. Lond., xix 1 ff., its author was Polybus, the son-in-law of Hippocrates (the theory of veins in ch. 11, 58 1 ff., may also be ascribed to Polybus on the authority of Aristotle, HA 512b 12 ff.). While most scholars date this work in the second half of the fifth century (e.g. L. Bourgey, Observation et Expérience chez les médecins de la collection Hippocratique, Paris, 1933, 31 ff.), F. Heinemann for one (Nomos und Physi, Basel, 1945, 158 ff.) puts it at the very end of that century or the beginning of the fourth, on the grounds that the antithesis between νόμος and φύσις is already drawn quite sharply in chs. 2 and 5.
In other fifth- and fourth-century texts these opposites are associated with the elements, rather than named as elements themselves. In fr. 21 3–6 Empedocles connects certain pairs of opposites (hot and cold, bright and dark) with certain of his 'roots', though how far he went in working out a comprehensive schema of such associations, it is much more difficult to say. But in the Hippocratic work On Fleshes, for example, we find a quite elaborate schema (ch. 2, L vii 584 g ff.). Here too there is a four element theory, the most important element being the hot itself ('what the ancients called ἄθροι'). Of the other three elements, earth is said to be cold and dry, ἄθροι is hot and wet, and 'that which is nearest the earth' (the sea?) is 'wettest and thickest'. Another such theory is found in On Regimen where the writer asserts that there are two component elements of all living creatures, namely fire and water, but 'the hot and the dry belong to fire, and the cold and the wet to water', though 'there is some moisture in fire' and again 'there is some of the dry in water' (chs. 3 and 4, L vi 472 12 ff., 474 8 ff.).

The evidence for the post-Parmenidean period is clear enough. In the late fifth and early fourth centuries the hot and the cold, the dry and the wet, whether alone or in conjunction with other pairs of opposites, were used in at least two types of physical theories, (1) where they figure as elements themselves, i.e. as the primary substances of which other things are composed (as in On the Nature of Man) and (2) where they are associated with the elements, when these were conceived in the form of other substances (as in On Fleshes).

In ch. 4 (38 19 ff.) the writer describes the components of the body in terms of the four humours, blood, phlegm, bile and black bile, but these are in turn correlated both with the opposites and with the four seasons in ch. 7 (46 9 ff.): e.g. blood, like the season spring, is said to be wet and hot.

'Sun' (i.e. fire) is undoubtedly described as bright and hot in verse 3 (δέλεν μὲν λέον όρε καὶ θερμών ὑπάντη) and 'rain' (i.e. water) as dark and cold in verse 5 (δοξίδον δ᾿ εν πάση δυναστείᾳ τε βραδίλλων τε), and when Aristotle quotes these lines at GC 314 b 20 ff., he adds that Empedocles 'characterizes the other elements too in a similar way'. But in the more complete version of the fragment quoted by Simplicius (in Ph. 159.13 ff.) the interpretation of the lines which refer to the other two elements is far from clear. Verse 6 (ἐκ δ᾿ αὐτὸς προσόντος θελεμάτω καὶ στεροσεύτω) seems to associate earth with solidarity, particularly (but not, apparently, with hot or cold): Aristotle, at least, took Empedocles' theory to have been that earth is heavy and hard (GC 315 a 10 ff.).

Verse 4 is even more obscure: ἄμβροτα δ᾿ ὕσσον εἶναι τε καὶ ἀργήτη δεῖ ταύτα κατά. If we take this to refer to air (though this has been doubted by some scholars), it seems to imply that this element, too, like fire apparently, is warm and bright.

4 C. H. Kahn, Anaximander and the Origins of Greek Cosmology, New York, 1960, 127, has suggested that 'if the complete poem of Empedocles had survived, we might see that his theory was as fully articulated as that of Aristotle' (in which each of the four simple bodies is constituted by two of the four primary opposites, hot, cold, dry, wet, e.g. fire by the hot and the dry), but this is a very doubtful conjecture. Kahn points out that 'other fragments permit us to catch a glimpse of the causal roles ascribed to the hot and cold, the dense and rare, and to the qualities of taste—bitter, sweet and others', and he instances frs. 65, 67, 75, 90 and 104. But there is nothing in these fragments to suggest that these opposites are associated with specific 'roots'. Empedocles' theory of these associations survives in fr. 21 alone, and there the doctrine seems to be still in quite a rudimentary form.

5 On the date of this treatise, see K. Deichgräber, Hippokrates, über Entstehung und Aufbau des menschlichen Körpers, Leipzig, 1935, 27 n. 4, who concludes that it belongs to the end of the fifth century. It should be remembered, however, that the date of this, as of almost every other, Hippocratic treatise cannot be determined with any degree of precision.

6 This treatise is now generally agreed to be more probably a fourth- than a fifth-century work (see, e.g., J.-H. Kühn, System- und Methodenprobleme im Corpus Hippocraticum, Hermes Einzelschriften 11, 1956, 80 n. 1).

7 Anon. Lond. xx 25 ff. attributes a neat schema of elements and 'powers' to Philistion: there are four elements and each of these has its own 'power', fire, hot, air cold, water wet and earth dry. The dates of On Fleshes and of Philistion are, of course, known only very imperfectly, but it seems possible that the theories of Empedocles, On Fleshes, Philistion and Aristotle represent a gradual and continuous progress towards a doctrine of elements and opposites that is at once simple and comprehensive.

8 We should also note theories of the type found in Diogenes of Apollonia in which there is a single primary element (in his case ἄθρος) but hot and cold, dry and wet and so on are mentioned as its differentiation (see fr. 5: ὅτι γὰρ πολύτροπος οὐκ ἐν πατρί καὶ θερμότερος καὶ ψυχρότερος καὶ ἐξωτερικῷ καὶ ἐσωτήρην κόσμον ἔχων).
In addition, there are, of course, innumerable pathological theories in the Hippocratic Corpus, in which diseases are attributed either to these opposites directly, or to their effect on other substances or parts of the body. To mention just two typical examples, in On the Places in Man the writer says that ‘pain is caused both by the cold and by the hot, and both by what is in excess and by what is in default’ (ch. 42, L vi 334 ff.), and the author of On Affections puts it that ‘in men, all diseases are caused by bile and phlegm. Bile and phlegm give rise to diseases when they become too dry or too wet or too hot or too cold in the body’ (ch. 1, L vi 208 ff.). The author of On Ancient Medicine (writing after Empedocles, as we know from ch. 20, CMG 1.1 51 10) attacks the extreme version of such a theory in which the hot, the cold, the wet or the dry was postulated as the sole cause of diseases, and it would appear from a text in Aetius, at least (v 30.1, DK 24 B 4) that the use of these opposites in theories of disease goes back as far as Alcmaeon, who is reported to have held that health depends on the ̀ιόνωμια of certain factors in the body, and that the ̀μοναρχία of one of these causes diseases: as examples of the factors, or as he calls them the ‘powers’, in question, Aetius mentions wet, dry, cold, hot, bitter and sweet.

Hot and cold, dry and wet seem to have been used in pathological theories as early as Alcmaeon, but how far back can we trace their use in cosmological doctrines? With the exception of some fragments of Heraclitus (notably fr. 126), we rely on the testimony of Aristotle and Theophrastus, and sources which were in turn dependent on them, for our evidence concerning the role of these opposites in pre-Parmenidean writers. For many years these authorities were thought quite reliable enough by the majority of scholars. What might be described as the traditional view of the role of these opposites in the early stages of Greek philosophy is expressed by Cornford, for example (Principium Sapientiae, Cambridge, 1952, 34): ‘in studying Anaximander’s system we shall find the four cardinal opposites mentioned by Hippocrates [Cornford has just referred to the treatise On Ancient Medicine]—hot and cold, moist and dry—playing a leading role in cosmogony. They can be identified with the four seasonal powers of summer and winter, rain and drought; and, in the order of space, they become the four elements of Empedocles, fire, air, water, earth.’ Yet first Heidel9 and Cherniss10 and then more recently McDermid11 have seriously impugned the accuracy of both Aristotle’s and Theophrastus’ accounts of Presocratic philosophy. Moreover it was just at this point, in his attribution of theories based on opposites to the Presocratics, that Aristotle’s interpretation was particularly attacked on the grounds that in Physics A, for example, he is attempting to show that all earlier thinkers held the opposites as principles in order to establish that his own theory of Form and Privation is both final and inclusive. Commenting on the accounts of Anaximander given by Aristotle and Theophrastus, McDermid put it that ‘their interpretation clearly presupposes that he had subscribed to the specifically Aristotelian notion of the equilibrium of the contraries and the genesis of the four simple bodies through the interaction of the contraries on undifferentiated matter’ (op. cit., 99), and Hölscher,12 too, remarked, with reference to the report on Anaximander in pseudo-Plutarch Stromateis 2 (DK 12 A 10), that ‘die gegensätzlichen Qualität en Heiss und Kalt können als solche nicht anaximandrisch sein’. Nevertheless both Kahn13 and most recently Guthrie14 have upheld the general reliability of our evidence concerning the role of opposites in the systems of the Milesians.

10 H. Cherniss, Aristotle’s Criticism of Presocratic Philosophy, Baltimore, 1933.
The general problem of the reliability of our sources for Presocratic philosophy cannot be entered into here. But since there is a quite fundamental disagreement, among those who have studied the matter, on whether the hot and the cold, and the dry and the wet themselves were used in cosmological theories as early as Anaximander, a brief re-examination of the evidence that is relevant to this particular question must be undertaken.

The problem hinges on our interpretation of the evidence concerning Anaximander, and first of all we should grant a general point to McDiarmid and others, that on certain occasions when Aristotle or the doxographers have attributed a theory based on contraries, or on the hot and the cold in particular, to one or other of the Presocratic philosophers, they have undoubtedly reformulated the theory in question. A clear example is Ph. 188a 20 ff. (cf. Metaph. 986b 34) where, indeed, Aristotle himself makes it plain that he has reformulated the doctrine of Parmenides. καὶ γὰρ Παρμενίδης θερμὸν καὶ ψυχρὸν ἀρχάς ποιεῖ, he says, and then goes on ταῦτα δὲ προσαγόρωμεν πῦρ καὶ γῆν.15 The report in Hippolytus (Ref. i. 7 3, DK 13 A 7) on Anaximenes’ cosmology contains what seems to be a similar rationalisation of an original theory in terms of the hot and the cold, for having described the changes which air undergoes as it rarefies and condenses (becoming fire on the one hand, and wind, clouds, water, earth and stones on the other), Hippolytus concludes with the inference: ὅστε τὰ κυριῶτά τῆς γενέσεως ἐναντία εἶναι, θερμῶν τε καὶ ψυχρῶν.16 As regards Anaximander himself, the main sources of evidence on which we must base our interpretation of the role of opposites in his system are (1) the reports in Aristotle and Simplicius, (2) the account of his cosmology in pseudo-Plutarch and (3) the extant fragment. We may consider these in turn to see how far they enable us to determine the nature of his cosmological theories and the terms in which they were expressed.

Anaximander is rarely mentioned by name in Aristotle,17 and of the passages in question only one (Ph. 187a 20 ff.) is relevant to the problem of his use of opposites. At Ph. 187a 12 ff. Aristotle divides the theories of the φυσικοὶ into two groups. One group of philosophers postulated a single element and derived other things from this by condensation and rarefaction, and the other group is described as follows: οἱ δ’ ἐκ τοῦ ἐνὸς ἐνοσίας τὰς ἐναντιότητας ἐκκρίνονται, ὅσπερ Ἀναξιμανδρὸς φησι, καὶ ὁσοὶ δ’ ἐν καὶ πολλά φασιν εἶναι, ὅσπερ Ἐμπεδοκλῆς καὶ Ἀμαζγόρας: ἐκ τοῦ μέγιστος γὰρ καὶ οὗτοι ἐκκρίνουσι τὰ λαλά. Now with regard to Empedocles and Anaxagoras, the accuracy of this statement can be checked by referring to the extant fragments of the philosophers themselves. Anaxagoras, at least, undoubtedly refers to a ‘separating off’ of certain pairs of opposites (the term used in the extant fragments being ἀποκρίνεσθαι), though, as Aristotle himself points out at Ph. 187a

15 It may be noted that in this instance later writers refer to Parmenides’ principles more often as fire and earth than as hot and cold, e.g. Theophr. Phys. Op. fr. 6 (Alex. in Metaph. 984b 3, p. 311, 7 Had., DK A 7), Hip. Ref. i 11.1, DK A 23, Diog. L. ix 21, DK A 1, but contrast τὸ θερμὸν καὶ τὸ ψυχρὸν in Diog. L. ix 22, and at Act. ii 20.8a, DK A 43, what is ‘rarer’ is described as hot, and what is ‘denser’ as cold. Aristotle’s reference to Parmenides’ second principle (Νῆξ) as ‘earth’ has been taken as a typical misinterpretation (McDiarmid, op. cit., 120 f.), but it seems probable enough, to judge from the description of night as ‘dense’ and ‘heavy’ (fr. 8 59), that this principle was associated with earth, if not actually identified with it.

16 Contrast the testimony of Plutarch (de prim. frig. 7 947F, DK 13 B 1) which suggests that Anaximenes reduced differences of temperature to differences of density (and this accords with the frequent mention of ‘the rare’ and ‘the dense’ in other reports on Anaximenes, e.g. Theophr. Phys. Op. fr. 2, Simpl. in Ph. 24.26 ff., DK A 5). Kahn, however, apparently takes the evidence of Plutarch in precisely the contrary sense, as if Anaximenes reduced differences of density to differences of temperature, for he comments (op. cit., 160) ‘Anaximenes seems in fact to have made use of these two (viz., Hot and Cold) in their classic function, as powers of rarefaction and condensation (B 1, A 7-3).’

17 Four times in all (Ph. 187a 20 ff., 209b 14, Cael. 295b 12, Metaph. 1069b 22), excluding the valueless reference in the spurious de Meliso (975b 22). On the passages which refer to unnamed philosophers who postulated an element intermediate between fire and air, or between air and water, see the full discussion in G. S. Kirk, ‘Some problems in Anaximander’, CQ N.S. v (1955) 24 ff.
25 ff., it is not only the opposites, but also what Aristotle calls τὰ ὁμοιομερή that are present in the original mixture and separate off from it. In Empedocles, on the other hand, the principal substances that are described as coming together and separating off are the 'roots' (between which there are, it is true, certain oppositions), rather than such opposites as hot and cold, or wet and dry themselves. It would seem, then, that when Aristotle groups these three philosophers together and suggests that they all 'separate out the opposites that are present in the one', this is only true as a broad generalisation, and indeed there is a further reason not to press the term ἐναντιώτητες too far here, in that throughout this chapter Aristotle is arguing towards the general conclusion that all previous philosophers 'make the opposites in some way the principles of all things' (Ph. 188a 26 ff., cf. also a 19 and b 27 ff.). It is true, of course, that the theory of the separating-out of opposites is particularly associated with Anaximander at Ph. 187a 20 ff., but if this passage is good evidence that he referred to opposed substances of some sort in his cosmological theory, it leaves the problem of the precise nature of those substances still unresolved.

The interpretation that the substances that separate off from the Boundless were the hot and the cold, the dry and the wet themselves rests, rather, on the testimony of Simplicius. First there is a passage which contains not so much a direct report on Anaximander's theory, as a generalising comment on it. We are told that Anaximander ὅν ἀλλοιωμένον τοῦ στοιχείου τὴν γένεων ποιεῖ, ἀλλ' ἀποκρανισμένον τῶν ἐναντίων διὰ τῆς ἀκόλουθου καθίσματος (in Ph. 24.23 ff., DK 12 A 9). The use of the term ἀλλοιωθάνα shows that this comment is made in part, at least, in the light of Aristotle's analysis of different types of change, and once again it would be rash to try to press the term ἐναντία too far. Elsewhere, however, Simplicius specifies the opposites in question as θερμὸν, ψυχρόν, ἐρυθρόν, ὑπέρ καὶ τὰ ἄλλα (in Ph. 150.24 ff., DK ib.). Yet this passage occurs in a commentary on the text of Aristotle which we have already discussed (Ph. 187a 20 ff.), and it is quite clear, as indeed Kahn grants, that Simplicius is, here, 'more concerned to explain Aristotle's text than to describe Anaximander's doctrine in detail'. Kahn goes on to suggest, however (op. cit., 41), that while the second pair of opposites, dry and wet, may or may not have been supplied by Simplicius, the first pair, hot and cold, was certainly given by Theophrastus. He cites two pieces of evidence for this, which we must now consider, namely a text of Aetius and the account of Anaximander's cosmology in the Stromateis, and these do indeed suggest, quite strongly, that the hot and the cold were mentioned in Theophrastus' account of Anaximander. This still leaves the much more difficult question of whether Theophrastus mentioned the hot and the cold in an attempt to elucidate Anaximander's theory, or in a direct report on it, and while it is, no doubt, impossible to achieve certainty on this problem, some light is thrown on it by examining the context of the reference to these opposites in the two texts in question. Thus at ii 11.5 (DK 12 A 17a) Aetius reports: 'Ἀναξιμάνδρος ἐκ θερμὸν καὶ ψυχρὸν μέγα τοὺς πτερίδες παρέσυρε τὴν ἀκρόπολιν'. As the context in Aetius shows, the theory reported here is not a general cosmological doctrine, but one that concerns the substance of the heavenly sphere (ὁ οὐρανὸς means 'heavens' rather than 'universe' here). Now we do not lack other reports on Anaximander's theory of the nature of the heavenly bodies, the stars,

19 See above, n. 3, on fr. 213 ff. The main opposition referred to there is that between fire and water, and this is an obvious fact of experience. As Kahn points out, op. cit., 160, the enmity between these two was proverbial, e.g. Theognis 1245 f., Aesch. Ag. 650 f. and cf. the fight between Hephaestus and the river Xanthus at Ill. xxi 342 ff.

19 This text occurs in a passage in which Simplicius first quotes Anaximander's fragment and then goes on to conjecture the motives which may have led him to postulate τὸ ἀπεργον (δήλων δὲ ὅτι τὴν εἰς ἀλλήλην μεταβαλίν τῶν τετειρών σταχυίων ὤντος θεασάμενος ὅν ἠξίωσεν ἐν τι τούτῳ ὑποκείμενον ποιήσαι, ἀλλ' τὰ ἄλλα παρὰ ταῦτα ὤντος δὲ ὅν ἄλλωσιμένον κτλ.), and while this conjecture is plausible enough (cf. also Arist. Ph. 204b 22 ff.), it is obvious that Anaximander cannot have argued in precisely the way in which Simplicius suggests since this argument depends on the anachronistic doctrine of the four elements.
sun and moon. Hip. Ref. i 6.4–5 (DK A 11), Aet. ii 13.7 (DK A 18), ii 20.1 (DK A 12) and ii 25.1 (DK A 22) all agree in the main features of the theory of the heavenly bodies which they attribute to Anaximander, which is that they are circles of fire which are enclosed in ἀφρό but which have certain openings at which the stars, sun and moon appear. When we have a set of reports that tell us that according to Anaximander the heavens contain a series of circles composed of fire and opaque mist, and when we also know that Aristotle and the doxographers do sometimes reformulate the theories of the Presocratics in terms of the nearer opposites hot and cold (see above), there is clearly a distinct possibility that the report in Aet. ii 11.5 which refers to the heavens themselves as being composed of a mixture of hot and cold is just another such reformulation, especially since Aristotle’s interpretation of Parmenides’ principles fire and earth/night as hot and cold sets such a close precedent.

The second passage in which the hot and the cold are mentioned is in the fullest account of Anaximander’s cosmology which we possess, pseudo-Plutarch Stromateis 2: φησί δὲ τὸ ἐκ τοῦ ἀδίων γόνυμον θερμοῦ τε καὶ ψυχροῦ κατὰ τὴν γένεσιν τοῦτο τοῦ κόσμου ἀποκριθήναι καὶ τῶν ἑκ τούτου φλογὸς σφαίραν περιφέρει τῷ περὶ τὴν γῆν ἀέρι οἷς τὸ δεύτερο φλοίον ἡ στοινος ἀπορραγεῖσθαι καὶ εἰς τινας ἀποκλεισθέντας κύκλους ὑποστθὲν τὸν ἤλιον καὶ τὴν σελήνην καὶ τοὺς ἀστέρας. The comparison between this passage and Simplicius in Ph. 24.23 ff. is instructive. Where Simplicius refers quite generally to a ‘separating off of opposites’, the author of the Stromateis specifies that what separates off from the Boundless (or the Eternal) is, as he puts it, γόνυμον θερμοῦ τε καὶ ψυχροῦ, i.e. that which is ‘capable of generating hot and cold’. But Kirk and Raven21 rightly point out that ‘the nature of the hot (substance) and cold (substance) thus cryptically produced appears from what follows: they are flame and air-mist (the inner part of which is assumed to have condensed into earth)’. And thereafter the account describes how the flame is broken up into circles (surrounded by mist, though this is not stated here) to form the heavenly bodies. Here too, then, there seems to be a distinct possibility that the reference to ‘hot and cold’ is in the nature of a comment on Anaximander’s theory, rather than a verbally accurate report.

To recapitulate the evidence for Anaximander considered so far: on the one hand, Aristotle and Simplicius refer quite generally to a separating off of opposites in Anaximander’s cosmology, and Simplicius certainly reports in one passage that the substances in question are ‘hot, cold, dry, wet and the rest’. Again, the hot and the cold at least are referred to in one text of Aetius (i 11.5) and again in the phrase γόνυμον θερμοῦ τε καὶ ψυχροῦ in the Stromateis. On the other hand, Aristotle and the doxographers do sometimes reformulate Presocratic theories in terms of the opposites (such as hot and cold); against Aet. ii 11.5 it may be argued that elsewhere Anaximander’s theory of the heavenly bodies is much more often described in terms of fire and mist, and the reference to hot and cold in the phrase quoted in the Stromateis seems simply to anticipate the fuller and more precise description that follows: in effect what is produced from the original ‘germ’ is a flame which grows round the mist that encircles the earth. On this evidence, the view adopted by Hölscher, that what was produced from the Boundless was not ‘the hot’ and ‘the cold’ as such, but rather such substances as (e.g.) flame and mist, seems, to my mind, to have the balance of probabilities in its favour; and it seems to derive some support from the fact that in the earliest physical and cosmological theories which have survived in original fragments (those of Xenophanes, Heraclitus, and Parmenides in the Way of Seeming) the primary

20 Diels (Doxographi, 579 note ad loc.) suggested τι as a possible alternative, and this is preferred by Kahn. DK and Kirk and Raven retain τό, however.
21 Hölscher, Kahn and Guthrie understand the term γόνυμον (rightly, I believe) on the analogy of the similar expression γόνυμος καὶ ζήλον καὶ φυτῶν
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which is used of the heat of the sun in Theophrastus de Igne 44.
22 The Presocratic Philosophers, Cambridge, 1957, 133.
23 Hölscher (op. cit., 266) refers tentatively to ‘Feuer und Luft’, but stresses that the question must remain open.
elements of which other things are composed are such substances as water and earth, fire, light and night, and not the hot, the cold, the dry and the wet themselves.

Yet if the likelihood is that Anaximander's cosmological theory was stated in terms of such substances as e.g. fire and mist, there is surely no reason to doubt that an important and original feature of that theory was that the relationship between the substances in question was conceived as some sort of opposition. This seems clear from the one major piece of evidence that we have yet to consider, the fragment itself, where he says διδότα γὰρ αὐτὰ δίκην καὶ τᾶς ἀλλήλους τῆς ἀδύκας κατὰ τὰν τοῦ χρόνον τάξιν. If we assume, as we surely must, that αὐτὰ here refers to cosmological factors of some sort, the sentence tells us nothing about the nature of those factors, but what it does illuminate is Anaximander's conception of the relationships between them, which are described in terms of alternating aggression and reconciliation. As Kirk and Raven, among others, have noted (op. cit., 118) 'the things which commit injustice on each other must be equals, different and correlative', and they continue 'these are most likely to be the opposed substances which make up the differentiated world'. Now the parallels between the known features of Anaximander's cosmology and the system which Parmenides describes in the Way of Seeming are quite striking. Whether or not we hold (with Gigon) that the substances that separate off from the Boundless are Light and Darkness themselves, there seems to be a close similarity between the fire or flame and opaque mist which appear in the reports of Anaximander's theory of the heavenly bodies and in the early stages of the cosmology attributed to him in the Stromateis, and the two principles which Parmenides calls φωστὸς αὐθέρην πῦρ (fr. 8 56) or φῶς (fr. 9) or φωτικός (fr. 12) and νῦν ἄδικης οὐ διάνοις (fr. 8 59, fr. 9 3). And it has often been noticed that the obscure system of interwoven rings, composed of fire or light and darkness, which Aetius (ii 7,1, DK 28 A 37) attributes to Parmenides and which is referred to briefly in fr. 12, bears a marked resemblance to Anaximander's system of the heavenly bodies, with its series of rings compounded of fire and mist. But it is the account of the relationships between major cosmological factors which especially concerns us here. Where we infer from Anaximander's fragment that certain cosmological factors (whether two or more than two in number) are opposed to one another, but of equal status, Parmenides' description of his principles in the Way of Seeming is explicit: they are opposites (τὰντα δ' ἐκπεργαντο δέμας fr. 8 55, τάντα v. 59) and 'both of them are equal' (ἰσον ἁμβοτέρων fr. 9 4).

There is, of course, no need to suppose that Anaximander is the only, or even the most important, influence which can be detected in the Pythagorean χωρογία given by Aristotle at Metaph. 986a 22 ff., though we cannot be certain whether this represents a pre- or a post-Parmenidean theory. It may be added that the question of whether, or in what sense, the cosmology of the Way of Seeming represents Parmenides' own beliefs is irrelevant to the present issue in that whatever view we adopt on that problem, the cosmology in question may exhibit resemblances to Anaximander's system.

There is a full discussion of Parmenides' highly obscure theory in Heath, Aristarchus of Samos, London, 1913, 66 ff., who notes that it 'seems to be directly adapted from Anaximander's theory of hoops or wheels'. Yet Parmenides' theory was clearly an adaptation, rather than simply a copy, of Anaximander's, for while each of Anaximander's wheels consisted of fire enclosed in mist, Parmenides apparently distinguished between three different kinds of rings, one sort made of 'the rare' (light) alone, a second made of 'the dense' (darkness) alone, and a third composed of both elements combined.
claim that it is the best cosmology of its kind (which is implied by the remark ὲσ ὃ μὴ ποτέ τις σε βροτῶν γνώμη παρελθοντάς fr. 8 61) suggests that whatever influences he has undergone, or whatever the sources from which he has drawn, he has modified and developed his predecessors’ theories in the Way of Seeming, not simply copied them, and indeed there appear to be certain specific points of difference between the theories reported from the Way of Seeming and those ascribed to Anaximander in particular (see above n. 28). But if we ask what form the opposition between cosmological factors took in Anaximander, the Way of Seeming suggests one possible answer. The things whose acts of aggression are referred to in Anaximander’s fragment were certainly ἐναρτίον in the original sense of hostile to one another. But if the things in question were not the hot and the cold, the dry and the wet themselves, it seems probable enough that they were conceived as possessing certain specific opposite characteristics, just as in Parmenides’ Way of Seeming Night is described as dense and heavy and Fire is, no doubt, conceived both as rare and light (though the text of fr. 8 57 is corrupt). In conclusion, we may agree with Hölsher and others that the reports in Aristotle and Simplicius that refer to a separating off of opposites have to some degree reformulated Anaximander’s ideas, and yet the actual extent of that reformulation may not have been very great. Whatever the substances were that separate off from the Boundless in the early stages of cosmological development, the notion of some opposition between them certainly seems to have been part of Anaximander’s theory.\footnote{A similar doctrine of the interaction between opposed substances of various sorts can be traced not only in the fragment and in the theory of the formation of the world reported by pseudo-Plutarch where (hot) flame and (cold) mist separate off from the Boundless, but also in several of the biological and geophysical theories attributed to Anaximander. Thus according to Hip. Ref. i 6, 6 (DK A ii) he held that living creatures arose from the wet acted upon by the sun (cf. Aet. v 19.4, DK A 90), and according to the theory described in Arist. Mete. 353b 6 ff. and attributed to Anaximander by Alexander and Aetius (DK A 27) he may have represented the sea as what is left of the original moisture in the region round the earth, after this had been dried by the sun.} If the evidence suggests that these substances were, e.g., fire and mist rather than the hot and the cold themselves, it nevertheless seems possible, and indeed quite probable, that Anaximander himself distinctly associated hot and cold and other pairs of opposites with the substances in question, just as Parmenides associated certain pairs of opposites (rare and dense, light and heavy) with Fire and Night in the Way of Seeming. A note should be added on the originality and significance of Anaximander’s theory of the interactions between opposed cosmological factors. It is well known that certain Presocratic cosmological doctrines are foreshadowed, though often only quite dimly foreshadowed, in Homer and Hesiod. The notion of ‘elements’, i.e. primary component parts, is, perhaps, implicit in the myth of the making of Pandora out of earth and water,\footnote{Hes. Op. 60 ff. II. vii 99 is also usually mentioned in this context, though its interpretation is, to my mind, quite doubtful.} and Kahn\footnote{Op. cit., 134 ff. cf. also M. C. Stokes, ‘Hesiodic and Milesian Cosmogenes’, pt. 1, Phronesis vii (1962) 1 ff.} has recently attempted to trace how Empedocles’ doctrine of the four elements may have developed out of references to the four world-areas, sky, sea, earth and underworld (or night) in such passages as ll. xv 189 ff., Hes. Th. 106 f., 736 f., although this seems at best a remote possibility. It has been noted, too, that opposed factors of various sorts appear quite prominently in the Theogony. Emphasis is sometimes laid on the reference to Night (and Erebus) and Day (and Aither) at 123 ff. (a passage which implies the priority of Night); even more important, no doubt, is the role ascribed to Earth and Heaven (132 ff., 147 ff.) as the parents of various creatures, and the use of sexual imagery, the pairing of male and female personifications, recurs, of course, throughout the Theogony. Yet what the Theogony contains is merely an account of the origins of the various entities mentioned: it is not concerned with the present interactions, changes or relationships between them. Anaximander, on the other hand, attempted, it seems, not only to describe the formation
of the universe from the Boundless, but also to give an account of the continuing interactions between things in the world as we know it. Unlike Earth and Heaven in the Theogony, which unite simply to produce a series of offspring, but whose subsequent relationship is left undefined, the unnamed subjects of Anaximander's fragment are involved in a continuous self-regulating interaction, an alternating cycle of δικη and ἀδικία. This is a new and undoubtedly most important conception, the introduction of which may be said to mark the beginning of cosmology as such, as opposed to cosmogony or theogony. Moreover, we should note that it is in connection with theories of a type broadly similar to Anaximander's that the hot and the cold, the dry and the wet are most often used in later philosophical and medical writers. The first philosophical text in which these four opposites appear (Heraclitus fr. 126) describes their interactions: τά ψυχρά θέρετα, θερμῶν ψύχεται, ὑγρῶν αναίνεται, καρπάλων νοτίζεται. Anaxagoras, too, stresses the interdependence of hot and cold in fr. 8, and there are, of course, many medical theorists who connected health, or the natural state of the body, with the balanced interaction of these and other opposites, and disease with their temporary imbalance. We find, then, that while the first extant physical theory based on the hot and the cold, the dry and the wet as the four primary elements of other things appears in On the Nature of Man, the doctrine of the balanced interaction of opposed substances occurs in various forms in earlier theorists and goes right back to Anaximander himself. Furthermore, while we cannot be certain of the exact terms in which his cosmological theory was expressed, it is likely enough that he referred to some interaction between hot and cold substances, at least, if not between the hot and the cold as independent entities.

II

The hot and the cold, the dry and the wet appear particularly often, though not exclusively, in theories of a general type which can be traced back as far as Anaximander, in which the relationship between opposed factors is conceived as one of a continuous, balanced interaction. Unlike right and left, male and female, or light and darkness, these four opposites do not figure in the Pythagorean συνταξια, and it may seem unlikely that any of the theories in which they appear should be affected by preconceived notions of value such as influence many of the doctrines which were based on right and left. Yet these two pairs are undoubtedly correlated often enough with other opposites, and moreover some of the correlations which were proposed quite clearly correspond not to any empirically verifiable data, so much as to assumptions concerning the values of these opposite terms. Several of the theories which were based on these opposites can, in fact, only be explained on the hypothesis that they too sometimes possessed certain positive and negative values, though these may be neither so pronounced, nor so stable, as those of the pair right and left. First, however, we should consider to what extent these opposites, hot, cold, dry and wet, had any marked symbolic associations for the ancient Greeks as a whole, in so far as we can judge from the pre-philosophical texts.

Like ourselves, the ancient Greeks connected warmth not only with life itself, but also

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32 See G. Vlastos' article, 'Isonomia', AJP lxxiv (1953) 337 ff.
33 Hot and cold, dry and wet are, however, described as being ἐσκόμια in the world in the cosmology reported to have been found by Alexander Polyhistor in certain 'Pythagorean notebooks' (Diog. L. viii 24 ff., DK 58 B 1a) though the value of this evidence is disputed (see most recently Guthrie, HGP i 201 n. 3). As Kahn notes (op. cit., 190) the doctrine of the equality of hot, cold, dry and wet resembles that which is found in, for example, On the Nature of Man ch. 7 (L vi 48 20 ff.). The conception of the special importance of the hot, and its connection with life, in chs. 27 ff., may also be paralleled elsewhere, e.g. in the theory attributed to Philolaus in Anon. Lond. xviii 8 ff., and On Fleshes ch. 2, L viii 584 9 ff.
with such emotions as joy and relief. Conversely, cold is associated, naturally enough, with death, and then also with such emotions as fear. As regards the pair dry and wet, several usages suggest that the Greeks conceived the living as 'wet' and the dead as 'dry'. In the Odyssey (vi 201) we find the expression διερός βροτός used apparently as the equivalent of ζωός βροτός (e.g. xxiii 187) to mean 'living mortal' (cf. ix 43, where the same adjective evidently means 'active'), and there seems no good reason to suppose that διερός here is anything other than the common Greek word which literally means 'wet' (e.g. Hes. Op. 460). Conversely, dead or dying things are 'dry'. This is obviously true of dead wood (e.g. ἀξίοκεφον used of a poplar drying after it has been felled, Il. iv 437), but 'dead' parts of the body, such as the nails, are also described as 'dry' (e.g. Hes. Op. 743). The dead themselves were called διάβατος (e.g. Plato Rep. 387c) which was taken to mean 'without moisture', and the old, too, were apparently thought of as 'dry' for when Athena is about to transform Odysseus into an old man at Od. xiii 392 ff. she says she will 'dry up' his fine skin (κάρπῳ μὲν χρόα καλὸν 398, cf. 430). These usages are, for the most part, clearly derived from such obvious facts of experience as the dryness of dead wood, the warmth of living animals and the cold of the dead. From this largely biological point of view, hot and wet are together opposed to cold and dry. Yet from another point of view a different correlation naturally suggests itself. The Greek summer is unmistakably hot and dry, the Greek winter unmistakably cold and wet. The contrast between the season of cold and rains and the season when 'Sirius dries the head and the knees and the skin is parched by the burning heat' is vividly described by Hesiod (Op. 594 ff., 582 ff.). If the antithesis between Heaven and Earth is particularly important in early Greek religious beliefs, the hot and the dry are naturally associated with the sun and so with Heaven. Unlike right and left, neither hot and cold, nor dry and wet, it seems, possessed any strong positive or negative values in themselves, though they acquire such values by association in different contexts. But while hot and cold have uniformly positive, and negative associations respectively, the dry and the wet, on the other hand, appear to have ambivalent associations. On the one hand, hot and wet are both connected with what is alive, and cold and dry with the dead, and here wet acquires certain positive, dry certain negative, overtones; but on the other hand, observation of the seasons naturally suggested a different correlation, in which the dry and the hot are the positive terms set over against the negative cold and wet.

In the philosophers, these opposites are correlated sometimes from a 'cosmological', sometimes from a 'biological', point of view. Anaxagoras, for instance (fr. 15), separates on the one side cold and wet and dense and dark (associating these, in all probability, with earth) and on the other side hot and dry and rare (connected with aither), and Parmenides,

34 Thus the basic meaning of λάειν seems to be to warm (e.g. Od. x 359, cf. melt, Od. xii 175), but when applied to the θυμός, for example, it comes to mean 'comfort', e.g. Od. xvi 379, Il. xiv 119, cf. ιησομα, Od. xiv 537.
35 Among the objects to which the epithets κρύος and κρύος are applied in Homer or Hesiod are Hades (Op. 153), fear (Il. ix 2), war (Th. 936) and γόης (Il. xxiv 524).
36 Cf. Onians, Origins of European Thought, Cambridge, 1951, 254 ff. Onians (ch. 6, 200 ff.) also discusses the expression κατεύθετο δὲ γυναικῶν αἴων (e.g., Od. v 152) and collects certain evidence which suggests that sexual love and desire may have been associated with moisture by the ancient Greeks (e.g., Hes. Th. 910, Alcman 59 Page, Anacreon 114 Page, h. Hom. xix 33 f.).
37 Cf., Aesch. fr. 229 which speaks of the dead in whom there is no moisture (ιλακας). In the Orphic fragment 32 (a) and (b) (in DK as 1 B 17 and 17b) the dead man who speaks describes himself as αίων, and the belief that the dead are thirsty evidently underlies the widespread Greek practice of offering them libations.
38 Cf. e.g. Soph. El. 819 where Electra, foreseeing her old age, says αδελφόν βιον.
39 It is to be noted, however, that there is no sign in Hesiod, or anywhere else in pre-philosophical literature, of any schematic correlation between the four seasons and the four opposites, such as we later find in, for example, On the Nature of Man, ch. 7 (L. vi 46 9 ff.) where winter is wet and cold, spring wet and hot, summer dry and hot and autumn dry and cold.
too, very probably associated hot, as well as rare, with fire and light, and cold, as well as dense and heavy, with night or darkness, as the scholion reported by Simplicius (in Ph. 31.3 ff.) explicitly suggests. On the other hand, there is plenty of evidence that the belief in the connection between life and the hot and the wet continued in Greek philosophy and medicine.40

Each of the two types of correlations which we have noted so far draws on a different set of observations, on the one hand such meteorological phenomena as the alternation of summer and winter and day and night, and on the other such biological phenomena as the warmth of living creatures and the coldness, and in some cases the dryness, of what is dead. But when we turn to some of the more detailed doctrines which were based on these opposites, we find theories which apparently have no basis in empirical evidence at all. Thus it was widely assumed that the difference between the two sexes was in some way to be connected with a difference between hot and cold (and sometimes, too, between dry and wet), though there was no general agreement as to which opposites corresponded with which sex. The view that women are hotter than men is mentioned by Aristotle at PA 648a 28 ff., where he ascribes it to Parmenides among others. Furthermore the grounds on which this theory was adopted are clear from Aristotle’s text and from the Hippocratic treatise On the Diseases of Women (Book i, ch. i, L viii 12 17 ff.), namely that menstruation is due to an abundance of (hot) blood and is, therefore, a sign of the greater heat of women. But if this was one opinion that was put forward, the contrary view, that men are hotter than women, was maintained, for example, by Empedocles (Arist. ib., cf. frr. 65 and 67). If we ask why this position was adopted, the answer lies not so much in any empirical data that could be adduced, as in the belief that male and hot are inherently superior to their respective opposites female and cold. True, both the authors of On Regimen and Aristotle feel it necessary to defend this view with arguments, though on examination their arguments can be seen to consist largely of special pleading. (1) At On Regimen i ch. 34 (L vi 512 13 ff.) the writer suggests that males are hotter and drier (a) because of their regimen, and (b) because females purge the hot from their bodies every month. But (a) depends on the writer’s schematic, a priori analysis of the effects of food and exercise, and (b) is clearly a case of special pleading, since if the effect of menstruation is taken to be that females become colder, then by the same reasoning they should also become drier on the loss of blood. Yet in the writer’s view males are hotter and drier (conforming to the element Fire) and females are colder and wetter (conforming to the element Water). These opposites are, in fact, arranged according to the writer’s notions of fitness (hot, dry, male and fire each being the positive terms), rather than according to his observation of the differences between the two sexes. (2) Aristotle shares a similar doctrine, but the arguments he uses to defend it are no more convincing. At GA 765b 8 ff. he distinguishes males and females by their ability, or inability, to concoct the blood, assuming that that which becomes the menses in females, becomes semen in males. He notes once again (as at PA 648a 28 ff.) that other theorists took menstruation as a sign of the greater heat of the female sex, but he suggests that this view does not take into account the possibility that blood may be more or less pure, more or less concocted, and he argues, or rather asserts, that semen, though smaller in quantity, is purer and more concocted than the menses.41 While it is interesting that

40 The idea that living creatures originated in the wet when acted upon by the sun is attributed to Anaximander at Hip. Ref. i 6.6 (DK 12 A 11) and a similar theory occurs in the cosmology reported in Diodorus (i 7.3 ff., DK 68 B 5.1) (cf. also the view mentioned by Aristotle when he discusses the possible reasons which may have led Thales to make water the principle, namely that ‘the hot itself comes to be from this and lives by this’, Metaph. 985b 25 f.).

41 There is an obscure comparison with the production of fruit at GA 765b 28 ff. (‘the nutriment in its first stage is abundant, but the useful product
Aristotle refers here to qualitative, rather than to purely quantitative, differences, in order to determine what is ‘hot’, his view that males are hotter than females depends first on the notion that semen and menses are the end-products of strictly comparable processes, and second, and more important, on the quite arbitrary assumption that semen is the natural product of the process of conception, and the menses are an impure residue. Aristotle believes that the female is, as it were, a deformed male, and it is this conviction, rather than any empirical considerations, that underlies his doctrine that males are hotter than females.42

One clear instance where certain theorists correlated hot and cold, dry and wet and other opposites according to preconceived ideas of fitness or value has now been considered, and other less interesting examples could be given from fifth- and fourth-century writers.43 But the most striking evidence of the tendency to treat these pairs as consisting of a positive and a negative pole comes from Aristotle, whose theories should now be considered in more detail. First we should note how he defines these four terms at GC 329b 26 ff. ‘Hot’ is ‘that which combines things of the same kind’ (τὸ συγκριῶν τὰ ὁμογενή); ‘cold’ ‘that which brings together and combines homogeneous and heterogeneous things alike’ (τὸ συνών καὶ συγκριῶν ὁμοίως τὰ τε συγγενῆ καὶ τὰ μὴ ὁμόφυλα); ‘wet’ is ‘that which, being readily delimited (i.e. by something else), is not determined by its own boundary’ (τὸ ἄδραστον οἰκεῖο ὑγρό, εὐόρθιστον ὄν) and ‘dry’ ‘that which, not being readily delimited (i.e. by something else), is determined by its own boundary’ (τὸ εὐόρθιστον μὲν οἰκεῖο ὑγρό, δυσόρθιστον δὲ). These definitions are highly abstract and convey no hint that these opposites had any positive or negative associations for Aristotle. It is all the more surprising, then, that in other contexts these pairs are clearly conceived as divided into a positive, and a negative, pole. This is particularly obvious in the case of hot and cold. As we have just seen, Aristotle’s belief that the male sex is hotter than the female has no empirical basis, but merely reflects his preconceived notion of the superiority of male and hot. The doctrine that the right hand side of the body is hotter than the left is another theory that derives from assumptions concerning the values of these terms and has no basis in fact (see JHS lxxxii (1962) 62 ff.). Then again in PA B 2 647b 29 ff. he suggests certain correlations between the temperature and the purity of the blood, and the strength and intelligence of different species of animals or even of different parts of the same animal. Thus thick and hot blood is conducive to strength, and thinner and colder blood to sensation and intelligence,44 but ‘best of all are those animals whose blood is hot and thin and clear: for such are favourably constituted both for courage and for intelligence’.45 Elsewhere the association between heat and perfection is again apparent when he suggests at PA 653a 27 ff. that the region round the heart and the lung is hotter and richer in blood in man than in any element Fire, and old age with the inferior element Water (cold and wet): his view that the old are cold and wet in particular runs counter to the generally accepted Greek notions (see above, p. 101) though it also appears in On Regimen in Health ch. 2 (L vi 74 19 ff.).

43 Thus there is an elaborate schema in which the four ages of man are correlated with pairs of opposites in On Regimen i ch. 33 (L vi 510 24 ff.), the first age being hot and wet, the second hot and dry, the third cold and dry, and old men cold and wet. But this schema seems dictated in part, at least, by the author’s desire to associate the second age, that of the young man, with the male sex and the superior

44 One of the facts which he has in mind when making this latter suggestion is that some bloodless animals (e.g. bees) are more intelligent than many sanguineous animals which are hotter than them (PA 648a 5 ff.).
45 He goes on to suggest that the upper parts of the body are distinguished in this respect (i.e. in the heat and purity of their blood) from the lower, as also are males from females, and the right side of the body from the left (PA 648a 11 ff.).
other animal (and in males more so than in females), for this too is a doctrine for which he can have had no direct evidence.

In Aristotle's theory hot is clearly the positive term, cold the privation: indeed this is explicitly stated on several occasions (e.g. *Cael.* 286a 25 f., *GC* 318b 16 f.). His attitude towards the pair dry and wet is, however, less clear. At *PA* 670b 18 ff. where he correlates hot and right on the one hand, and cold and left on the other, the inferior left side of the body is said to be both cold and wet. Again at *GA* 766b 31 ff. he says that parents who have a ‘wetter or more feminine’ constitution tend to produce female children, and this suggests that wet is also associated with the inferior female sex.\(^46\) On the other hand at *GA B* 1 (732b 15 ff.) he defines the main genera of animals according to their methods of reproduction (which correspond to differences in their constitutions) and here the most perfect animals, the Vivipara, are said to be ‘hotter and wetter and less earthy by nature’ (732b 31 f.). The second group, the ‘ovoviviparous’ animals (e.g. cartilaginous fishes) are cold and wet, the third and the fourth (the Ovivipara which lay perfect, and those which lay imperfect, eggs) are hot and dry, and cold and dry respectively, and the fifth and final group (insects) are ‘coldest of all’ (733b 10 ff.). Greater perfection clearly corresponds, in this schema, to a combination of greater heat and greater ‘humidity’. Again at *Long.* 466a 18 ff. he says that ‘we must assume that the living animal is by nature wet and hot, and life too is such, while old age is cold and dry, as also is that which is dead: for this is plain to observation’. Yet he goes on to note that both the quantity and the quality of the humidity of animals affect their length of life: ‘for not only must there be a lot of the wet, but it must also be hot’ (a 29 ff.). The pair dry and wet occupies, then, a somewhat ambivalent position in Aristotle’s philosophy, as indeed it had also done, to some extent, in earlier Greek speculative thought. On the one hand he notices a connexion between humidity and life, and between dryness and death (and here he develops a notion which can be traced back to Homer and Hesiod). Yet this does not prevent him from suggesting, in other contexts, that the wet is the inferior, privative term, when he correlates it with female, left and cold.

The evidence we have considered shows that the tendency to divide opposites into a positive and a negative pole is by no means confined, in early Greek speculative thought, to the use of such pairs as right and left, or light and darkness, but is found also in some of the theories based on the much more common pairs hot and cold, dry and wet. This is all the more striking since the symbolic associations which these two pairs possessed for the ancient Greeks were not particularly marked (compared with those of right and left and certain other opposites) and indeed in the case of dry and wet these associations were rather ambivalent. Hot and cold, dry and wet do not figure in the Pythagorean ἀντικείμενα as reported by Aristotle: moreover when they are introduced into cosmology and medicine, they are used most often in theories in which their balance and continuous interaction are stressed, which certainly do not imply, though they do not exclude, the idea that these pairs are each divided into a positive and a negative pole. Yet on several occasions we find theorists proposing correlations between these and other opposites which have no basis in empirical evidence at all, but merely reflect certain preconceived notions of the values of the contrasted terms. Several theorists maintained, for example, that the male sex is hotter than the female, and Empedocles, for one, took the difference in temperature to be the origin or source of the difference between the two sexes (*frs.* 65, 67). If this may have been the traditional view, or rather the view that accords with the generally accepted notions concerning the values of these opposites, other theorists rejected it and reversed the correlation. In discussing the use of right and left, I noted that some of the *a priori* Aristotelian Problematas (e.g. *879a* 33 ff.).

\(^46\) The view that men are dry and hot, and women wet and cold is also often expressed in the pseudo-
theories which were proposed, were rejected by later writers on empirical grounds: thus Aristotle refuted the belief that males are formed on the right side of the womb by referring to the evidence of anatomical dissections (GA 765a 16). So too it would seem that some theorists (of whom Parmenides may well have been the first) argued from the fact of menstruation that females, and not males, are the hotter sex. Among later philosophers, the tendency to divide hot and cold, and dry and wet, each into a positive and a negative pole is particularly marked in Aristotle, even though the abstract definitions which he gives of these four terms at GC 326b 26 ff. give no hint of this. Although he never presents his theory in the form of a complete avarostxia, his Table of Opposites, if we reconstructed it from remarks scattered through the physical works, would be almost as extensive as that which he attributed to the Pythagoreans. Right and male and up and front and hot and dry would certainly appear on one side, set over against left, female, down, back, cold and wet on the other, and such other pairs as light and heavy, rare and dense, might also be included in the list, correlated with hot and cold or dry and wet—the Table as a whole reflecting both empirical and a priori considerations. Even where the facts appeared to conflict with his correlations, he did not abandon his belief that these pairs of opposites may be arranged in a single systematic schema. This is apparent from his firm correlation of wet with left, female and cold, even though he recognised the connection between humidity and warmth and life, and between dryness and cold and death. Indeed, on the subject of hot and cold, he explicitly remarked on the extent of the disagreement that existed between different theorists as to which things are hot and which cold. As he puts it at PA 648a 33 ff. (cf. also a 24 f.) 'if there is so much dispute about the hot and the cold, what are we to think about the rest? For the hot and the cold are the most distinct of the things which affect our senses'. One instance of such a dispute, concerning the temperature of the two sexes, has already been discussed, and Aristotle mentions several others. Yet we saw that while there was no agreement as to which sex was hot, which cold, both parties in this dispute seem to have assumed that some correlation may be established between male and female on the one hand, and hot and cold on the other. Some at least of those who rejected the view that males are hotter than females, were not content simply to reject that view, but proposed the contrary, but equally erroneous, theory that the female sex is uniformly and essentially hotter. It is, then, one of the notable features of the use of hot and cold and dry and wet in early Greek speculative thought, that while there was little agreement about their particular application to different problems, the assumption that some correlation

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47 At Cael. 286a 26 ff. 'heavy' is said to be the privation of 'light', and at Ph. 217b 17 ff. and Cael. 288b 7 ff. 'heavy' and 'dense' are associated together, and so too 'light' and 'rare' (for this reason, perhaps, as well as because of the association between 'thin' and 'clean', the blood of males is said to be 'thinner' than that of females, PA 648a 11 ff.). Yet with such a pair as 'soft' and 'hard' we find different types of correlation proposed in different contexts: on the one hand 'hard' is associated with 'dense' (e.g. Ph. 217b 17), but on the other it is assimilated to 'dry' at GC B 2 330a 8 ff. in a chapter in which he reduces various types of opposites to 'hot' and 'cold' or 'dry' and 'wet' while at the same time pointing out some of the ambiguities of these terms. From other passages (e.g. Ph. 259a 6 ff.) it would appear that like the Pythagoreans, Aristotle thought 'one' superior to 'many' and 'limited' to 'unlimited'.

48 E.g. the dispute as to whether land-animals are hotter or colder than water-animals, and the disagreement about which 'humours' are hot and which cold. Cf. also the opposite views about the nature of the ψεύδη mentioned at de An. 405b 24 ff. We know little about the arguments used on each side in these controversies, but it seems unlikely that a priori considerations affected the discussion to any great extent. Thus Philolaus seems to have held that the phlegm is hot (as opposed to the generally accepted view that it is cold) simply on the grounds of a suggested etymology of the word φλέγμα from φλέγω (see Anon. Lond. xviii 41 ff.).

49 It is strange that the fact of menstruation was taken by both sides in this dispute to be significant of a difference in the temperature of the two sexes (though they disagreed about how this evidence was to be interpreted). Yet while the temperature of the female certainly rises and falls according to the menstrual cycle, the fact of menstruation provides no evidence concerning the relative temperature of males and females at all.
was to be set up between these and other pairs of opposites was sometimes shared both by those who put forward views based on *a priori* considerations, and by those who rejected those views on empirical grounds.\(^\text{50}\) And it is, perhaps, particularly surprising that the tendency to incorporate these opposites into a single systematic, but often quite arbitrary, schema should survive in Aristotle, even though he was fully aware of the lack of agreement among his predecessors on the subject of the hot and the cold and the dry and the wet, and drew attention, on several occasions, to some of the ambiguities which the use of these terms involved.\(^\text{51}\)

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\(^{50}\) The writer of *On Ancient Medicine* provides something of an exception to this general rule, for he notices the difficulties which arose concerning the application of the doctrine of the hot and the cold, the dry and the wet to problems of diagnosis and cure (ch. 13, *CMG* i. 1 44.8 ff.) and then suggests that hot and cold are the least important of the \(\delta\nu\rho\alpha\mu\varepsilon\varsigma\) in the body (ch. 16, 47.12 ff.).

\(^{51}\) I must express my thanks to Mr G. S. Kirk for reading and criticising an earlier draft of this paper. The faults that remain are, of course, entirely my own responsibility.
Carian Armourers—The Growth of a Tradition*

One of the more persistent and widespread minor traditions in ancient literature represents the Carians as the great military innovators and practitioners of early times. It is one of several ‘Carian’ traditions, in which this people is given a greater importance than it seems historically possible to allow,¹ and which at one time led certain scholars to believe that the Aegean Bronze Age civilisation as a whole was Carian in origin.² This particular example can be checked, up to a point, from the evidence of archaeological discoveries; and the experiment may prove worth making, both as a supplement to the archaeological record, and as a test case for the value and quality of such traditions. In its more extreme version, the Carians are credited with the actual invention of various military devices: this, as I hope to show, is unlikely to be true. But there is a milder form of the tradition, which states that the Carians habitually used these devices. This version may in part arise from the vaguer wording of certain ancient authorities, but as it stands it is quite acceptable.

A. Literary Evidence

The tradition appears in a range of literature covering nearly two thousand years, from Alkaios down to Eustathius. Much of this literary evidence is already well known and has been discussed by others but, largely because the archaeological record has only recently become anything like adequate to serve as a check, I do not think that the question has been systematically reviewed before.³

We may begin with two quotations from poets. Only here do we have writers speaking at first hand of the devices in question: but their evidence is far from explicit.

Alkaios, fr. 22 Bergk (58 Diehl, Z 65 Lobel & Page):

'λόφον τε σείων Καρικόν'

Anakreon, fr. 91 Bergk (81 Diehl, 56 Page):

'dia &eacute;ute Karikophrē&omicron; | &omicron;kh&omicron;nou &chi;&epsilon;&omicron;ra ti&beta;̄emenoν'

These fragments are known only from a quotation of Strabo⁴ and from later commentators.⁵ There are ambiguities in each fragment, and there are differences between the two. Alkaios tells us that someone—probably himself, possibly a companion or even an enemy—wears a helmet-crest that is called ‘Carian’. It is perhaps more likely that one specific type of crest is so called, than that crests habitually bear that epithet. If one particular type is meant (and if Alkaios or one of his comrades-in-arms is wearing it), then we may compare another of his poems in which armour is described, in this case definitely belonging to

* I am most grateful to my colleagues M. C. Stokes, Dr E. K. Borthwick and D. B. Robinson for their advice and help, and also to Professor A. Andrewes, John Boardman and Dr G. E. Rickman for several valuable suggestions. ¹ Compare the alleged early Carian settlements on the Greek mainland (Pausanias i 40.6: Strabo vii 321–2; viii 374), and the Carian Thalassium, also improbably early on one account (Myres, JHS xxvi (1906) 107–9). More plausible is the tradition of bachelor Ionian settlers marrying Carian women when founding Miletus (Hdt. i 146.2).
² E.g. U. Köhler in Ath. Mitt. iii (1878) 8 ff.; F. Dümmler and F. Studniczka, ibid. xii (1887) 1–17. ³ The nearest approach is Miss Lorimer’s discussion, BSA xlii (1947) 128–32.
himself or his close comrades. He relates how the roof and walls of a building are decorated

'λάμπρωσις κυνίασι, κατ τῶν λείκο κατέσπερθεν ἦπιοι λόφοι | νεύοιος'.

These lines may be in part inspired by the repeated passage in the Homeric poems, in which the crest of various heroes 'δείνων ... καθόπερθεν ἔφευ', but it is overwhelmingly likely, in a poem of this kind, that they also correspond with contemporary life. An equation between this description and the 'Carian crest' of the other poem must remain tentative, but it is not at all unlikely: the notion of 'shaking' in the one fragment goes well with the 'nooding' of the other.

The Anakreon fragment, besides being perhaps nearly a century later, refers to a different piece of equipment: the ὀξανων, or arm-band of the Greek hoplite shield—a fairly certain identification in this case, though not always. There is a further slight difference between the two, in that the article is said to be of actual Carian workmanship: this could mean more than Alkaios' phrase, but once again there is the uncertainty as to whether this particular specimen is made by a Carian, or whether the epithet is habitually used of ὀξανα.

But there is no doubt as to how the ancients understood the two references. Strabo is the first to cite them, in his description of Caria: and he does so to illustrate his statement 'τοὺς δὲ περὶ τὰ στρατιωτικὰ ξίφοι τὰ τε ὀξανα ποιοῦνται τεκμηρία καὶ τὰ ἐπίσημα καὶ τοὺς λόφους ἀπαντὰ γὰρ λέγεται Καρικά'. For his second item, the ἐπίσημα or shield-devices, he offers no evidence: possibly he knew that they were the inevitable concomitant of the ὀξανα, both being distinguishing marks of the hoplite shield. More probably, however, the statement is simply a reflection of the more famous passage in Herodotus, i 171:

'καὶ ὁφι τρεξά ἐξευρήματα ἔγνετο, τοῦτοί οἱ Ἑλληνες ἐχερήσαντο· καὶ γὰρ ἐπὶ τὰ κράνα ῥόφους ἔποδέσθαι Κάρες ἔλις οἱ καταδεξαντες καὶ ἐπὶ τὰς ἀσπίδας τὰς σημεία ποιέονται, καὶ ὀξανα ἀσπίδας ὄστρακον εἰσίν τοὺς ποιημάτων πρῶτον· τέως δὲ ἀνεί όγγων ἐφόρον τὰς ἀσπίδας πάντες οἱ περ ἐκόθεσαν ἀσπίδας χρώσαν, τελαιώσαν αὐτοκόμον ὀμήκιζοντες, περὶ τοὺς αὐξέως καὶ τοὺς ἀριστερῶς ὀμόμισι περικείμην'.

Exactly the same three items are listed here, though Strabo has reversed the order. But whereas Strabo's statement is a guarded one, and a fair inference from the evidence he quotes, Herodotus goes further. The Carians are said to have originated the use of these objects in war, and to have passed them on to the Greeks: nor is any authority given for the statement. It would seem extremely likely that there is a connection between Herodotus' source and that of Strabo: but there is a difficulty here. For Herodotus' account occurs in the middle of a passage which purports to be a Cretan version of early Carian history: the passage ends with the words 'κατὰ μὲν δὴ Κάρας οὖν Κρήτης λέγοντος γενέσθαι'. Nevertheless, these words are most naturally taken to refer only to the main theory just propounded: that the Carians had come to their homeland after being expelled from the Cyclades by Dorians and Ionians. Herodotus goes on 'οὗ μέντοι ... ὀμολογεῦοι τούτοις οἱ Κάρες, ἀλλὰ νομίζουσιν αὐτοὶ ἐνοτοὺς εἶναι αὐτόχθους ἀπειρώτας'—they claimed to be autochthonous but, not unnaturally, they did not disown the inventions. There is no reason why the list of specific Carian ἐφήματα should not have been inserted by Herodotus, however anachronistically, to enlarge on the previous sentence in the 'Cretan version', where it was said that the Carians, through manning the warships of Minos, had become the most famous people in the whole world. It is inherently improbable that the Cretans would credit the Carians (probably gratuitously, as we shall see) with such specific inventions. Perhaps it could still

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6 Fr. 15 Bergk (54 Diehl, Z 34 Lobel and Page).
7 Iliad iii 337 (Paris), xv 481 (Teukros), xvi 138 (Patroklos): Odyssey xxii 124 (Odyssseus).
8 The equation between ὀξανων and the stricter term πάμαξ is expressly made by the Scholiast on Iliad viii 193: Miss Lorimer (BSA xli (1947) 128–30) has shown that it is valid for the earlier writers and that only later, perhaps through confusion, are the two differentiated.
be maintained that Herodotus’ statement does derive from his Cretan informants. But it is worth noting that this, if true, would greatly reduce its authority; for the ‘Cretan version’ of i 171 contains several quite unacceptable assertions, and is apparently related to the period of ‘Minos’, which surely precludes it from giving a serious account of the invention of the Greek hoplite shield.

Let us then assume that there is a connexion between the sources of Strabo, who was clearly indebted to no ‘Cretan version’, and of Herodotus. This connexion could come about in one of two ways. First, Herodotus could have based his account directly on the two quotations from the lyric poets, with the possible addition of a third authority for the invention of the σημεία. That Herodotus was familiar with the works of Alkaios at least, we may infer from v. 95, where he gives both the content and the name of the addressee of a lost poem. (Anakreon is only once casually mentioned, at iii 121.) The main arguments for this view are the precise identity of the three items listed by Herodotus and by Strabo, and the fact that no ancient writer names any source other than the two lyric poets for these and similar statements; and if an instance is required of the historian using poetical sources without acknowledging them, we need look no further than the second part of this same sentence (τέως δὲ . . . περικείμενοι), which can only be based on study of the tactics of the Iliad.

Yet most scholars would, I think, find it difficult to imagine Herodotus carrying out such systematic research among written texts, and we may therefore turn to the second explanation as being perhaps the more probable alternative: that Herodotus, in keeping with his common practice to which he has admitted in this very chapter, is repeating a contemporary oral tradition—a tradition which also finds an earlier and much less detailed expression in the lines of Alkaios and Anakreon, who may even have initiated it. One cannot tell what grounds, if any, Herodotus had for his own further inference that the Carians had invented these arms: possibly it was merely the chauvinism of a native Halicarnassian (as also with his account of the ‘Ionic’ chiton, below, p. 116). Certainly the poets give no adequate grounds for such an explicit statement. The tradition, one must note, cannot have been confined to Caria: for Herodotus is here contrasting the Cretan version with the Carian, and would hardly include under one tradition an important tenet of the other. Rather, it was a notion familiar throughout Greece: but we have only Herodotus’ word that it included a specific tradition of invention. Later commentators, as has been noted with some surprise, prefer to give the vague reference of Anakreon as their authority for saying that the Carians invented δχανα, rather than the much more explicit evidence of Herodotus: but this would be natural if they assumed, as I have done, that poet and historian represented the same tradition. Anakreon was, after all, a more direct witness, since he was probably personally familiar with the weapons in question.

Strabo clearly draws on both the poets and Herodotus in his version. By his time no doubt the oral tradition will have been dead, since the objects concerned were long since obsolete. But now there was an important additional source: the writers on εφήματα, who may have begun with Theophrastos in the fourth century, and of whom Ephoros was certainly one of the most important. It was surely from these that Strabo drew his supporting evidence, the quotations from Alkaios and Anakreon. At the same time, he must have found that nothing in the earlier written sources justified Herodotus’ confident statement, for, as we have seen, his own version is considerably toned down.

We have, then, a specific claim, possibly by his Cretan informants but in my view by

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9 It is, however, possible that Herodotus’ acquaintance with the texts would derive from oral recitation.
11 Lorimer, Homer and the Monuments 193 n. 1.
12 Diog. Laert. v 47.
Herodotus himself, that the Carians invented 14 three military devices; and a more circumspect notice of Strabo that these same three devices were called 'Carian', and so were evidence of Carian military propensities. Strabo clearly takes the references of Alkaios and Anakreon to mean that 'Carian' was the habitual epithet of any crest and any shield-armband. Herodotus, if he used these references, will have done likewise: but he makes further inferences that are, so far as we can tell, his own.

In later years the tradition, though it blossomed as traditions will, followed in the main the same two courses that we have seen foreshadowed. One group of writers, whose sources must have been exclusively literary, ascribes to the Carians the invention of these, and indeed of other, military innovations: the other group simply states, as Strabo appears to mean, that the Carians were noted for the practice of using these pieces of equipment. Both groups could have used sources other than those we have considered above: but only in one case is such a source indicated.

The first group, which represents the Carians as inventors, is not a very reputable company. The Elder Pliny is its earliest representative after Herodotus: he gives a bald list of inventions by various peoples, including the words '[invenunt] . . . ocreas et cristas Cares'. 15 The introduction of greaves is surprising: I know of no other authority for this, save perhaps the Oxyrhynchus Christomathy (second century A.D.) which generously credits the Carians with 'taios aspiow oxaia kai e[ter] opila'. 16 The Carians, it was no doubt felt, should appear in such a list of ephýmata wherever they could be accommodated. Several later commentators also subscribe to the theory: Schol. A on Iliad viii 193 remarks of Homer's heroes that 'oúpsu gáp érówno týs pórtašu, ouš óxana ekálwv . . . ósteren gev ódóv épkeñosn kápo Járón, ouš kai 'Anakréón phýs' (again quoting the fragment cited above, p. 107). 17 Here we see a writer actually making the unjustified inference that, because Anakreon called his arm-band a 'Carian-made handle', this means that the Carians invented such arm-bands. The first statement of the Scholiast, 'oúpsu . . . ekálwv', is very probably derived from Herodotus' account; 18 the second, presumably, taken from the writers on ephýmata, but without the caution of Strabo.

Another relevant scholiast is on Thucydides i 8.1 (a passage to be discussed below, p. 113), where we read 'ól Kárês prátwi éfron tóu ómofálous tóv aspidow kai tóv lóφwv. tóv ouš anvbýδa kai synésbapton aspíódakwn mikrovn kai lóφwv, sýménov tís ephýmatos'. Here once again we find the expected crest, together with an entirely new attribution. The omphalos or projecting shield-boss is a characteristic of at least one type of pre-hoplite, single-grip shield, 19 but is quite incompatible with the hoplite type. Its presence would exclude a sýménov or blazon, and its appearance here must be due to confusion.

A more important authority, though his evidence is vaguer, is Aelian. He states that the Carians began the practice of serving as mercenaries, and fitted óxana to their shields and crests to their helmets. 20 It is not specifically claimed that they invented them. There are two points to note here: first, the association of the equipment with mercenary service and secondly, the fact that these are the same two items for which Alkaios and Anakreon provided evidence: the shield-device or blazon is here omitted. This is a sign of dependence on these same sources, which we have postulated as the dominant ones throughout, and

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14 I do not think that the distinction between 'kata/déizartes' in the first two instances, and 'poumpángwv' in the third, can be pressed, as was held by George H. Chase, HSCP xiii (1902) 62 n. 3. Miss Lorimer (BSA xlii (1947) 131) suggests that 'épidéizartes' is a curious word to use for the attachment of a crest: yet it is so used in, e.g., Aristophanes, Frogs 1039, and the archaeological evidence for other methods is only very rarely available.

15 Nat. Hist. vii 200.

16 Ox. Pap. x no. 1241, p. 106, col. iv, lines 28–30. The verb used is again 'kata/déizartes', which makes it likely that Herodotus is the source for the statement.

17 Et. Mag. 489, 39: Et. Gud. 297, 43 'Káromepnéwv óxanov'.

18 So Lorimer, Homer and the Monuments 193 n. 1.

19 Ibid. 173–80, 183–5. This account, though out of date in some respects, is adequate for our present purpose.

20 De Nat. An. xii 30.
which indeed were to be quoted yet again in a much later commentator, Eustathius. The tradition reaches its most absurd pitch, however, at the hands of Polyainos, who tells us that the future Pharaoh Psammetichos I, at the time of his seizure of power, hired Carian mercenaries on the strength of their having invented the helmet-crest, which information he (Psammetichos) learned from a Carian named Pigres.

These accounts of ἐφήμετο, though they add to the bare evidence of Alkaios and Anakreon, could all be in the main derived from them, directly or indirectly. Certainly they never cite any other source. The Iliad Scholiast and Eustathius definitely, and Aelian probably, depended on the poets: while the Scholiast is probably indebted to Herodotus as well, as the Oxyrhynchos Chrestomathy may have been (p. 110, n. 16 above). The remainder—Pliny, Polyainos and the Thucydides Scholiast—involves themselves in varying degrees of improbability as they stray from the main sources. As we shall see, there is no likelihood of greaves or omphaloi being in any sense of Carian origin, while the logical absurdity of Polyainos’ anecdote is evident. This enables us to conclude that the fragments of Alkaios and Anakreon, if not the only source that such writers could draw on, were certainly the best source.

Our second group, those who merely cite the Carians as military practitioners, has only one certain representative, Plutarch, to put beside the more equivocal Strabo and Aelian. He offers the first definitely independent piece of evidence, when he tells us that the Persians called the Carians ‘Cocks’, on account of the crests that they habitually wore. The statement, which refers to the period of the battle of Cunaxa (401 B.C.), is perhaps true: the anecdote which precedes it would otherwise be pointless. It may be significant that there is no mention of invention here.

For further evidence, we must turn to two well known but difficult passages in fifth-century literature, which may be illuminated by the above discussion. The first is an exchange in Aristophanes’ Birds. The four single birds, whose arrival precedes that of the main chorus, have entered one by one and been greeted with ribaldry from the two travellers. The last of them is called the Glutton, a name which at once reminds Euphides of Kleonymos, the poet’s invertebrate butt. But Peithetairos, at line 290, sees a difference:

ΠΕΙ. πῶς ἂν ὡν Κλεώνυμος γ’ ὡν ὡν ἄπεβαλε τὸν λόφον; 290
ΕΥ. ἀλλὰ μὲν τοι τῆς ποθ’ ἤ λόφωις ἤ τῶν ὀρνέων;
ἤ’ τιν τὸ διαυλον ἠλθον; ΕΠ. ὧσπερ οἱ Κάρες μὲν ὡν
ἐπὶ λόφοις οἰκώναι, ὑγαθ’, ἀσφαλείας οὖνεκα.

After our review of the evidence, in which nine different authorities were found to connect the Carians with helmet-crests, it might seem superfluous to point out that one, perhaps the primary, sense of λόφων in 293 must be to do with helmet-crests. This will affect the meaning of λόφωις (ἅπλ. λεγ.) in 291 and, less certainly, of λόφον in 290. Yet in one of the more recent discussions of these lines, by H. Lamar Crosby, it was argued that on each occasion the words are used in the first instance as geographical terms, referring to the crest or ridge of a hill.

It is likely that some kind of pun is intended at l. 293. But we should note that there are three possible meanings of λόφος involved in the word-play hereabouts: (a) the crest on a bird’s head, (b) the helmet-crest and (c) the crest or ridge of a hill. The word appears first at l. 279, where the previous bird enters and is greeted with ἵππος ἀδ λόφον κατειληφώς

21 367, 25 and 707, 60.
22 Strat. vii 3.
23 Artaxerxes, x 11.
24 Hesperia, Suppl. viii (1949) 79-80.
τὸς ὤρνης ὀντος. Here a pun may again be intended: the phrase 'λόφον κατεύρησος' is most easily understood in the geographical sense, 'who has seized a vantage-point'. 26 Yet the sequel shows that most, if not all, the four birds which have entered are to be imagined as having prominent crests on their heads. This should therefore be a play on meanings (a) and (c) above.

This brings us to l. 290, where another pun provides the best joke in the passage. The notorious cowardice of the ρήδασμος Kleonymos is used for a play on meanings (a) and (b). Any suggestion of the third, or geographical, meaning would be inapposite: and Crosby, by taking λόφος to mean 'hill-crest' here as well, drowns humour in absurdity. He remarks that absurdity is the particular role of Euelpides; but unfortunately for this interpretation, most editors since Meineke have given the line in question to Peithetairos. 26 Similarly, λόφων in the next line can only mean 'wearing of crests' in such a context: the costume of the birds would at once make the question intelligible. The other interpretation of the word, as 'the act of sitting on crests', which Crosby perforce adopted, is most improbable etymologically.

Lines 292–3 involve a more complex problem. As we have seen, the mention of the Carians points inevitably to the meaning 'helmet-crests', (b), for λόφων: yet the construction of 293 is intolerably strained by any translation other than 'they live on (hill-) crests' (meaning (c) above). 27 Since the notion of the birds' crests, (a), has also been present throughout, and stressed in the immediately preceding passage, I see no alternative but to regard this as a triple pun. The three meanings have all been aired in the double puns of the preceding lines, and this would be a suitable climax, although the joke, as is natural with such complexity, loses sharpness. Plutarch's anecdote about the Carian 'αἰλεκτρυνές' (see above, p. 111) may add to the pointedness of meaning (a) in this passage. 28 It is also worth observing that other occurrences of λόφος in this play, as at 94 ('τριλοῦσια') and 1366, certainly refer to helmet-crests: while there is plenty of evidence that Aristophanes thought helmet-crests humorous, notably the Lamachos scenes in the Acharnians. 29

In adopting this explanation I am, in part, reverting to the view of most authorities before Crosby, and of at least one since. 30 So too in the interpretation of διαυλον in 292. In most recorded uses of this word, the meaning is a double race, to a turning-point and back again. Most evidence that this race was run in armour is slight: Pausanias indeed implies that it was not always, when he tells us that the diaulos was instituted in the 14th Olympiad (724 B.C.), and the hoplitodromos not until the 65th Olympiad (520 B.C.). 31 Yet we have Pausanias' own evidence that in later days the diaulos was run in armour, or perhaps that the race in armour had become a double race. 32 This being so, Crosby's suggestion, that διαυλος here means a double pipe, loses probability, for surprisingly there is no parallel for such a usage. 33 A quick joke like Euelpides' is pointless if the obvious meaning is not the right one.

26 Ibid. 58.
27 Schroeder's attempt (op. cit., 41) to salvage the meaning 'helmet-crests' here, by taking ἔτι as not locative but modal, is not really successful. The parallel phrase he cites in Euripides, Phoenissae 1467, 'καθ' ἐμέ Κάθησα λαος ἄσπιδων ἔτι', is itself ambiguous in the same way; and it is very doubtful whether οἰκέω could be used in the sense required, to 'remain' or 'be continually'.
28 So too may the remark in Artemidorus, Onirocritica iv 24: 'Sic enim et diaulodromos gallus fit, per aulam enim currit'. The etymology is rightly ridiculed, but the remark shows that the cock, presumably on account of its crest, actually was given this name.
29 Ach. 572 ff., 1072 ff.
30 H.-J. Newiger, Metapher und Allegorie (Zetemata 16 (1957) 85–6). On the other side, B. B. Rogers (The Birds of Aristophanes (1969) 38) reasonably argued that the Carians did in fact live habitually on hilltops, but had to admit that this was a 'curious coincidence'.
31 v 8.6: v 8.10.
32 ii 11.8: x 34.25. Cf. the Scholia on Birds 292, and Pollux iii 151.
33 Although, as Crosby says, both διαύλος and διαύλεω are used in a musical sense. Cf. n. 28, above.
CARIAN ARMOURERS: THE GROWTH OF A TRADITION

We may therefore claim Aristophanes as a witness for the currency of our tradition in the late fifth century—apparently in its more moderate form, though not necessarily to the exclusion of the 'inventor' version. As with the previous stage in the transmission of the tradition, from the lyric poets to Herodotus, so here one could argue for a purely literary transmission. It is very clear that Aristophanes possessed, and could assume in his audience, a close acquaintance with the text of Herodotus—particularly at the dates of producing the Acharnians and the Birds, and particularly with Book I of Herodotus. The passages which parody Herodotus are too well known to require discussion here.\(^{34}\) It is conceivable that even so recondite a matter as the military history of the Carians had become familiar through the publication (and perhaps the recitation) of Herodotus' work. But once again, I think one must concede that the alternative explanation is more plausible: that the oral tradition of 'Carian' military devices, apparently current in some form down to the time of Herodotus, was still alive in 414 B.C., and prompted the joke in the Birds.

The second passage is even more controversial: Thucydides' brief excursion into Carian archaeology in i 8.1 (the Scholiast on which was quoted above, p. 110). When the Athenians purified Delos in 426/5 B.C. and removed the dead buried there, 'τῶν θηρίων ἀναβρασθέντων ... ὑπὲρ ἡμῶν Κάρης ἔφαγαν, γνωσθέντες τῇ τε σκευῇ τῶν ὀπλῶν ἔστεκαμένη καὶ τῷ τρόπῳ ὁ νῦν ἐπὶ βάττουσιν'. This passage has been almost exhaustively discussed, most recently by R. M. Cook\(^ {35}\) and Charlotte R. Long.\(^ {36}\) All authorities are now agreed that Thucydides was wrong in his identification of the graves: they differ as to what the 'Carian' graves really were, and whether they are among those whose remains were reinterred in an enclosure on the neighbouring shore of Rheneia, and excavated in 1898 by D. Stavropoulou.\(^ {37}\)

I can offer no new answer to these questions, which are not strictly relevant to our purpose here: in general, I would incline to accept Cook's account. But I would suggest that Thucydides' criteria for 'Carian' graves must inevitably have been affected by the tradition which we have been considering, and to which his contemporaries subscribed: especially since Thucydides, like Herodotus, discusses the Carians and their arms and migrations in the context of Minos' reign. This brings us to the difficulty of the words 'τῇ σκευῇ τῶν ὀπλῶν ἔστεκαμένη'. I agree with Cook that the mere presence of the arms is itself being stressed: but I cannot believe that a writer whose style was of such close verbal texture as Thucydides, would have used this cumbersome phrase to mean only 'the fact that they were buried with arms'. σκευῇ must imply something as to the nature of those arms.\(^ {38}\) And yet according to our tradition, the characteristic Carian arms would be, ex hypothesi, indistinguishable from early Greek ones: 'ἠσπαστὴρ ἡμᾶς ... τοῖς Ἕλληνες ἐφήσαντο'.

It seems to me therefore most likely that Thucydides is here making two quite separate points. The fact that the dead were buried with their arms (ἔστεκαμένη) indicated, as he thought, that they were not Greeks—as Cook has argued. Yet the arrangement of the arms (σκευῇ) was so close to that of early Greece, that he could only conclude that, of all barbarians, they must be Carian. Wherein did this similarity consist? Surely it was in some way connected with the invariable attribute of the Carians, the helmet-crest; and perhaps also with the hoplite shield. The consequences of this will emerge in the next section.

\(^{34}\) Acharnians 85 f. and Hdt. i 133: 524 f. and Hdt. i 1–4: Birds 552, 1124 f. and Hdt. i 178–9: and perhaps 961–2 and Hdt. viii 77. See J. Wels, Studies in Herodotus 169 ff.

\(^{35}\) BSA 1 (1955) 267–70.

\(^{36}\) AJA lxii (1958) 297–306.

\(^{37}\) Praktiká 1898, 100: K. A. Rhomaïos, Deltion xii (1929) 181–223.

\(^{38}\) So Gomme, Historical Commentary on Thucydides i 107: Long, op. cit., 304 f.
B. Archaeological Evidence

The archaeological record, though it has been substantially improved during recent years, remains quite inadequate for one important field—that of Caria itself. While a number of sites have now been examined, few of them include excavated graves and fewer still have any substantial material that can be dated to the years between the Bronze Age and the seventh century B.C. Further, it is open to doubt how far certain of the sites can be regarded as native Carian, rather than Greek, in nature. There are still only two relevant cemetery sites, Assarlik (almost certainly the ancient Termera) and Gökcüler (probably Pedasos). Of these, Assarlik, with its graves of Protogeometric to Archaic date, shows every sign of having begun as a Greek migration settlement and, to judge from the earliest pottery, one people specifically from Attica. Gökcüler, where 'local geometric pottery' and fragmentary bronze arms are reported, sounds a little more promising, but publication is still awaited. We still have no archaeological information about Carian armour at the period in question, and little about Carian burial-practices.

We must therefore turn to Greek archaeology for evidence of the introduction of helmet-crests, hoplite shields and other armour to Greece. I discuss these matters at length elsewhere, and give only the essential references in the account that follows. To take crests first: the plain assertion that the Carions invented the helmet-crest as such, and introduced it to Greece is, as has been pointed out, absurd. Not only are crests of several types known in Greece from the period of the Shaft Graves onwards, but they reappear in eastern Anatolia as early as the ninth century B.C.: the Assyrian artists of Shalmaneser III portray the king's Urartian enemies as wearing them in the campaigns ending in c. 850 B.C., and figurines of soldiers with crested helmets have occurred in the Urartian capital of Toprakkale.

It was doubtless from this source that one form of crest, which lay directly along the crown of the helmet, was diffused over north Syria, an area known to be under Urartian domination during the eighth century. That it spread from there to Greece is a more questionable assumption, but the presence of at least one Greek settlement at this time on the nearest part of the coast, at Al Mina, makes it at least plausible. It appears in Greek

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42 JHS xvi (1896) 202, 244 f.: BSA 1 (1955) 123–5, 149, 165–7. But to these we must add the site of Iasos, recently excavated by D. Levi. See Anuaria, n.s., xxiii–xxiv (1961–2), 505–71, and especially the prehistoric cemetery, described ibid., 555 f., which produced Protogeometric pottery but no arms, apart from a bronze axe. A Protogeometric tomb at Dyrmen is described in AJA lxvii (1963) 357–61.

43 Desborough, Protogeometric Pottery 218–21.


45 At Assarlik, cremation had been the universal rite. Thereafter, the rite seems to be uncertain, although according to a recent authority (E. Akurgal, Die Kunst Anatoliens, 162), cremation still remained the rule: this could mean that Thucydides is specifically referring to cremation. Among the types of tomb, cist-graves in enclosures, chamber tombs and rock-cut tombs are all known (BSA 1 (1955) 165–7). Long (op. cit.) suggests that Thucydides mistook Mycenaean chamber tombs for Carian tumuli, but the differences were patent. Mycenaean chamber tombs have now been found in Caria, AJA lxvi (1963) 353–7.

46 In Early Greek Armour and Weapons (Edinburgh, 1964).

47 E.g. Lorimer, BSA xlii (1947) 131.

48 Homer and the Monuments 229 ff., fig. 5, pl. 15,1–4.

49 L. W. King, Bronze Reliefs from the Gates of Shalmaneser pls. 37 ff.

50 Iraq xvi (1954) pl. 22A–C.


52 Cf. J. Boardman, BSA lli (1957) 29 ad fin.
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art before the end of the eighth century, and becomes extremely common thereafter. It is difficult to see what part Caria could have played in its diffusion: but in any case it is unlikely that this crest should be identified with that of Alcaeus, which could be 'shaken' and which also perhaps 'nodded from above'.

A likelier identification is with the other main form of crest introduced to Greece at this time, which rose high above the helmet on a support and emphasized the movements of the wearer's head. Here we can be more explicit about the immediate source. There are two varieties of the crest in question. One is only partly supported by the stilt; stilt and crest alike curve forward at the top, while at the back the crest is anchored to the helmet itself. The other variety rests entirely on the stilt, to the top of which it is attached; from here it curves downwards, front and back, describing almost a full circle and nearly touching the helmet both in front and behind. Both forms occur in Greek art (and the second in an actual find from a grave at Argos) near the end of the Geometric period, rather before 700 B.C. Both forms are also well known in the immediately preceding period in Assyria. The resemblances are close: for the first type, we may cite a relief of Sennacherib (705–681) and an actual specimen dating probably from that king's destruction of Lachish in c. 700 B.C.: earlier Assyrian reliefs show a very similar shape of crest. There can be little doubt that this is the model for the earliest crests of this type in Greek art.

With the other variety, there is even less room for doubt. The helmet from Argos (see n. 54) finds a remarkable analogy, in the shape of helmet and crest alike, on a relief of Tiglath-Pileser III (745–724): a wall-painting dating probably from the same reign again reproduces the crest very closely. It is of course just possible that all these Assyrian finds represent foreign auxiliaries in the king's service: it is inconceivable that all, and unlikely that any, are Carians. There is no reason why the Greeks should not have derived the crests direct from the Assyrians, with whom they were certainly in intermittent, if hostile, contact.

To revert briefly to the Thucydides passage discussed on p. 113: if there is any truth in the contention that the historian was thinking of the same tradition about Carian arms as the other ancient writers, then it is probable that his 'Carian' graves would have contained helmet-crests. Such crests have in fact been found twice in Greek Geometric graves, but not otherwise. This lends further weight to Cook's suggestion that the graves in question were Geometric. Hoplite shields, however, have not occurred in graves in Greece proper, at this or any other time. The remote possibility that Thucydides was really thinking of the omphaloi of an earlier type of shield, as the Scholiast suggested, would lead us to roughly the same chronological period as the helmet-crests indicate (see below, pp. 116 f.).

It may seem irrelevant to dwell on the events of the late eighth century when Herodotus appears to date the invention and transmission of the crest to the time of 'Minos'. But I have suggested that the statement of Herodotus is not part of the 'Cretan version' in which it occurs: and the other items in Herodotus' and Strabo's lists, the ὀξώα and the bazonos, point inevitably to the period in question. Both are characteristic of, and peculiar to, the Greek hoplite shield, which also makes its appearance in the last quarter of the eighth

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58 E.g. on the bronze relief from Knossos (Brock, Fortetta pls. 115, 168), the Hunt Shield from the Idaean Cave (Kunze, Kretische Bronzereihs pls. 10 f., Beilage 1), and perhaps a terracotta from Amyklai, Athenlitt. Iv (1930) Beilage 42.2 and 43.2.
55 Handbuch der Archäologie (ed. W. Otto) i pl. 170.2 (Berlin).
56 Lachish iii 98, 387, pl. 39.1.
57 R. D. Barnett and M. Falkner, The Sculptures of Tigrath-Pileser III pls. 36, 42.
58 E.g. on a bronze relief from Kavousi (Kunze, op. cit., 218, fig. 31): terracotta votive shields from Tiryns (Homer and the Monuments pls. 9, 10): figure from Olympia, Olympia IV (Die Bronzen), pl. 15 no. 247.
59 Barnett and Falkner, op. cit., 32, 39, pl. 73.
60 F. Thureau-Dangin et al., Til-Barsib, pl. 49, bottom.
61 See e.g. L. W. King, JHS xxx (1910) 327–35.
62 At Athens (W. Reichel, Homerische Waffen, 110, n. 1) and Argos (supra, n. 54).
century. It is detectable in art at first by its distinguishing device or blazon: this generally takes the form of a bird or animal, or the head or fore-part of one, and so was not invertible. It is seen on at least two Late Geometric vases: inside views of the shield, which show the distinctive arm-band and hand-grip, are not available until the early seventh century. It has been generally assumed that this shield was an original Greek device, since neither the blazon nor the two-handled arrangement is known elsewhere at this date. The kingdoms of Assyria and Urartu, once again, used a shield of a comparable size and shape: but it remains a bare possibility that the specific devices mentioned by Herodotus and Strabo, and first known in the art of eighth-century Attica, were invented across the Aegean, in Caria. One can only say that the archaeological evidence gives no inkling of this, and that the circumstances make it highly unlikely.

There remains the possibility that, in Greek tradition, 'Carian' was used as a general or 'blanket' term for Oriental barbarians. The case of the helmet-crests might incline us towards this view: and a possible parallel is provided by the linen chiton which the Greeks called Ionic. The word 'chiton' itself, and the fact that it was of linen, indicate that its origin should lie in the Levant or Egypt: furthermore the word is now considered to occur, in more than one case, on Linear B tablets from Knossos. Yet Herodotus, once more, attributes the chiton to Caria. One can only guess at the reason for crediting the Carians with the invention of a garment that cannot have originated in their land; it could be simply because the writer himself was a Carian, or perhaps because the Carians served as intermediaries, or else because their name was used as a substitute for those of more remote and unfamiliar peoples. If this last were the case, then it is not impossible that, in the field of armour, tradition substituted Caria for Assyria or Urartu by a similar process. I believe, however, that a more convincing explanation is available.

The other arms attributed by later writers to the Carians, greaves and omphaloi, may be briefly dismissed. The greave is now well-established in Mycenaean Greece: it reappears, both in representations and in actual finds, in the early seventh century. At both periods it is an object almost exclusive to the Greek world. The omphaloi or shield-boss is a more doubtful case, since scholars are not agreed in the identification of the evidence. A series of metal discs, with a domed centre often incorporating a spike, have been found in graves and sanctuary sites all over Greece, not to mention examples in

63 S. Benton, BSA xlviii (1953) 340.
64 Athens, Benaki Mus.: BSA xiii (1947) pl. 19: Eretria, BSA xii (1957), pl. 3 A, a vase which is given by Miss J. M. Davison, Attic Geometric Workshops, 67-73, to the Attic workshop of the 'Sub-Dipylon Hand', perhaps as early as c. 725.
65 The only alleged precedent, on an Egyptian relief of the New Kingdom, was found to be based on an inaccurate drawing: Homer and the Monuments, 151-2, pl. 6.1. It is worth noting that another later literary tradition ascribed the invention of the shield to Argives, and indeed the hoplite shield was certainly known at some periods by the name 'άσπις Ἀργολική': Dion. Hal. i 21. 1: Pausanias viii 50.1: Οἰχ. Ποπ. x no. 1241, p. 106, ll. 14 ff.: Pindar, fr. 95 (Bowra)—but it is not certain that shields in particular are meant here. See E. Kunze, Olympische Forschungen ii 216 for a discussion of this question.
66 Assyria: Layard, Nineveh and Babylon 193-4. Urartu: Iraq xii (1950) 1-43, fig. 8, pls. 9, 10; B. B. Piotrovskii, Karmir-Blur iii (Erivan, 1955) 27, fig. 17, pls. 1, 12, 13.
68 v. 88.1.
69 Phrygia also may possibly come into the picture, since a Phrygian ivory found at Gordian, and dating from before the Cimmerian sack of c. 680, shows a mounted warrior with a crested helmet and shield (non-hoplite), very close to the early Greek types: AJA lxiv (1960) 240, pl. 60, fig. 255. But it is perhaps likelier that the ivory is Greek-inspired.
70 See now C. M. Bowra in Mnemosyne xiv 2 (1961) 97-110: and add an even earlier possible example from Dendra, AE 1957, παράρτημα p. 17.
71 The first representation is perhaps on the Hymettos amphora, CVA Berlin i pls. 43-4 (c. 675): the earliest actual examples, from Kavousi, Annuario xiii-xiv (1930-1) 88, fig. 31.
72 See Homer and the Monuments 154-5, to which many more recent examples may be added.
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Central and Northern Europe, the Caucasus, the Levant and Luristan. At different times, particular examples have been identified as cymbals, 73 shield-bosses, 74 corslet-attachments, 75 belt-ornaments 76 and items of horse-harness. 76a I am convinced, after investigation, that many of the specimens found in Greece are in fact shield-bosses, from the centres of full-sized shields made of hide. The fact that they are themselves small (the average diameter is about 15 cms.) could perhaps explain the Thucydides Scholiast’s reference to miniatures. These discs occur frequently in graves (often warrior-graves) from the early twelfth century to the late eighth—the very period when evidence for the use of the grave is almost totally absent. Any connexion between the two, or of either with the Carians, would have to rest on vastly more impressive evidence before it could deserve serious consideration.

C. Conclusion

It might be argued that, before a tradition of such persistence can be rejected, it must be proved false beyond all possible doubt: this I have hardly done. It may, on the other hand, be felt that the tradition is too worthless to merit serious discussion. I think that the truth lies between these two views. We can, I suggest, explain the growth of the tradition in human terms. We have inferred that, by the fifth century, an oral tradition, couched probably in vague terms, connected the Carians with certain military devices. Our earliest evidence consists of two slightly equivocal references from lyric poetry, and it is quite possible that the whole subsequent tradition stems from these two very lines. After c. 400 B.C., there is no sign that the oral tradition was still alive, and later writers use almost exclusively literary evidence for their attributions. We have seen that one version of the tradition, beginning with Herodotus, which holds the Carians to be the inventors of the arms in question, has only a remote possibility of archaeological confirmation, even if modified. It remains to explain the other, more plausible variant, and the original quotations themselves. I do not think that we need look far for an explanation. 77 The complete absence of our tradition from Homeric epic might in any case incline us to seek its origins in a later period; and the fact is that, from the seventh century onwards, the Carians were famous over a wide area of the Hellenic world, as the first great mercenary force of historical times. Our earliest and best evidence is a line of the contemporary Archilochos:

Fr. 24 Bergk, 40 Diehl: ‘καί δὴ ’πίκουρος ὅσ τε Κάρ κεκλησθοιμι’

But we have also the well-known statement of Herodotus that Ionians and Carians were the ‘Brazen Men’ who came to the help of Psammetichos in c. 663 B.C. 78 The very phrase implies that these men were equipped as hoplites, for they would not otherwise have been a remarkable sight in Egypt, a country where the scale-corset had long been used, 79 and the bronze helmet apparently familiar in the immediately preceding period. 80 Thus, at a crucial period in the development of Greek armament, the Carians were indeed the most famous exponents of the new style of equipment, one that made its wearers the foremost heavy infantry of the day. No wonder that Anakreon’s πόρμαξ was a ‘Carian-

73 H. Thiersch, AA xxviii (1913) 47 ff.: E. Kunze, Olympiabericht v 118.
74 K. Kübler, Kerameikos iv 27.
75 W. Ridgeway, The Early Age of Greece i 421.
77 Compare W. Helbig, Homerische Epos (1884) 248.
79 Homer and the Monuments 197-8.
80 Hdt. ii 151.2.
made handle': no wonder that Alkaïos knew of a 'Carian' crest. But the further inference that many ancient writers made, that the Carians actually invented these arms and passed them on to the Greeks, was almost certainly the reverse of the truth. In many of the great innovations which took place in Greece in or about the later eighth century—armour provides several cases, and the adoption of the alphabet is another example—the pattern is emerging more and more clearly, as one of the Greeks themselves going and seeking out these innovations from their source, however far away that might be. The nearer barbarians to the East and North seem to have contributed comparatively little, and in many cases were soon to be attracted into the Greek orbit. There is every sign that Caria conformed to this pattern.

Our 'inventor' tradition, then, is not history: but nor is it myth. Rather it is a rationalisation, more or less speculative according to the author concerned, of evidence ultimately based on historical fact. It is perhaps significant that some modern writers have carried the tradition further than even Herodotus.

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81 It is hardly mere chance that the Carian Thalassocracy was dated to a similar period in the Canons of Eusebius (who gives '730–671 B.C.') and Jerome ('720–671'): Myres, JHS xxi (1906) 107. See also A. Schulten, Rhein. Mus. bxxv (1936) 293 for an optimistic estimate of Carian merchant enterprise, based on place-names in Morocco.


83 Thus Paton and Myres, JHS xvi (1896) 267, attribute the whole hoplite panoply to the Carians.
ON THE POSSIBILITY OF RECONSTRUCTING MARATHON AND OTHER ANCIENT BATTLES

[This paper was read to the Oxford Philological Society on October 22, 1920. Present: J. L. Myres (in the chair), A. C. Clark, J. A. R. Munro, J. K. Fotheringham, G. B. Grundy, E. M. Walker, W. W. How, J. U. Powell, R. H. Dundas, D. C. Macgregor, N. R. Murphy, G. H. Stevenson, M. N. Tod, H. M. Last, F. P. Long, J. Bell, and five visitors. It has never been published, though Mr Whatley has given a brief account of Marathon in the Proceedings of the Hellenic Travellers Club, 1927. It was intended to provoke a discussion (in which it failed) rather than for publication, and the author had thought that the publication of CAH iv made it out of date. It has, however, been in private circulation for many years, and Mr Hignett, among others, has acknowledged its influence. Mr Whatley has been persuaded to believe that it is perhaps only superficially out of date, and it is printed here more or less as it was delivered.]

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I must begin with an explanation of my reasons for writing this paper. I have for some years been interested in modern attempts to reconstruct ancient campaigns and battles—especially those between Greece and Persia—in fact, most of the arguments I am using tonight were first written down in a rather different form in 1913. But I should not have thought it worth while to read them to this Society had not Admiral Cusance’s book, his lectures, his address to this Society and the discussion which followed it made me feel that this subject is still one of fairly general interest, that we are still far from arriving at certainty with regard to the history of ancient fights, and that it may be worth while to raise some general questions such as, ‘How far it really is possible to reconstruct ancient battles with any finality’ and ‘how far the methods of attempting to do so usually followed by modern writers really are the soundest methods to employ’. I felt, for instance (and I think others did too), in the case of Admiral Cusance, that so long as he was using his expert knowledge in cases where there was no doubt about the essential facts and no question of motive or intention, he was most convincing and illuminating. The Athenian fleet at Syracuse was made to wait upon the army and this did nullify its possible usefulness. Similarly his professional knowledge led him to insist on the fact (already emphasised by Macan, Grundy and Tarn, but constantly forgotten) that ancient ships in line of battle cannot have been packed together side by side like hoplites. But he was less convincing when he proceeded to fix the interval between ships, when reconstructing the detail of the different fights and when interpreting the minds of generals and statesmen. I do not propose tonight to cover the same ground as Admiral Cusance. But I hope it will be clear that whatever ancient campaign is selected for illustration the same fundamental problems with regard to the evidence arise in more or less degree. I want, if possible, to raise these fundamental questions for others, better qualified than myself, to discuss.

Nor do I propose to offer any fresh reconstructions. I am afraid that the more I study the subject the more sceptical I become about the possibility of reconstructing the details of these battles and campaigns with any certainty and of discovering what was in the minds of the admirals and generals who conducted them. In saying this I do not, of course, mean to suggest that the attempt at reconstruction should not be made. Not only would such a suggestion be useless, for no one can read Herodotus’ account of Marathon, for instance, without automatically trying to reconstruct it; but also the mere fact of playing about with such problems must, I suppose, increase our familiarity with—
ultimately, perhaps, our knowledge of—ancient life. But it must be frankly admitted that there is so far very little to show as a result of a generation of controversy in the way of undisputed new knowledge. Almost the only new fact concerning the Persian Wars about which all modern writers are agreed is the negative one that Xerxes' army was not really nearly so big as Herodotus represents it. And I cannot help thinking that modern writers often spoil a valuable piece of criticism or research by finding it necessary to tack on a constructive suggestion of their own. Obst's *Feldzug des Xerxes* is a good example of this. The book consists mainly of a thorough and valuable analysis of the narrative of Herodotus in relation to his sources and a summary of the chief modern views. Obst appears to have just the kind of mind for doing that part of the work excellently. Unfortunately professional etiquette compels him to tack on original views of his own with regard to the chief battles and these, with possibly one exception, are merely futile.

I have some pet theories of my own about the Persian Wars, but they are the merest theories and to drag them into this paper would merely obscure my main idea of reviewing the whole situation. My rather brief experience of teaching makes me strongly of opinion that such general reviews are occasionally healthy. For inexperienced students at any rate the undiluted study of reconstructive theories is apt to have two undesirable effects: first, that they accept what are only clever hypotheses as established truths (I have constantly found this to be the case with the Battle of Marathon); second, that they lose the wood for the trees.

We shall never know exactly what happened at Marathon, but we know enough to be able to allot its place fairly satisfactorily both in the History of the Art of War and in the Histories of Greece and Persia. I am not sure that this fact is not sometimes overlooked in the excitement of the hunt for the Persian Cavalry.

Of all controversial subjects, military history seems especially to stir very deep passions. I suppose there is usually so little evidence that what there is must be used with unrestricted force if it is to carry any conviction. But in this country, where military reconstruction is particularly popular, strong opinions have been expressed and criticisms uttered without much bitterness. We have not descended to the personal abuse which accompanies the controversies conducted by Delbruck on the one side and by Kromayer and Veith on the other. I hope that any controversial criticisms I make in this paper will be regarded as being much more humble in spirit than might be suggested by the rather bald expression of them which limitations of time make necessary. I want to emphasise this point, for the matter is a rather delicate one seeing that several of the authors I shall criticise are senior colleagues of my own (and actually present in this room). I only wish that there was time for me to begin by attempting to express my consciousness of the immense amount I owe to them.

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To begin with I want to make a few remarks about military history generally. Battles of all periods are difficult things to reconstruct. In battle many and different events happen simultaneously and changes are rapid. The actors are in a state of excitement and extreme nervous tension—the worst possible condition for viewing a situation with a proper sense of proportion. It is impossible for anyone to know what is happening in every part of an engagement and there is unlikely to be the occasion, even if there is the desire, for an impartial inquiry and examination of representative witnesses while the battle is recent and its memory fresh. There is the greatest difficulty in distinguishing what was foreseen from what was unforeseen, able generalship from a stroke of good luck.

It is particularly difficult to discover what was in the mind of a general. The general himself may not find it easy. No battle follows one simple plan. There are not only constant improvisations to meet new situations, but constant flukes and, above all, constant mistakes. But it is only human to forget the mistakes if they do not lead to disaster and
the flukes if they lead to success. Similarly, outside opinion inevitably tends to regard what happened as having been carefully thought out and intended, which is by no means always the case. But I must give one or two illustrations. A Fellow of this College, being interested in the question of the value of the evidence of participants with regard to the details of a battle, tried an experiment on the spot in the recent war. He had been out at night with a patrol of quite intelligent Territorials. Immediately on returning to our trenches he asked each man how many bombs he had thrown. The total of the answers came to 21: the correct total was 7. The N.C.O. was positive that the officer had recharged his revolver and that he and the officer between them had fired at least 12 shots. Really only 3 had been fired and the officer's revolver had not been recharged. In the same Battalion the Adjutant in describing a night working party said that there was a bright moon and he was surprised that the Germans did not fire: the Colonel, reporting the same occasion in the Battalion War Diary, said, 'Luckily it was a very dark night'. Similar instances will occur to everyone who has tried to reconstruct any military operation from the reports of participants. But there is similar disagreement about infinitely more important occurrences which one would at first think must from their very obvious importance have stamped themselves indelibly on the minds of all spectators. Who raised the white flag at Nicholson's Nek? Every Irish Fusilier with, I believe, absolutely sincere conviction asserts that it was a Gloucester: every Gloucester, with equal sincerity, that it was an Irish Fusilier. To whom belonged the credit of shaking Napoleon's Old Guard as it came up the slope at Waterloo? To the British Guards in front or to the 52nd Light Infantry on the flank? The point has been disputed for 105 years, and even impartial spectators of that memorable scene were at variance. There is the same sort of doubt as to what was in the minds of the generals on great occasions. Was the Battle of the Marne premeditated by Joffre or did Gallieni let him in for it? Who is right about le Cateau, French or Smith-Dorrien? And yet, how many of us on November 11, 1918, innocently thought that now we should really know all about these things. Some of them may be cleared up when reputations have no longer to be saved, but only by means of official evidence such as did not exist in the Greek world. For in the case of modern wars we are in a comparatively advantageous position. For establishing a true narrative of events we have, to begin with, a keen contemporary interest in getting at a true history; specialists employed for this purpose; the Press and its correspondents; official War Diaries kept by every unit in the field and handed over at once to the official historians; written operation orders which are preserved; field messages written or signalled whenever possible and copies of them kept. The numbers of units present on any occasion are known from orders: the strength of these units from Ration States and similar evidence. Above all, we are completely informed about the organisation of the armies engaged, their drill, formations and methods of fighting; the nature and limitations of the weapons they employ.

To assist us in discovering the plans of generals we have their correspondence with their governments, the orders they issued, their diaries, memoirs, and those of their staffs. We know what course of study they pursued at the military colleges, what previous campaigns they may reasonably be presumed to have studied. As a result we can in the case of almost all modern wars get a very good general picture both of the strategy and the fighting; but we are still constantly in doubt about two things: (1) the exact details of what happened on any particular occasion, and (2) exactly what was in the minds of the generals, what was foreseen and what unforeseen. Yet these are precisely the things which modern writers reconstruct most positively in the case of ancient battles.

It will be argued against this that ancient battles were much more simple, and from some points of view this is certainly true. Otherwise we could not even attempt to reconstruct them. Ancient armies were much smaller (though Macan and Grundy are
inclined to credit Mardonius at Plataea with an army several times larger than that of Wellington at Waterloo and than our own regular army in July 1914; but in the case of modern battles obscurity in detail is by no means restricted to the battles where large numbers were engaged, and ancient strategy and tactics were simple almost to crudity: ancient armies fought in close order and therefore in small space and the view of spectators was not impeded, as at Waterloo, for instance, by smoke. But here again there is another side. To begin with there were no expert spectators. Napoleon had an excellent view of Waterloo through glasses, waiting with his reserve, and even when his reserve went in he had no more thought of accompanying it than did Haig of assaulting the Hindenburg Line in person. But Callimachus had no reserve and was in the thick of it on the right wing; Miltiades was almost certainly with his tribal τάξεις. Alexander in person led the charge of his cavalry into the unfortunate Persians and cannot himself have had any general view of his battles. Even Caesar, whom the Roman discovery of the value of a reserve made more detached, went into the front line when the situation was critical. Secondly, the Greeks, at any rate, had no permanent Staff and no War Office to prepare plans and organise reports—a fact so often forgotten by historians of the Persian Wars. Thirdly, close order fighting is not necessarily easier to reconstruct than open—at least in detail: an Association Football forward would find it easier to reconstruct a particular match than a Rugby forward, and a cricketer would find it easier than either. Fourthly, though Greek strategy and tactics were simple they seem to have been unscientific and rather illogical. War was treated rather as a religious ordeal. There is an element of the heroic combat about many Greek fights besides those referred to by Professor Gardner in the paper on the Lelantine War which he read to this Society last year.\(^1\) In wars between Greeks and Greeks, at any rate, a request for the return of corpses led automatically to a cessation of fighting. There was no attempt to follow up a victory. The two sides went home with as little attempt to molest each other as do the rival teams after a modern football match. Similarly there is very seldom any attempt to take advantages or effect surprises—to attack an enemy’s phalanx before it is properly drawn up, for instance, which was the Roman way of dealing with a phalanx. Polybius\(^2\) tells us that οἱ ἀρχαῖοι thought little of victories gained ἄν ἀπάτης and not ἐκ τοῦ προφανοῦς, and the history of Greek warfare in the fifth century so far as we know it on the whole bears this out. But this kind of simplicity is not a help to the historian. From the military point of view it is extremely illogical. In modern times we can assume that every army is at least aiming at the crushing defeat of the enemy and is only fighting a particular battle as a means to that end. But that lack of hard logic about Greek warfare makes it as hard to reconstruct the actions of generals on a priori grounds of strategy as it is with the conventional warfare (in many ways so like Greek warfare except that it was fought on horseback) of the age of chivalry. (These last remarks do not apply to Philip and Alexander or to the Romans.)

We lack, then, in the reconstruction of ancient battles, those sources of information which are our chief assistance in dealing with modern military history—written orders, states, diaries and the like. Equally important is the fact that the ancient historians on whose narratives we have to rely were to all intents and purposes as much without this form of evidence as we are. Herodotus, certainly, had practically nothing of the kind; Thucydides very little; Roman historians rather more, because the Romans had more national instinct for preserving this kind of record. The technical military writers—Aeneas, Arrian, Vegetius and others—only rarely throw light on particular campaigns, though they are of some help towards an understanding of Greek and Roman armies. We are thrown back on ancient historical writers who usually take a knowledge of military

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\(^1\) CR xxxiv (1920) 90-1.

\(^2\) xiii 3. 2-3.
routine for granted (Polybius and Josephus, who describe what to them are foreign armies, are an exception to this and therefore particularly valuable) and whose narratives are based on very few real military documents, but on more or less careful examination of participants in the battles or of previous similarly unscientific histories. We get the best results with painstaking people like Thucydides and Polybius. When they give a reasonably coherent story (Thucydides’ account of the Sicilian Expedition, for instance) I think we may safely accept at least its main outlines. But where their accounts are not wholly acceptable or do not give us as much detail as we should like (Thucydides on Mantinea, for instance, or Polybius on Cannae) and we want to reconstruct or supplement, we at once have to fall back on mere conjecture which, as far as I can see, is very unlikely in most cases ever to become anything else.

Of all ancient campaigns we undoubtedly know Caesar’s best. In studying them we can start with a very fair knowledge of the topography of his various campaigns; quite reasonably good knowledge of the numbers engaged—at any rate on Caesar’s side; and of the organisation, equipment and methods of fighting of Roman armies of the period. On top of this we have in Caesar’s commentaries a clear, in many ways short, but at any rate authoritative, account both of what happened and of what was in the general’s mind. Yet even with Caesar we are helpless the moment his account is incomplete or open to the suspicion of partiality. Where, for instance, was Caesar’s cavalry in the last stage of the battle of Pharsalus? Did they not rally after being beaten back at first and take part in the flank attack on Pompey, or was this really carried out as Caesar suggests by about eight cohorts of infantry only? If so this looks like another case of χωρίς ἲππεις. In particular we suffer from not having a similar record written by one of his opponents. Caesar’s estimates of their numbers, for instance, are much less convincing. It must be remembered, too, that we do not know enough of the method of fighting of Caesar’s army to reconstruct its battles in exact detail. We do not know, for instance, what was the depth of a cohort, whether there were gaps between cohorts, how the change was made from fighting densis to fighting laxatis ordinibus; and here again continuous controversy does not seem to be leading to a decision. Nor, I fear, is fresh evidence very likely to solve these problems of battles and tactics. It is very significant that in what is practically the only sphere of ancient history in which we are undoubtedly getting new military knowledge, as distinct from theory, namely, the Roman Empire, it is with regard to the organisation of the Roman Army and its frontier defences that our knowledge is enlarged. We do not get any nearer to solving the problems connected with particular battles (those at Bedriacum, for instance). In all branches of history I suppose we have a much better chance of reconstructing organisations and institutions than events and motives; but I hope I have shown that in the case of military history reconstructing the latter is quite exceptionally difficult and especially in the case of ancient history.

I now want to consider under separate headings the different methods employed by modern writers in their reconstructions. All, of course, start from the ancient texts, though they vary considerably in their attitude to these; but as the texts are unsatisfactory reconstruction can only be attempted with the use of certain Aids.

The first Aid is the study of geography and topography of the theatre of war and is employed by all modern critics, but especially by Grundy, Kromayer and many writers of specific articles (such as Sir William Ramsay’s investigation of the topography of Xerxes’ march through Asia Minor).3

Until someone with the necessary qualifications takes the trouble to do for an ancient campaign what Grundy has done for the Persian Wars and Kromayer for some of the

3 JHS xl (1920) 89 ff.
less known later wars, examination of the campaigns is more or less futile. We cannot even argue about them. Topography gives us negative evidence which is almost irresistible. If a particular move is rendered absolutely impossible by the nature of the country, that move (unless there has been an earthquake since) never took place. An examination of a scaled map of the Straits of Salamis shows decisively that the battle of Salamis never took place as Herodotus described it and with the number of ships which he states. The importance of the study of topography to the military historian cannot, therefore, be exaggerated. It is an essential study, but cannot give positive results of itself. Let me take a simple imaginary instance. Suppose that someone had in the distant future to reconstruct a battle fought, say, in the neighbourhood of Shotover, his information consisting of rather vague Herodotean accounts which gave a general but no full or very clear account of the movements, but mentioned certain definite features—The Brickworks, The Reservoir, Open Brasenoose, Blackbird Leys Farm—in connexion with these movements. By visiting the district the reconstructor might identify these sites rightly, but he would still be far from reconstructing the battle unless he also knew the numbers engaged, the orders issued, the formations adopted and the training manuals of the period. Only so could he discover among other things how much ground was covered, who was visible from where and when. I have fought many sham battles over that ground: the same topographical features were prominent in each, but the battles were entirely different. I have also given different bodies of cadets the same battle to fight—that is to say issued the same scheme and the same orders. But the resulting battles were never identical or even very similar, though precisely the same tactical features were there. Herodotus, in speaking of what is usually called the second position of the Greeks at Plataea, says that they were drawn up near the fount of Gargaphia and the precinct of the hero Androcrates. We will suppose that Herodotus is right in this. We will suppose, too, that some modern historian has rightly identified these spots (though there does not seem much prospect of general agreement). We are still far from knowing what position the Greeks took up, for even if we accept Herodotus’ estimate of the numbers we still do not know what formation the 38,000 Greek hoplites adopted. Supposing they were 8 deep: then they covered 2½ miles frontage; if 16 deep, only 1 ½ miles: if 4 deep, 5 miles. It makes all the difference, but we do not know. Where too were the 69,000 light-armed and attendants? Till we know that, and I do not think we ever shall, it is hopeless to try and trace on the ground the exact movements of the troops. It is impossible to say at exactly what stage of the battle the Greeks were hidden from the Persians by this or that hill. If the numbers for the two armies usually accepted in this country are even approximately correct the whole district must have been thick with troops; and it would be a clever staff officer who worked out orders for the supposed crossing of one another in the night (during the retirement from the second to the third position) by the Greek left and centre (16,000 and 20,000 strong respectively) which would not break every ordinary military rule of space and time. It is easy enough to draw little squares representing troops on the map, but it is extraordinarily difficult to make these squares correspond to the facts and realise how immensely they should increase in size the moment they are put in motion.

Topography, then, is an essential Aid to military history, but we want much more knowledge than it can supply by itself. It gives good negative results: to positive results it can contribute, but only in a limited degree.

The second Aid may be called the use of a priori deductions from modern works on Strategy. It is the method particularly followed by Henderson in his Civil War and Rebellion in the Roman Empire, in which the evidence of Tacitus is treated with suspicion and the campaigns are rewritten in accordance with excerpts from Von der Goltz’s Nation
In Arms and Hamley’s Operations of War. No other critic carries this method so far as Henderson, but it is very commonly employed. If the Persians, argues Munro, had intended to march on Athens from Marathon, they would have occupied the passes: they did not occupy the passes, therefore they did not intend to march on Athens. Custance employs this method constantly and every critic inevitably uses it to a certain extent. But it is a method which can justly be used only with the greatest caution, for it tends to make two false assumptions: first, that generals never make mistakes, whereas it is notorious that success in war consists in making rather fewer mistakes than the man on the other side; second, that there are certain great military principles which have been fully understood in all ages. Now it may be true that with a number of qualifications and if allowance is made for changes in armour, the introduction of gunpowder, etc., certain strategical principles are always true in the sense that their employment on suitable occasions always makes for success. Napoleon certainly found it helpful to study Caesar’s Commentaries. But it does not follow that generals have always been guided by these principles even today when they have Napoleons as their examples, a vast literature of military science, maps, an intelligence service, trained war staffs, and, above all, when they lead trained and disciplined armies. Much more was this the case in the Greek world before Alexander and Hannibal and Caesar had discovered the art of war and when untrained generals led half-trained troops. I doubt whether Napoleon himself could have been clever with a fifth-century Greek army unless he were given opportunity to train it—certainly not if his own experience of leading large armies was as small as that of, say, Pausanias, and if he had Homer’s Iliad as his Field Service Regulations. Yet modern writers take up modern books on strategy and rewrite ancient wars in the light of them. The result is magnificent, but it is not ancient war.

Only three things seem to be universally true of all armies:

(1) That a man takes up a certain amount of room and that therefore a large army, especially on a narrow road, takes up a great deal of room. (Xerxes’ army, for instance: compare what I have said above about Plataea.) Henderson seems to me to leave this out of account altogether in his reconstruction of the movements before the first battle of Bedriacum. From Herodotus downwards many writers about ancient wars have treated armies on the march as if they were flags stuck in with pins on a Daily Telegraph war map.

(2) That a man takes time to move and that with a long column when the head halts the rear takes a long time to come up with it (and yet someone is always surprised if an army on the march delays at all before delivering battle).

(3) That a man has a stomach which must periodically be supplied with food.

I am almost inclined to add, though they are not of quite such universal truth:

(4) That generals make mistakes and do idiotic and irrational things, and

(5) That large bodies of troops are awkward things to handle, and when in contact with the enemy always tend to settle a fight in their own way.

As a note to my discussion of this Aid, I should perhaps just mention the drawing of analogies from modern battles. This again is not only irresistible, but may be of considerable help. But it must be used with great caution, for no two military situations ever are alike. Let me take just one example. Casson has compared the action of the Persians at Marathon with that of Von Kluck in the great German sweep of 1914. So far as this is a reminder that generals make blunders the quotation seems to me admirable;

\[4\] JHS xl (1920) 44.
but when he states that the Battle of the Marne is the modern counterpart of Marathon and argues from the supposed parallel action of General Manouri that the Greeks were deployed at the foot of Mount Agriliki, he is, in my opinion, on very dangerous ground. The analogy of modern battles is good argument for the possibility of certain general occurrences in certain types of situation in battle, such as delay, irresolution, over-confidence, etc. It will never prove reconstructive detail.

The third Aid—which no doubt overlaps the second—may be called Sachkritik, the attempt to reconstruct in accordance with die Realität der Dinge. It is the favourite German method. Like the last it is an Aid which must be used—I am trying to use it myself in this paper—but is very constantly misused. On the whole I think we may say of Sachkritik as of Topography that it is much more valuable as negative than as positive evidence. To take a stock instance. Delbrück shows that if Xerxes' army really numbered the five millions credited to it by Herodotus and if, as in many places the geography requires, it marched along one road in a narrow column, even if it had much less baggage and as good march discipline as a modern army, then just about when the Advanced Guard reached Thermopylae, the Rear Guard was leaving Sardis. This is a very good way of proving that Xerxes' army was anyhow much smaller than Herodotus states. Hauvette's reply that the army marched in great squares can easily be destroyed by further employment of Sachkritik. A literal interpretation of the mile run at Marathon may I think be similarly disproved in spite of what Hauvette declares himself to have witnessed in the case of French soldiers. But the moment anyone tries to get positive results from Sachkritik the result is much less convincing. Take, for instance, Delbrück's method of fixing the size of the Persian army at Plataea. If the Persians had considerably outnumbered the Greeks at Plataea Mardonius would have detached a turning force. He did not do so (according to Delbrück). Therefore the Persians did not outnumber the Greeks. As Delbrück by rather similar methods has fixed the strength of the Greeks at 20,000 hoplites and 40,000 light-armed, he concludes that therefore the Persians had altogether 60,000–70,000 men.

One of the chief difficulties seems to be that it is hard to get agreement as to exactly what die Realität der Dinge is. Delbrück is very anxious, for reasons which I need not go into here, to prove that in the Macedonian phalanx which met the Romans at Pydna and elsewhere each man was allowed a frontage of only 1½ feet. To support this argument he apparently borrowed the long spears in the Zeughaus in Berlin, armed his seminar with imitations of them and found that they could work quite well as a phalanx with only 1½ feet to each man and claims to have proved his point. But Veith, who is a real Hauptmann, says that this is nonsense. He has drilled a lot and his whole military experience makes him certain that a man must have more room in the ranks than 1½ feet. Again Delbrück wants to prove that the gaps usually supposed to have existed between maniples in the Roman army before the time of Marius were not really there at all. To do this he has to ridicule the well-known statement of Polybius that at Zama, because of Hannibal's elephants, Scipio placed the maniples of the principes behind the maniples and not as usual behind the gaps of the hastati (i.e. so as to allow the elephants an attractive avenue to bolt along when they became excited). Delbrück thinks the whole idea mere imagination. Why make avenues for elephants? Elephants, even infuriated ones, could

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5 Die Perserkriege und die Burgunderkriege, 138.
6 Hérodot, 311–12.
7 Ibid., 261.
9 Geschichte der Kriegskunst, 433.
10 E.g. in Kromayer-Veith, Heerswesen u. Kriegskunst, 358.
12 xv 9. 7.
perfectly well zigzag. He does not, unfortunately, seem to have borrowed elephants from the Zoologischen Garten in order to demonstrate this point with his seminar, but quite apart from the far-fetched nature of the argument, the inherent weakness of it is that even if Delbruck is right about the habits of elephants, Scipio may not have been equally well-informed, or if well-informed, may have been unwilling to take the risk.

I could continue this sort of thing indefinitely, but I hope I have said enough to show that the moment Sachkritik is used to give positive constructive results it is very easily abused.

Note to Aid 3 on Numbers. I am not in this paper going very fully into the problem of the numbers of ancient armies, immensely important though it is. The details of a campaign, for instance, cannot be reconstructed without fairly exact knowledge of numbers or at least of the number of units (legions, for instance) present on both sides. The exact number present with each legion, so long as we know it approximately, is not quite so important. For the general understanding of a campaign exact detail as to numbers is not quite so essential, though here too we must have approximate knowledge and above all we must know the relative strength of the armies engaged. Three methods are available for dealing with numbers:

(1) To accept the numbers given to us by the ancient authorities.

(2) To argue from probabilities and possibilities.

(3) To deduce the numbers from what we know of command and organisation.

With regard to these:

(1) No one accepts all the figures given by the ancient authorities. Often they are incredible (Herodotus' numbers for Xerxes' army, for instance), and often the authorities contradict each other or themselves. Many modern authors, however, select a number here and there which suits their theories, but there is considerable variety of choice. Many suspect round numbers but jump at exact figures such as 1,207 or 53. Others (Tarn for instance) are more inclined to accept the round numbers because they look like evidence of a definite organisation. Beloch rejects both: round ones because they are round: odd ones because if you make a judicious selection and add them together they become round. Numbers of troops based on the calculations of participants in battles, even generals, are so notoriously unreliable that I think it impossible to put much faith in the numbers given by Greek historians until we have evidence that scientific methods of counting were employed.

(2) The argument from possibilities and probabilities helps, as I have already argued, to reduce absurdly big numbers. It does not help so much to fix exact numbers. We cannot deduce the numbers from the tactics employed or from the casualty list (even if the latter is accurate). Munro's suggestion\(^\text{13}\) that 20,000 Persians fought at Marathon on the ground that 6,400 were slain and the Persian centre wiped out and that \(6,400 \times 3 = \text{about} 20,000\) is to my mind unacceptable as an argument even if the conclusion be approximately correct. A centre is not very likely to be exactly one-third of an army. It is a rough tactical not an exact mathematical division, nor is it at all likely that the centre was really exterminated or that the wings got off scot free, or that the survivors of the centre were equal in number to the killed of the wings. Still, Munro does not urge that argument at all strongly.

\(^{13}\) JHS xix (1899) 189 n. 1.
The argument from the analogy of what we know of the population of Greek states later in the fifth century is of some help with regard to Greek armies in the Persian wars, but every link in such chains of argument is weak and the conclusions arrived at cannot really be so positive as Beloch would have them.

(3) The argument from command and organisation is more promising at any rate with armies which, like the Persian, appear to have had a thorough organisation. Nothing is more attractive in the writings about the Persian Wars than Munro’s discussion of the organisation and strength of the Persian army and Tarn’s corresponding treatment of the navy. But even if the recurrence of certain round numbers such as 10,000 and 60,000 does point to some real bit of Persian military organisation, it is still possible to argue for 300,000, 180,000 and even 60,000 as the strength of Xerxes’ army. Even if new evidence from Asia teaches us more about the Persian army we shall still probably remain uncertain as to what units of this army came to Greece and how far these units were up to establishment. There is enough evidence to justify ingenuity: not enough to hold out much prospect of certainty.

The Fourth Aid is what I think I may call the Sherlock Holmes method. This again is used inevitably by all historians, especially in this age of Quellenkritik, but its own particular master is Munro. Reading his article on Marathon leaves me with just the same feeling as reading Conan Doyle. It is so attractive and such an artistic whole that it seems almost a crime to take it prosaically to pieces and inquire whether the steps in the first argument do follow one another so irresistibly as at first appears.

This Aid consists in a combined use of the three Aids I have previously mentioned together with an ingenious selection of statements from ancient authors of different periods and a subtle interpretation of them. Here I am touching on a very big question which affects a great deal of the modern interpretation of, at any rate, Greek history and I want if possible to avoid raising that question in too general a form tonight, but ancient history does lend itself particularly to this kind of treatment. The subject, we have already seen, is one in which we often know little of facts and still less of motives, and thus a wide field is open for speculation. The evidence is very incomplete. There are obvious difficulties and gaps and the temptation to try to solve these difficulties and fill in the gaps is very great. It goes against the grain to admit that we cannot find out with greater certainty. The ancient accounts (that of Herodotus, for instance) are often tinged with a supernatural colouring and do not appear to be the work of military experts. This gives many opportunities for accepting one statement and rejecting another so as to suit a particular theory; also for inventing motives and strategical designs to explain the selection of facts which one has made from the unmilitary ancient historians. Furthermore, the ancient tradition of the Persian Wars grew and grew until by the early years of the Christian era it had incorporated many details which are absolutely contradictory to the statements of our ancient historians. I do not see any prospect of the most exhaustive Quellenkritik ever really deciding which of these details are late additions and which really go back to Ephorus or, even if it could, of deciding for Ephorus against Herodotus. It seems so clear that the Sherlock Holmes method began to be employed in a mild way very early in Greek historiography and the chances that the earliest account of a Greek war that we possess is the best seem to me very great indeed. But the modern method is so often to accept as sound any element in the later stuff which suits a particular theory (for instance, Nepos on the Athenian defensive arrangements at Marathon, Suidas on the Persian cavalry...
or Diodorus on Salamis) and to reject the rest as valueless. I cannot help thinking that a sounder attitude is that declared by Tarn to be his,\textsuperscript{16} that we cannot make much use of this late evidence, though it is at least interesting when we find that our own conclusions agree with it.

Lastly, the wars I am chiefly alluding to tonight belong to a period of history which every classical scholar studies and on whose interpretation there is, here in Oxford in particular, great concentration of energy. The evidence being what it is this has of itself, I think, made for rather excessive ingenuity. Each new writer or teacher tries to screw one little bit more out of it. Compare the very elaborate causes which are now usually alleged for the Peloponnesian War and contrast any ancient war which has not attracted many historians, where it will be found that people are still allowed to go to war for quite simple reasons and to fight in quite simple ways. I am going to deal with Marathon in a few minutes, but a few points about Marathon here will illustrate my point. Munro’s theory argues from, among other things, a supposed silence of Herodotus on one point, a rather strained interpretation of one phrase in Herodotus (about the Persians being in their ships when the shield-signal was made) which involves supposing that Herodotus has got the phrase from someone else and has surrounded it in his account with statements which are false, from an explanation in Suidas of the proverb χωρίς ἔπτεις and from a statement in Nepos (though this is not urged strongly) that 100,000 Persian infantry were present at the battle, when he has previously mentioned 200,000 as the total Persian infantry strength—from these and other similar points it is argued that half the Persian infantry and all the Persian cavalry were on ship-board during the battle. (Incidentally Nepos says that all the cavalry were present at the battle and Suidas never says that they were on ships.) A comparatively slight supposed silence of Herodotus is thus employed as an argument in favour of a whole theory of the battle about which Herodotus is entirely silent and which in my opinion involves shutting up Herodotus as absolutely valueless at any rate on this battle. Yet How,\textsuperscript{17} in replying to Casson, argues for Munro’s theory and maintains that in so doing he is arguing for the authority of Herodotus. It seems to me that it would be at least equally legitimate to argue back again from the silence of Herodotus as to the cavalry being in the ships and from Nepos’ statement that the cavalry were present, to a battle very like that which one would naturally deduce from Herodotus’ narrative. It is merely a question as to which of many flatly contradictory bits of late evidence you select. But more about Marathon later.

\textit{Note to Aid 4.} Another rather popular method of dealing with the ancient evidence is to cling to one main ancient authority (Herodotus, for instance) and to try and get some fixed rule for his interpretation. The rule most commonly adopted is to suppose that Herodotus heard the views of soldiers who took part in the battle, but not the views of officers: that the facts in Herodotus are, therefore, more or less right, but that the interpretations put upon them are wrong or distorted. Henderson takes much the same view of Tacitus and Cunstace of Xenophon. With regard to this matter I would urge:

(1) That to my mind fixed rules of this kind cannot be applied at any rate to Herodotus, who is good and bad on no fixed system. It is part of his charm.

(2) Even if it were correct it would not follow that we have got the facts right. I have already tried to show that a private soldier’s imagination soon gives him a very false picture even of that part of a battle in which he was personally engaged and also he certainly applies what was only local to the whole fight. A much more

\textsuperscript{16} \textit{Ibid.} 203, but cf. 232.

\textsuperscript{17} \textit{JHS} xxxix (1919) 48 ff.
critical investigator than Herodotus would be required to find out even the facts. Details of distance and the like would be particularly unreliable.

(3) The attempt to work out the theory in detail leads to what seem to me rather strained suppositions.

(4) I find it hard to believe that even in democratic Athens Herodotus, the friend of Pericles, should have got to know only the families of privates and never those of officers. This is at least equally true of Xenophon and more so of Tacitus or his sources.

I think that camp gossip did probably lead to many of the absurdities which have crept into our accounts, but recognition of the fact will only help us part of the way towards reconstruction.

*The Fifth* and last *Aid* consists in making the most thorough study from all sources of the armies engaged, their strategy and tactics, their weapons and method of using them, their system of recruiting and organisation, their officers and staff. This, when taken in conjunction with a judicious use of the other Aids I have mentioned, seems to be so extremely helpful, if only as a guide to the understanding of ancient authors, that it is surprising that it has not been more consistently employed. In the case of the Persian Wars, Macan makes frequent allusions to Greek military peculiarities, but his book contains no full consideration of the subject. He never, for instance, to the best of my knowledge, emphasises the difference between a Greek and modern army in the matter of a staff. Grundy draws constant attention to the difference between Greek and Persian methods of fighting, but his book on the Persian Wars does not, like his book on Thucydides, review the whole subject in full detail. Tarn, in his article on the Fleet of Xerxes, does base many of his conclusions, which are, however, rather far-fetched, on a thorough study of ancient naval warfare. But to the best of my knowledge Delbruck is the only historian of the Persian Wars who employs what seems to me to be the correct method of starting off with a study of the Greek and Persian armies. He is a historian of wars throughout the ages and has thus realised that in any age you must understand the armies before you can understand the wars. Unfortunately he is not a reliable Greek historian and his excellent method is spoiled: (1) By not being thorough enough. He omits an adequate treatment of the higher command of Greek armies and seems to assume that somewhere at the top is a German war-lord with a trained staff. (2) By misinterpreting the evidence about the Greek and Persian armies when, for instance, he says that the Greeks had no light-armed combatants and the Persian army was a small army of picked troops with no general levy. (3) By the blatant misuse of Sachkritik which I have already illustrated.

But on the whole modern writers have not made a sufficient use of what we can learn, little though that is, from the study of ancient armies. Occasionally a reference is made and occasionally a parallel quoted. But often, I think, further investigation will show that the parallel is not a real one, or that there are other instances which point to a different conclusion. I will illustrate this when I discuss the inferences usually drawn with regard to Marathon from the behaviour of the Persian cavalry at Plataea.

The armies of different countries differ as much as the constitutions. No one would endeavour to interpret the political history of the age of the Gracchi without making a thorough study of the Roman constitution. I suggest that a study of ancient armies is equally necessary for an understanding of ancient military history.

In discussing these five Aids I have tried to give some idea of what is in my opinion the legitimate and the illegitimate use of them. I am afraid my discussion has consisted mainly of a scattered collection of remarks, but would it not be possible for some better
qualified historian to propound principles of criticism whose general acceptance might act as a steadying influence in military reconstruction? Have we not been rather carried off our feet by the great flood of research? Where is it taking us? The propounding of these questions is really the chief object of this paper. But I feel that in order to justify many of the criticisms I have suggested I must give rather more illustration than I have done. So I propose, in conclusion, without attempting to go into all the problems raised by the battle of Marathon, to offer a few reflections on one or two of the salient points in modern reconstructions of that battle.

I select Marathon for several reasons. Not because I agree with Casson's statement\(^{18}\) that 'Marathon, perhaps the most important battle in antiquity, is the least accurately described'. The importance of Marathon seems in many ways to have been exaggerated by most ancient writers except Herodotus, and even Herodotus shares in the exaggeration in Book ix, Chapter 27. It certainly was not one of the decisive battles of the world. It decided nothing, for the Persians came again in ten years. Certainly it illustrated, possibly for the first time, the superiority of the hoplite in close order to the skirmishing Persian; but not in a way which the Persians accepted as decisive. As for accurate description, one has, I think, only to wade through half a dozen of the campaigns that are known to us only through Diodorus, Plutarch and others, to realise that it is just because we do know quite a lot about Marathon that we are all so desperately anxious to know more. My reasons are rather:

(1) It is the first battle in Greek history that lends itself to criticism.

(2) The whole campaign is so small that the main problems stand out clearly.

(3) (My chief reason) It is the battle of all others in which at any rate in this country what are mere theories are tending to be regarded as established truths. Practically every English writer from Macan and Bury onwards has accepted the theory that during the battle the Persian cavalry were on shipboard; practically every English writer since Munro has accepted his suggestion that half the infantry was on shipboard as well and that the landing at Marathon was a mere feint to lure the Athenians out. Grundy agrees with Munro on most points. How and Wells almost entirely; How, in the \(^{19}\) JHS, speaks of 'Theories already well known and in England at least widely accepted'. Caspari, in an earlier number of the \(^{20}\) JHS, says, 'Why did the Persians offer battle at all in an unfavourable position and why did their cavalry take no part in the action? Since no adequate answer has been given to these questions, the presumption is in favour of the alternative theory which has been adopted by the leading English historians.'

So strong is the tendency to regard the embarkation of the cavalry as established that Casson in his earlier article\(^{21}\) while arguing for the acceptance of Nepos' view of the battle, which states that the cavalry were present and that the Athenians constructed special works (of a suspiciously Roman type, by the way) to render them ineffective, still accepted the fact that the cavalry were on board. 'Undoubtedly the Persian cavalry was re-embarked, not so much for its supposed utility on the plains of Phalerum, but because of the discovery of the deceptive nature of the place which had been specially selected for its manoeuvres.' In his later article,\(^{22}\) logically, I think, he modifies this view. 'The presence or otherwise of the cavalry is of less importance if the battle proves to be the

\(^{18}\) JHS xl (1920) 43.
\(^{19}\) xxxix (1919) 48.
\(^{20}\) xxxi (1911) 104.
\(^{21}\) Klio xiv (1915) 69 ff.
\(^{22}\) JHS xl (1920) 44.
counterpart of the Marne.' I do not think it will prove to be that; but it is at least comforting to find Casson, the first Englishman for thirty years or so, hesitating to accept the embarkation of the cavalry.

First, then, how far are we well equipped with evidence with regard to Marathon? Herodotus' account contains a number of difficulties, some, I think, less serious than is often made out; but still, difficulties are there. They are not sufficient to prevent a general understanding of the battle, but they are sufficient to prevent a detailed reconstruction unless we have any other really good evidence. Unfortunately we have very little. The other literary sources either help us very little, such as Pausanias, or are of indisputably doubtful worth, such as Nepos and Suidas. The topography of the district is well known and assists a general understanding of the campaign; but although it limits the possible number of detailed reconstructions of the battle I do not see how it is ever going to decide definitely for any one. The Soros is a great asset, but though I entirely agree with those who argue that it probably marks the actual battleground, that of itself only rules out a certain number of hypotheses.

Archaeology has done a great work in proving the antiquity of the Soros, and it is quite conceivable that there is still something to discover by excavation—the site of the Greek and Persian camps, for instance. This would rule out still more hypotheses, but it would probably start a number of new ones. With regard to the numbers we can, I think, regard 10,000 Athenians and Plataeans as right within a few thousand. But of the Persian numbers we are in complete ignorance. The fact that they came in ancient ships rules out the vast numbers of post-Herodotean writers, but whether they were equal to the Greeks, twice their number, or what, is merely a matter for conjecture.

I think, then, that it is reasonable to start an inquiry into Marathon without expecting either to solve all the problems raised by Herodotus' account or to establish much certainty on points of detail. I believe that if we do this we shall get nearer to the truth than we are likely to if we expect to explain everything, especially if we can put Marathon definitely in its proper place in the history of warfare. That involves seeing what use can be made of the fifth and last of the Aids I mentioned above, but unfortunately it is not possible within the limits of this paper to embark on a full discussion of Greek and Persian armies and their methods of conducting war. I must be content with a very few short extracts from what was originally a much fuller treatment.

The Persian Army. There is not time to discuss organisation, strategy and policy, but I must say a few words about tactics. How was the Persian army armed? Herodotus' description of the dress and armour of the different nations is almost certainly incomplete (unless a great many of them were very naked) and is probably not altogether accurate, but certain prominent features stand out which there is no reason to dispute:

(1) There were great varieties of armour and therefore varieties in method of fighting.

(2) There is (except in the case of the Assyrians and a few others) a marked absence of strong defensive armour. Therefore the infantry was very vulnerable at close quarters.

(3) There is a marked predominance of distance weapons (the bow, javelin, etc.) which confirms the impression that the natural role of the Persian infantryman was not hand-to-hand fighting, and this is borne out by what we know of their tactics in the wars with the Greeks. The Persian (whether mounted or on foot) hustled the Greek with arrows; at close quarters he was no match for the Greek hoplite. At Thermopylae, Plataea and Mykale this was clearly the case, and what we know of Marathon bears this out, although the army of Datis and Artaphernes was very likely
a picked force. What formation the Persians employed we do not know, but probably it was much more open and less symmetrical than the Greek. Both at Plataea and Mykale we hear of Persians fighting gallantly in small groups.

The Persian cavalry was obviously an important arm of the service. Its tactics seem to have been like those of the infantry and it made great use of the bow. It did not employ shock tactics, that is to say it did not close with unbroken infantry. (The action of the Theban cavalry at Plataea\textsuperscript{23} is an exception which proves the rule. It was Greek cavalry and its victims were advancing in disorder.) The ordinary method was to ride up close to the infantry and shoot, then they wheeled and went back to prepare for another advance. (Masistius at Plataea.) Even in Asia against inferior infantry the Persians did not employ cavalry shock tactics until the opposing infantry was much broken.\textsuperscript{24}

With regard to Persian ships I need say very little here, but one can safely say three things: (1) That the number of horses carried in any fleet of the period cannot have been large. The facts, so far as I know them, of all ancient seagoing expeditions support this. (2) Ancient transports were uncomfortable, crowded and dangerous. They were merely a means of crossing water when this could not be avoided. Their first object was to get to land as soon as possible. (3) Though I do not know how ancient horses were embarked and disembarked, it was, if modern analogies are worth anything, a troublesome business not undertaken more often than absolutely necessary.

The Greeks. Here again I can only mention a few points. Most of our evidence comes from a later period. It is not impossible that in 490 the Athenian Army had better organisation and training than sixty years later, but it is unlikely. The discipline quite possibly was better. No Greek army of the fifth century, except Sparta’s and possibly Boeotia’s, has left to us these little glimpses of organisation which, I believe, always appear in the military records of states like Sparta, Rome and even Persia which have a proper system of discipline and subordinate command. Armies which have not got this are likely to rely on an orthodox type of tactics and on the use of men in bulk more than on individual trustworthiness. The Greek phalanx was a suitable formation for such an army, and there can be no doubt from what Herodotus says both of Marathon and Plataea that the Greeks already employed the phalanx (though we do not know its depth) whose members were armed as hoplites, that is to say their defensive armour was very strong and their offensive armour very strong at close quarters. But until he got to close quarters the hoplite could do nothing. He had not even the pilum of the Roman legionary. The hoplite phalanx on suitable ground at close quarters was a most formidable army. Its weaknesses were:

(1) Its clumsiness. Everything depended on the whole phalanx being properly drawn up. It was quite unsuited for sudden surprises. That is why when Greek meets Greek there is no hurry. Each side let the other’s phalanx draw up in peace and it took two to make a fight.

(2) Its inadaptability. It held no reserves. Nothing was left to subordinates. It could only fight one type of battle.

(3) Its liability to confusion. So much depended on the unbroken line. It could not seek out an enemy: only a battlefield. Nor could it attempt subtleties. For much the same reasons it could not pursue.

The Greeks who fought Persia employed no cavalry at all. Light-armed troops were

\textsuperscript{23} Hdt. ix 69. 2. \textsuperscript{24} Hdt. vi 29. 1.
doubtless present on all occasions, but until the Peloponnesian War light-armed troops seem to have been almost untrained and to have played little part in the battle.

*The Greeks and Persians from the point of view of one another.* What did the Persian know and expect of the Greek? He had met and conquered the Greek of Asia Minor, who had, however, put up a good fight. How far the Persian had actual experience of Greek infantry tactics we do not know; we do not, I think, even know whether the Ionians employed the hoplite phalanx. They probably did; but, as Grundy points out, the Ionian revolters can have had no military training for some thirty years or more; their phalanx would therefore be out of practice and easily thrown into confusion. This would give more opportunity to the Persian cavalry and may have led to an underestimate of Greek infantry. But what could the Persian do when he met a trained phalanx? If it encamped in the open his cavalry could ride up and shoot arrows at it, as in the second position at Plataea. His infantry could do the same, as against the Spartans at Plataea. But the enemy the Persians were used to were in the habit of fighting in this way too. It is easy to imagine that the Persians were puzzled when at Marathon, the first engagement, the Athenians advanced to close grips at once. (That, I think, was what surprised the Persians: not a mile run literally interpreted but the fact of closing without the preliminary bow and arrow business.) How were the Persians to deal with this? The cavalry which could hustle the Greeks at rest seems to have been quite ineffective against the phalanx in action at close quarters (in the final stage of the battle of Plataea, for instance). Mardonius’ curious treatment of the Phocians might even be fancifully interpreted as an attempt to find out what would happen if he charged unbroken hoplites with his cavalry. “The Persian horse having encircled the Phocians charged towards them as if about to deal out death with bows and arrows ready to let fly, nay here and there some did even discharge their weapons. But the Phocians stood firm, keeping close together and serrying their ranks as much as possible; whereupon the horse suddenly wheeled round and rode off.”

On the other hand, what would the Greeks think of the Persians? No doubt they were frightened of them, but Athenians in small numbers had dared to go to Sardis; the Ionians had held out for a long time, and Miltiades at any rate knew how the Persians fought. They had every reason to feel fairly confident about their prospects when they got to grips: the problem was how to get there. For, in the open, Persian arrows, if not very destructive, were at least most unpleasant.

I once umpired a Field Day on Roehampton Common between the Kensington Cadets armed with carbines and the Westbourne Boy Scouts who relied on the use of the staff at close quarters. I thought of the Persian Wars, for the real interest in the detail of the fighting in these wars is that the two sides fought in quite different ways. It was the spear versus the bow, as Aeschylus says. How could the Greeks get to close quarter without heavy preliminary loss? No wonder that at Marathon, Thermopylae and Plataea there were delays before the fights.

One last point. In all ancient warfare the Intelligence service seems to have been bad (in the preliminaries to the battle of Issus for instance). I think the Persians and the Greeks were probably much more in the dark about each other’s movements than is often supposed.

From this point of view let us approach the modern English theories of Marathon; and I will first deal with the two points common to all the theories and go on to deal with a few of those raised more particularly by Munro. The two points are: (1) Where was the Persian cavalry during the fight? (2) Why was there delay and why did the battle finally take place?
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(1) The Persian cavalry. It is argued that the cavalry were embarked before the battle. The only evidence put forward for this is (i) the absence in Herodotus of any reference to the cavalry during the battle, during the Greek pursuit and in the casualty list. (ii) Suidas' explanation of the proverb χωπίς ἵππεις. Other evidence has been put forward in support of some division of the force, but no other for the specific inclusion of the cavalry in the separated portion.

First, I should like to repeat that, though it is not necessary to suppose, as Beloch very typically does, that only horses sufficient for officers and orderlies can have been brought over, the evidence of other naval expeditions is in favour of the cavalry being few in number and small in proportion to the infantry. Also that simply on grounds of general probability, which I admit are not final, nothing is in itself more unlikely or more absurd from a military point of view than a re-embarkation of the cavalry. Incidentally they were the only troops who would have had any chance of stopping the Athenians returning to Athens. Infantry cannot act as a retaining force to infantry unless they block the line of retirement.

Now as to the silence of Herodotus. It is argued that as at the battle of Plataea Herodotus tells us that Persian cavalry checked the Greeks' pursuit we should have heard the same at Marathon if they had been there. At Plataea the Persians were retreating to a camp which cavalry would enter last: at Marathon to the ships which, owing to difficulties of embarking, they must obviously reach ahead of the infantry if they were to get on at all. I do not think I need say more. As to the absence of all reference to horsemen and horses in the casualty list, this is very regrettable; but if it means that no cavalry took part in the battle we must, to be logical, argue that in practically all ancient fights the cavalry were removed before battle. There is, for instance, no such reference in Herodotus' much fuller account of the battle of Plataea. The argument proves nothing at all. So we pass to the more serious argument from the supposed absence of reference to the cavalry in the actual battle. I say 'supposed' because Herodotus seems to me most clearly to imply that they were there, not so much in his remarks about the Persian reasons for landing at Marathon as when, in one of those passages which read so very much as if they came from a Persian source—possibly a Persian prisoner—he says how surprised they were to see a mere handful of men coming on without horsemen or archers. If I read in an account of a golf match that Braid on coming to the first tee expressed surprise that Taylor's bag did not contain a niblick, I should be justified in inferring that Braid had a niblick himself. I have always inferred from Herodotus' account that the cavalry were there with the infantry. But what did they do? I imagine that they were at the very least reduced to the value of Persian infantry by the rapid closing of the Athenian phalanx. This is entirely supported by Herodotus' account of the battle of Plataea, which is almost universally quoted to prove the opposite. At Plataea so long as the Athenians are at rest on the hills the Persian cavalry harass them, but do not close and are driven off; when the Greeks advance and encamp in the open the Persian cavalry makes itself very unpleasant, but again without closing. When the Greeks retire the Persian cavalry hustle them, but the moment the Spartans and Tegeans take the offensive and close with the Persians the cavalry disappears from the picture as completely as at Marathon until it as suddenly reappears during the pursuit, and that in spite of the fact that Herodotus' account of Plataea is much fuller in its detail and the Persian cavalry were much more numerous. Mardonius, we are told, was on a white horse; otherwise the one bit of Plataea which resembles Marathon is described by Herodotus with a precisely similar silence about horses and horsemen. Herodotus does mention (ix. 63) λογίδας τοὺς ἀριστοὺς χίλιους as fighting especially gallantly with Mardonius. These

26 Grieschische Geschichte, ii² 2, 80–1.
may be the 1,000 picked cavalry. Herodotus expressly says that they were on the
defensive and resisted attacks, i.e. they did not do what everyone says the Persian cavalry
must have done at Marathon if it had been there, namely, attack in flank or rear. The
Greeks had no cavalry at all in either case. Clearly Persian cavalry did not dare to close
with an unbroken phalanx or dared and failed. Herodotus, who is writing of things that
happened and seldom mentions things which did not happen, is quite consistent in saying
nothing about them.

But what about Suidas? It seems to be generally agreed that he lived about A.D. 1000
—considerably closer to Macan and Munro than to Marathon. Great as is his apparent
honesty, is it really conceivable that a version of Marathon so entirely different from that
of Herodotus can have been a genuine version which blushed unseen not only by Nepos,
Trogus, Pausanias and their sources, but also by the well-read author of the de malignitate
Herodoti who is at special pains to find fault with Herodotus' account of Marathon? That
there was a proverb χωπίς ἵππείς is of course probable. But it is a not very likely
or is at any rate a very pointless proverb for fifth-century Greece, in whose states cavalry
existed not at all or in very small numbers and was quite ineffective in battle. On the
other hand, it was a very natural proverb in the years after the battle of Adrianople,
when cavalry came into its own. But once the proverb existed its attribution to Marathon
was not unnatural. Marathon became the ancient battle, an earlier Hastings; and any
intelligent reader of Herodotus' account in the centuries following A.D. 378 would, judging
by the warfare of his own day, be surprised at the apparent inactivity of the cavalry, just
as we are today until we study the warfare of the period. But, argues Macan, the cir-
cumstantial detail in Suidas goes to prove the genuineness of his statement. I cannot
help feeling that the circumstantial detail, though it may support the origin of the proverb
in some definite incident, is only one more argument against its having any connection
with Marathon. Ionians, says Suidas—and it is surprising to find Ionians in a picked
force so soon after the revolt; still, they might have been sailors—Ionians climbed up
into trees and signalled to the Athenians that the cavalry were away. Now to me this
is merely incredible. Did they use Morse or Semaphore? In the Peloponnesian War
there is a good deal of evidence for the use of simple signals, probably of a pre-arranged
type. But this implies a definite alphabet. That the Athenians and Ionians had such
a thing which they could mutually understand and which could be employed from trees
and read at a distance before the discovery of telescopes is more than I can swallow. And
why should it be necessary? The embarkation of horses is not only a noisy business, but
must have been considerably more visible at any rate than an Ionian up a tree. I mention
all this not very seriously but simply to show what the accepting of Suidas involves, especially
when he is further strained to support modern theories. For Suidas says nothing about
ships; at least I can find nothing though I read in one recent work, ‘The fact that the
cavalry were embarked (Suidas) is the strongest evidence that the move on Athens was
to be made by sea’.

If Suidas supported anything at all it would be the absence of the cavalry foraging
or its employment on some such tactical turning movement as that of Hydarnes at
Thermopylae. But there really is no evidence at all in favour of this embarking of the
cavalry. It is in itself most unlikely, and I cannot see any serious difficulty in supposing
that the cavalry was present at the battle.

(2) The second problem—the delay before the battle and the reasons for its finally
taking place—cannot be dealt with so precisely, because there is not the same agreement
among modern writers. All seem to see a need for some ingenious explanation of the
battle, but some (Grundy, for instance) are not worried by the delay. I will therefore
deal with the delay very shortly. If the Greeks were going to attack, it is asked, why
delay when there was fear of treachery? If the Persians were going to attack, why delay
when there was a chance of Spartan reinforcements to the enemy? I can only reply that there has been delay before half the battles in history. Opposing armies are always extremely unlikely to delay while they make preparations (disembark stores, for instance), conduct inquiries or hope that the other side will make a mistake. Delay is especially likely if, as probably at Marathon, the smaller army is the more strongly posted. Compare the delays at Thermopylae and Plataea which have also caused what seems to me unnecessary worry. Here at Marathon was the added reason that one side expected reinforcements and the other treachery (for that like so many Sherlock Holmes arguments can be used both ways). The two forces were differently armed and had no experience of one another. The warfare of the period made a battle unlikely until both sides wanted it. Compare the numerous occasions in ancient history in which one army ‘offers’ battle: for instance Caesar’s in the days preceding Pharsalus. More modern parallels are equally numerous, but I will only mention one. The manoeuvres of Cromwell and Leslie which culminated in the battle of Dunbar afford a parallel with Marathon which superficially (compared for instance with the battle of the Marne), is quite extraordinarily close. I do not want to deduce any reconstructive detail from the parallel: there were of course great differences. But it is legitimate to use it as an illustration that delay in war is quite usual.

What then led to a fight? Two opposing armies constantly delay. On the other hand there is almost sure to be a fight before long. A situation such as that at Marathon produces a gradually increasing tendency to collision. Hunger, cold, ambition, mere impatience, and above all misjudgement—all have their influence. There is very likely no sufficient military reason. The history of war is very largely the history of generals selecting the wrong moment for attack. Why did Pompey finally accept battle at Pharsalus? Why did Leslie finally come down into the plain at Dunbar? The latter, at any rate, is usually recognised to have made a blunder; and Cromwell’s ‘The Lord has delivered them into our hands’ is a parallel to the Persian ‘Look at these fools coming on without cavalry and archers’, with just this difference—that Cromwell was right and the Persians wrong.

It also seems to me that the expression ‘taking the offensive’ is often used rather wildly. It is often hard to say which side did take the offensive. At Pharsalus Caesar was trying to bring on a battle for some days, but the actual day was fixed by Pompey drawing up his army for battle. Then in the battle itself the tactical offensive was taken by Caesar. At Dunbar the strategical offensive was taken by Leslie when he came down to the plain, but the tactical offensive by Lambert’s cavalry. Wellington’s usual tactics in Spain can only be described as defensive-offensive. At Marathon the tactical offensive was, I think, certainly taken by the Athenians; but from Herodotus’ account it seems extremely likely, as has been pointed out by others, that the Persians challenged them by drawing up their army and offering battle; just as the Peloponnesians did, though without success, outside Athens during their first invasion of Attica.

But I do not pretend to be able to interpret exactly what passed through the Athenian generals’ minds. I do, however, insist that there is no justification for the way Marathon has been discussed as though it were very peculiar in these matters.

Finally, just one or two remarks about Munro’s theory. Munro, I should say, never claims that it is more than a theory. ‘The theory put forward’, he says, ‘does not contradict any well-accepted fact in the evidence, nor involve imaginary causes.’ I cannot subscribe to that, but it is the more recent tendency to regard his theory as final which makes a further reference to it imperative. I have already dealt with the cavalry problem. For the supposed division of the infantry the additional evidence is so slight that there is hardly anything to argue about. Everything in Nepos’ account, especially the numbers, seems to me palpably unreliable. The suggestion that the shield signal, which according
to Herodotus was only shown when the Persians were in their ships, was really when half of them were in their ships, still leaves the same difficulty that the Persians took action before the signal. If their intelligence service was so good that they knew that the signal was imminent, as Munro suggests, why was it necessary to employ so cumbrous a method of sending the final information? With what we now know of the value of tales about signalling to German submarines, I think we may well give up all hope of fixing the exact place of the shield signal in the story. An at first sight stronger argument of Munro is that the landing at Marathon must have been a feint to lure the Athenians from Athens. ‘Why burden themselves’, he says, ‘with a march of five and twenty miles through the enemy’s country when their fleet might have put them at once within striking distance of the city?’ How and Wells put it more strongly still. ‘Nor is it likely that the Persian leaders doubted their power to force a landing on the open coast near Phalerum.’ But the whole of military history, ancient and modern, shows that landings on foreign soil are generally made, if possible, away from the defending army. The Athenians, it is true, landed near Syracuse, but they first drew the Syracusans away by a fictitious message (they did not divide their force), for, says Thucydides,27 ‘They knew that they would fail of their purpose if they tried to disembark their men in the face of an enemy who was prepared to meet them’. Compare the great difficulty Caesar has in landing in Britain, where he fails to secure an unopposed landing. Contrast his easy landing in Epirus. But there is no need to go far afield for parallels. Immediately before Marathon the Persians attacked Eretria. In this case they landed at three different places, one of which, Tamynae, was some fifteen miles distant from Eretria.28 Why? Did they doubt their ability to land closer or were they intending to lure the Eretrian army out of Eretria? I cannot find that any modern writer has noticed this simple parallel. I suggest that the landing at Marathon was the most natural thing in the world and was due to a combination of simple causes. It was a good landing-place, fairly near Eretria and more or less on the direct route to Athens, where the Persians could disembark their cavalry and stores undisturbed. As I have said before, the ancients used ships as little as possible when they could use land, and to land at Phalerum would not only involve a longish sea voyage round a promontory, but would at the very least have been an extremely difficult operation in the face of the Athenian army. But, says Munro, if they intended to march on Athens from Marathon the Persian generals had quite time enough to send an advanced guard to occupy the passes. How and Wells are again more positive. ‘The idea that the Persians intended to march from Marathon on Athens is decisively negatived by the fact that they made no attempt to seize the passes leading from the plain of Marathon towards Athens.’ As so often it is here argued that a general cannot make a mistake, though history is crowded with instances of omissions by generals to take obvious precautions. And what does ‘occupy the passes’ mean? There is no Thermopylae between Athens and Marathon. Even if there had been, the detaching of a small body of Persians would not have denied it to the Athenians. The language of an age of machine guns is inapplicable to an age of bows and arrows. To occupy the passes the Persians would have had to detach several bodies each of which would have had to hold a position which could easily be forced or turned by superior numbers. It would have been a mere waste of men. Nor, even if successful, would it have secured the Persians an approach to Athens unless they occupied the Athenian end of the passes. To send scouts out would be reasonable; they may have done it, but if so the fact is recorded neither here nor on most other occasions in ancient warfare when the sending out of scouts seems reasonable. There are very few references in Greek historians to scouting, still fewer to the more solid protective duties, especially to the holding of advanced positions by small outposts.

27 vi 64. 1. 28 Hdt. vi 101. 1.
RECONSTRUCTING MARATHON AND OTHER ANCIENT BATTLES

To me it seems much more likely that the Persians had the surprise of their lives when the Athenians came out to Marathon. What the Persians finally did, or would have done, when thus cornered I do not know. They might have tried to re-embark their whole force, they might have tried to force their way through to Athens. Their position was a very difficult one. The Athenians finally accepted a challenge to battle. That at least seems most likely from Herodotus and is intrinsically most probable. If anything more elaborate happened we can only say that the means of discovering what it was are irretrievably lost.

Yet I do not wish to argue that there are no unsolved problems connected with Marathon. The importance to be attached to the possibility of treachery is a great problem which I do not pretend to solve. I would only point out that it is unlikely that, as seems often to be supposed, all the loyal men came to Marathon while the traitors were graciously excused the levy and allowed to remain behind. Nor have I attempted to meet all the arguments raised in favour of modern theories. If it is urged that their strength is in their cumulative effect I will try to deal with others during the discussion. I feel that I have said enough to make clear my point that though we cannot hope to reconstruct all the detail, Herodotus' account may not be quite so inadequate when judged in the light of what we know of contemporary warfare as it is if we take it apart from its context, and that where the evidence fails it is better to admit the fact. As Strachan-Davidson writes of German pamphlets in the introduction to his Polybius: 'I find myself in agreement with each of them in turn as each upon one point or another is content to accept the plain statements of Polybius or to draw obvious common-sense inferences from his language. Beyond this, unless I am mistaken, little has been discovered or can be discovered of sufficient certainty to justify us in receiving it as history. Of most of the propositions advanced I feel myself compelled to repeat "It is probable and the contrary is also probable". When, as is too often the case, the theory leads along an elaborate series of deductions into direct contradiction with a statement of Polybius, a more decisive verdict may be pronounced upon it.'

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SOME AGIAD DATES: PAUSANIAS AND HIS SONS

THUCYDIDES' digression (ι 126–38) on the later careers of Pausanias and Themistokles is deservedly famous for the lucidity of its style and the interest of its subject matter, but the chronological difficulties of the apparently perspicuous narrative are notorious. A. W. Gomme singles out the date of Themistokles' flight to Asia Minor as the major difficulty of the first half of the Pentekontaetia, 'a difficulty which, if it could be solved, would lead to the solution of most of the others'. He concludes his discussion by saying: 'The question remains unsolved.' However, some simple facts (for which Thucydides is himself the authority) about the number and ages of Pausanias' sons have a direct bearing on the time of Pausanias' death and so on the date of Themistokles' flight; curiously enough their importance has not been noticed.  

After the deaths of his uncle Leonidas and his father Kleombrotos in 480, Pausanias became regent for his nephew Pleistarchos, the son of Leonidas and Gorgo, who died in 459/8 or 458/7 and before the battle of Tanagra. Pausanias' eldest son Pleistoaanax then succeeded as the king of the Agiad royal house. Thucydides (ι 107.2) says that he was still a minor and that his uncle Nikomedes, the younger brother of Pausanias, commanded for him at Tanagra in 458 or 457.  

Pleistoaanax did, however, command the Peloponnesian forces which invaded Attica in 446, just after the revolt of Euboea (Thuc. i 114.2). Plutarch (Per. 22.2) says that because he was young the ephors sent advisers with him, especially Kleandridas who is described as φυλάκα και πάρεθρον αὐτῷ διὰ τὴν ἕλκιαν. If Pleistoaanax was too young to command at Tanagra, this must mean that he was still under twenty in 458 or 457. In Sparta military training seems to have been completed and regular service begun by the end of the twentieth year, the upper age-limit for the ephebic class.

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1 A Historical Commentary on Thucydides i (Oxford 1945) 401.
3 The usually accepted date for Tanagra is 457, see Gomme, Commentary i 313, 325–6; the editors of ATL iii 171–3 argue for a year earlier, 458. Diodoros (xii.75.1) places the death of Pleistoaanax in 408/7 after a reign of 50 years, and so his accession in 458/7; but at xiv 89.1 the exile of his son the younger Pausanias is placed in 394/3 after a 14-year reign. This is a year too late: see Xen. Hell. iii 5.25 for the defeat at Haliartos in 395/4 and his flight to avoid the death sentence on his return. The same discrepancy of a year may have affected Diodoros' dates for Pleistoaanax, so that his accession and Pleistarchos' death may have been in 459/8 rather than 458/7; cf. Ed. Meyer, Forschungen ii (Halle, 1898) 509, 509–11 on possible reasons for this and other difficulties in this portion of Diodoros' Agiad king-list. Nikomedes' command at Tanagra raises an interesting question, why did he command rather than Archidamos, the Eurypontid king? It is tempting to conjecture that Archidamos was needed at home because the Helot revolt was not yet completely quelled, so that we have here an additional reason for not emending Thucydides' δεκάτορ ἔτει (ι 103.1); cf. Gomme, Commentary i 402–13. Professor Andrewes has pointed out to me that it is also surprising that the young and inexperienced Pleistoaanax commanded the important invasion of Attica in 446 rather than Archidamos, and makes the interesting suggestion that Archidamos 'was Kimon's friend and had no stomach for ventures against Athens', and that Kimon's earlier dismissal from Ithome could be due as Thucydides suggests to the actual feeling among the Athenian troops rather than to any distrust of Kimon.
called εἰρενεῖ. It was at twenty also that political maturity was reached and a Spartan began to attend the assembly. A king who succeeded as a minor would take up his military and political duties as soon as possible, and there is no reason to believe that this was later than the normal age of maturity for ordinary citizens. By 446 Pleistoxanx had become old enough to command, but was still young enough to need advisers; by this time then he was over twenty but still under thirty. If, therefore, he was not yet twenty in 458 or 457 and under thirty in 446, his birth must fall no earlier than 475. The probabilities are, moreover, that he was more than one year below thirty since his youth is specifically mentioned as the reason for the advisers, so that his birth should probably be placed nearer to 470 than to 475; in other words it is more likely that in 446 he was in the mid-twenties than almost thirty. He was charged on his return to Sparta with having accepted a bribe from Pericles to retreat before he had advanced beyond Eleusis, and was exiled for eighteen years, returning to Sparta to resume his responsibilities as king in the late summer of 427.

What is known about the age of his son Pausanias, who was nominally king during his exile and succeeded on his death in 409, fits very nicely with our proposed date of 475-70 for the birth of Pleistoxanx. Thucydides (iii 26.2) says that this younger Pausanias was not old enough to command in 427 when his uncle Kleomenes, another son of Pausanias the Regent, commanded for him. The younger Pausanias was therefore still under twenty in 427, and born in 446 or later. How much younger he was is not known because he did not have occasion to command again until after his father's death. The fact that Pleistoxanx's eldest son was still a minor in 427 is an additional indication that Pleistoxanx at Cyprus and Byzantium. If the Agiad king or his regent was for any reason considered unsuitable, the other royal house could supply the commander as happened, e.g., in the early years of the Peloponnesian War when Archidamos led the annual invasions of Attica until 428 because Pleistoxanx was in exile and his son was still a minor. The prominence of Agis in the later years of the war was similarly due to distrust of Pleistoxanx and his own lack of self-confidence (Thuc. v 17).

4 This seems to be the meaning of the schol. ad. Her. ix 85, and the gloss in Strabo's Geography published by A. Diller, AJP clii (1941) 499-501; see the recent discussions by A. Billheimer, TAPA lxvi and lxvii (1946 and 1947) 214-20, 99-104; K. M. T. Chrimes, Ancient Sparta (Manchester, 1949) 84-136; W. den Boer, Laconian Studies (Amsterdam, 1954) 248-61. This interpretation is confirmed by Agesilao's remark in 379 B.C. quoted by Xenophon (Hill. v 4.13) that it was more than forty years since he had come of military age (ὑπερ τετεράκωτον ἄρι 

5 Both point out that the prohibition in Plut. Lys. 25.1 is against going to the Agora to buy household necessities under the age of thirty, and has nothing to do with attending meetings of the Assembly.

6 It will be remembered that Alexander commanded the left wing at the battle of Chaironeia in 338 B.C. at eighteen, and succeeded to the throne of Macedonia in 336 B.C. at twenty. There were two kings in Sparta; the Agiad was the senior royal house and normally held the more important commands as Pausanias did at Plataea in 479, and in the next year's operations with the Hellenic fleet
himself was unmarried and so probably under thirty in 446; this line of argument also shows that he was born later than 476.

Pausanias the Regent had two sons younger than Pleistoonax: the Kleomenes already mentioned who commanded in 427, and Aristokles who was said by Thucydides (v.16.2) to have helped Pleistoonax to bribe the Pythia at Delphi to secure his return from exile later in the same year; he may be the same man who was one of the polemarchs under the command of Agis at the battle of Mantinea in 418 (Thuc. v.71.3, 72.1).

The fact that three sons reached maturity, the eldest of whom was not born earlier than 475 and probably not much before 470, rules out a date as early as 473 for the death of Pausanias, and indeed any date before c. 470 (see below, pp. 143–5). It should be emphasised that even this is a minimum calculation and assumes that the three sons were born in succession as rapidly as possible. Thus what we know about Pausanias’ sons makes c. 470 the earliest possible date for his death.

The lower limit is, of course, provided by Thucydides’ account of Themistokles’ flight to Persia. When Themistokles arrived in Epheso he wrote to Artaxerxes who had lately become King (νεωσε ρω Βασιλειοντη, Thuc. i.137.3). Fortunately Xerxes’ death and Artaxerxes’ accession can be dated by independent evidence to late in the year 465. This is the only datable episode in Themistokles’ journey, and it is at the end. Now Thucydides’ narrative of the condemnation and flight follows immediately on the account of Pausanias’ death and suggests little delay between it and the Spartan charges against Themistokles. He says that Themistokles had been ostracized by the Athenians, was living in Argos, and travelling about the rest of the Peloponnesse. Presumably it was because his presence was disturbing to the Spartans, who feared that he was encouraging the anti-Spartan activities of Argos, Elis, and the Arcadians, that they saw an opportunity to be rid of him in the circumstances of Pausanias’ death. They claimed that evidence was found in their investigation of Pausanias’ Medism to implicate Themistokles and sent ambassadors to Athens demanding his punishment. The Spartan charges were not questioned, and Athens sent persons to arrest him. Themistokles did not await their arrival; Thucydides describes his flight from Argos to Kerkysra, then up the west coast of Greece to Admetos, King of the Molossians, who conveyed him to Pydna. From there he took ship for Ionia, and only

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11 Greek men normally married at thirty or shortly thereafter; see, e.g., Hesiod, Works and Days 695–7; Solon, fr. 27 (Edmonds) 9–10; Plato Rep. 5.460e, Laws 6.785b, 6.772d; Arist. Pol. 1333a. For Sparta there is the specific evidence of Plut. Lyce. 25.1 that men under thirty did not go into the Agora at all but had their household necessities supplied by relatives and lovers (αλλα δια των συναγεσκων και εραστων επιονυτο τας αναγκαιας οικονομιας; this surely implies that a Spartan was unmarried and did not have his own household under thirty, but that after thirty he would usually be married.

12 This is the date most recently proposed by W. G. Forrest in CQ x (1960) 237–8 (‘473 at the latest’).

13 Diod. xi 69 places the murder of Xerxes and the accession of Artaxerxes in 465/4. Evidence from oriental sources enables greater precision: the terminus post quem for Xerxes’ death is August 4–8, 465 and it cannot have been much earlier than December 17th, since a Babylonian legal text has been found, dated to the month of Kislimu of the 21st year of Xerxes, which in 465 began on December 17th. News of Artaxerxes’ accession had reached Egypt by January 2, 464. See R. A. Parker and W. H. Dubberstein, Babylonian Chronology (Brown University Studies 19, Providence, R. L., 1956) 17 and S. H. Horn and L. H. Wood in JNES xiii (1954) 9. I am indebted to Proffessors F. M. Heichelheim and R. F. G. Sweet for calling to my attention the latter reference.

14 Contra W. G. Forrest who (in the article cited in note 12) believes that there was a long interval between the two, that Pausanias died not later than 473 and that the charges against Themistokles were laid in the winter of 469/8, after Kimon’s return from the Eurymedon. He finds support for this in Ephoros, through Diodoros xi 54. Some time, of course, must be allowed for the charges against Themistokles to be elaborated and acted upon, but this is more likely to be months than years.

15 See Gomme, Commentary i 437–8 for the names of his accusers: Leobotes of the Alkmeonii; Pronapès, perhaps the hippocam of IG ii 400 who was a colleague of Kimon’s son Lakedaimonios, and a Lyssandros. Plut. Them. 24.4 says that Kimon himself prosecuted the friend of Themistokles who smuggled his wife and children out of Athens. This
narrowly avoided being intercepted by the Athenian fleet blockading Naxos. Arriving in Ephesus he wrote to Artaxerxes who had lately succeeded Xerxes on the throne of Persia, and after a year spent in learning Persian went up to Susa. There he was received with honour, and appointed governor of Magnesia, a post he held until his death.

The whole narrative moves rapidly; the only delay mentioned is the one year spent in learning Persian after Themistokles had arrived in Ephesus and sent his letter to Artaxerxes and before he went to the Persian court (Thuc. i 138.1–2). A year or little more between the death of Pausanias and the arrival of Themistokles in Ephesus would be ample time to accommodate all that Thucydides relates. The lower limit, then, for Pausanias’ death should be placed c. 467/6.16

The two termini for Pausanias’ death are therefore c. 470 as the earliest, and c. 467/6 as the latest. Where between these two is the probable date? It has long been recognised that a date close to 465 would accord better with Thucydides’ account of it and its consequence, Themistokles’ flight to Asia Minor ending with the letter to Artaxerxes not earlier than the very end of 465. I suggest that the evidence about Pausanias’ three sons greatly strengthens the probability of the later date, and that the objections urged against it should be re-examined. The earlier date c. 470 for Pausanias’ death is possible, if his eldest son was born soon after 475, and the other two in rapid succession. But, as was urged above, the probabilities are that Pleistocanax himself was not born much before 470 if he was still of an age to need advisers in 446, and time must be allowed for the birth of the two other sons who reached maturity. Further probabilities should be considered: first, that Pausanias had daughters as well as sons; absence of any mention of them is no argument against their existence—it merely shows that they were neither famous nor notorious.17 Second, we should add the probability of one or more of the following common hazards: infant mortality or death of a child before maturity, foetal deaths or miscarriages, and periods longer than the minimum between pregnancies.18 Clearly a minimum estimate of the time to be allowed between the birth of Pausanias’ eldest son and his death, when two other sons are known to have lived to maturity, is highly unlikely. Even one of these probabilities (i.e.

16 Gomme, *Commentary* i 397–9 believes that 467 or 466 are the latest possible dates, but doubts that Pausanias’ trial could have been postponed so long; see below pp. 144 for discussion of this point.

17 A. W. Gomme, *The Population of Athens in the Fifth and Fourth Centuries B.C.* (Oxford: Blackwell, 1933) 75–82, Note C, points out that statistics compiled from, e.g., the *Prospopographia Attica*, showing a great preponderance of males, are misleading because the majority of known names are those of people in public life, ‘a field in which women did not compete’. Census figures from modern Greece show the sexes as approximately equal, with males predominating slightly in the age groups below thirty, and females predominating thereafter; male death rates are higher than female up to the age of 35. See *Demographic Yearbook* 1959 issued by the Statistical Office of the United Nations (New York, 1959) Table 5, p. 161, and Table 26, pp. 572–3.

18 Gomme, *Population of Athens* 79; the average infant death rate (under one year) in Mediterranean countries in the 1920’s was 120 per 1,000 births, and it was presumably higher, or at least as high, in antiquity. Greek girls married young, about fifteen (Xen. *Oec.* 7.5; cf. Arist. *Ath. Pol.* 56.7), or at Sparta perhaps a little older (Plut. *Lyc.* 15.3 advises that they be ἀκμαίοιόντες καὶ πατεῖσθαι). Demographic studies show that the fertility rate of girls under fifteen is very low, that it rises slowly between fifteen and nineteen, and then rapidly to a peak between twenty and twenty-nine; after thirty it declines again. In Greece in 1956 the number of live births to mothers under fifteen was 20, to mothers fifteen to nineteen, 5,160, to mothers twenty to twenty-four, 38,792, and to mothers twenty-five to twenty-nine, 57,618; *Demographic Yearbook* 1959, p. 11, and Table 11, p. 261. I am grateful to three colleagues in the Department of Political Economy in the University of Toronto for information about and discussion of these problems: K. F. M. Helleiner, N. Keyfitz, and Andrew Watson. They warned that a ten-year period between marriage and the conception of the third son who reached maturity is on any calculation of probabilities a minimum estimate; see below p. 150 f. for the suggestion that Pausanias married c. 475.
that Pleistoanax was not born until nearly 470, that there were daughters as well as sons, that one or more children died before maturity, that all pregnancies were not at the shortest possible intervals and did not result in live births) makes 470 for Pausanias' death improbable. It is obvious that more than one should be allowed for, and therefore that the closer to 465 we put his death the less improbable the date will seem from the point of view of vital statistics.

We must now turn to the objections and ask whether there is any evidence about Pausanias' later career or Themistokles' condemnation and flight that rules out a date as late as 467/6. So far as Pausanias is concerned, the chief objection has been a priori, that it is incredible that the Spartan authorities should have allowed his intrigues to go on so long. But does Thucydides' narrative suggest that this is a valid objection? Plataia was fought in 479. In the next year, 478, Pausanias was sent out as commander of the confederate Greek forces to continue the expulsion of the Persians from the Greek cities of Asia Minor. After the capture of Cyprus, he went north to Byzantium where the arrogance of his conduct during the siege and the suspicion that he had released Persian prisoners to curry favour with the Great King as part of a design to rule Greece as a Persian vassal alienated the newly-freed Greek cities. The Spartans recalled him before the end of 478 to stand trial, and during his absence the Athenians, the character of whose commander Aristeides inspired the confidence that Pausanias' conduct had forfeited for the Spartans, assumed leadership of what is now commonly called the Delian League. In Sparta Pausanias was tried; although there was not enough evidence to convict him of Medism he was censured and deprived of command. He returned to the Hellespont the next year and recaptured Byzantium, 'on his own responsibility and without authority from the Spartans' (δικοστια με νυκτερετ έξεχερεθη, ιδια δη αυτος . . . άνευ Λακεδαίμωνοις αρκυκεραται ἐς 'Ελλαδοντον, Thuc. i 128.3). He was expelled before the end of that year, 477, but did not return immediately to Sparta. Instead he settled at Kolonai in the Troad to continue his intrigues with Persia. How long he remained in the Troad Thucydides does not say, but remarks (i 131.1) that the ephors 'hesitated no longer' (ούτω δη νυκτερετ ζεπασχον) on hearing rumours that he was up to no good (ουκ έστι αγαθω την μονην ποιομενος). Professor Gomme (Commentary i 397) thought this implied that his stay was no long period and placed his second return to Sparta 'in 474 or 473 at the latest'. Even if one disagrees and believes that such words as δωροθεις and την μονην δο imply a considerable interval, it is impossible to estimate its exact length.

In Sparta again he was imprisoned briefly, but secured his release when he offered to stand trial and no Spartan dared to lay a charge against him. Thucydides says that neither his enemies nor the state had the incontrovertible evidence needed to charge a member of the royal family, especially one in the high position of regent. Pausanias, therefore, continued to live in Sparta, and, Thucydides implies, continued to act as regent for his nephew Pleistarchos who could scarcely have come of age until after 470 (see below, p. 151).

Later investigations revealed that during this period Pausanias continued correspondence with Persia, and finally committed the unpardonable offence of inciting the Helots to revolt. Some of the Helots gave evidence of the promises of freedom and citizenship he had made, but the ephors still did not dare to proceed against him. Evidently the word of Helots would not suffice against the denial of a royal personage. At length one of his

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19 F. Jacoby, Die Fragmente der Griechischen Historiker (FGH) 70 Ephorus F 191.37–46; cf. Gomme, Commentary i 399–400 and Meritt, Wade-Gery, and McGregor, ATL iii 158–9. Gomme and the editors of ATL give good reasons for rejecting a seven-year rule in Byzantium based on the emended text of Justin's epitome of Trogus 9.1.3 and accepted by, e.g., K. J. Beloch, Griechische Geschichte ii.2 (Strassburg, 1916) 185–8, C. F. Lehmann-Haupt, Klio xvii (1921) 59–73, and F. M. Heichelheim, Zeitsch. f. Num. x1 (1930) 22–4, who uses as a supporting argument the existence of iron money in Byzantium. Diodoros (xi 44–6) puts his whole account of Pausanias' later career under this year, but which episode he intended to assign to it is anyone's guess.
servants, who had been entrusted with a letter to the Persian satrap Artabazos and became suspicious when he reflected that none of the previous messengers had ever returned (this implies several years of negotiation), turned informer and provided the ephors with the indubitable written proof they felt was required. To make assurance doubly sure, they arranged to overhear the interview between Pausanias and his servant. Only when with their own ears they had heard Pausanias' damaging admissions did they decide to arrest him. He, suspecting or being warned of their intention (Thuc. ix 134.1 says by one of the ephors out of goodwill), took asylum in the temple of the Goddess of the Brazen House where they starved him to the point of death, bringing him out at the last to avoid polluting the temple; he died as soon as he was carried out.20

Such is Thucydides' account. It concerns itself exclusively with Pausanias and he dates none of the incidents except the siege of Byzantium in 478. From other sources it is known that Sparta was having her troubles in the Peloponnese during the late seventies and early sixties, troubles undoubtedly aggravated by Themistokles' residence in Argos and travels in the Peloponnese after his ostracism, and by the fact that neither royal house could provide strong leadership, or worse still that both incumbents may have been suspected of conniving with the dissident parties. Pausanias, Regent of the Agiad house, was under constant surveillance for possible treason after his return to Sparta and he eventually lent encouragement to the demands of the Helots; Leotychidas, the Eurypontid King, was in exile at Tegea until his death in 469/8 for having accepted bribes during his expedition to punish the medising Thessalians, and his grandson Archidamos may have been acting for him, but his position was obviously insecure and difficult.21 Thucydides throws no light on Pausanias' connexion with these anti-Spartan movements (the evidence for them is so meagre that both chronology and interpretation differ widely in the various modern discussions22) except in the case of the Helots. If, as here suggested, there is

20 The action of the ephors was apparently resented by a strong faction in Sparta; indeed there may have been doubts about the evidence alleged against him since there was no trial and he had no opportunity to defend himself. At any rate the ephors were forced to give him proper burial and to allow him honours after his death: Delphi later ordered his tomb to be removed to the spot where he died and two bronze statues of him to be dedicated to Athena Chalkioikos; Thuc. i. 134.4 with Gomme's commentary ad loc. and Paus. iii. 17.7. Herodotos also reflects a tradition favourable to Pausanias; see W. W. How and J. Wells, A Commentary on Herodotus (Oxford, 1912) ii 12 for references.

21 Her. vi 71–2; cf. Plut. Them. 20.1, Arist. 22.2, Mor. 859 D; Paus. iii 7.9. The Thessalian expedition is not dated by Herodotus but, if it be granted that the setting of Plutarch's tale about Themistokles' plot to burn the Greek fleet at Pagasai is-historical (the plot itself may be rejected as a fabrication of the later anti-Themistoklean propaganda), E. M. Walker's date of 479 (CAH v 466) seems to me the most probable. To Walker's arguments for 479 might be added: (1) the phrase ἀντὶ δερτοῦ Σέρβου of Them. 20.1 is more appropriate to 479 than later; (2) a naval expedition from Sparta to Thessaly seems unlikely, but there is less improbability in the Hellenic fleet (minus the Athenian contingent) stopping in Thessaly on route from the Hellespont (Her. ix 114 says that they departed ἔτει τὴν Ἑλλάδα and Thuc. i 89.2 ἐπὶ οἶκων but neither wording precludes their stopping in Thessaly on the way); (3) when Pausanias was recalled from Byzantium in 478, why was Dorkis (Thuc. i 95.6) sent to succeed him unless Leotychidas who had commanded in 479 was already in disgrace?

For the dates of the reigns of Leotychidas and Archidamos see Gomme, Commentary i 405–7. Professor Andrews suggests to me that Diod. xi 48.2 where Leotychidas' death is placed in 476/5, archon Phaidon, is simply a mistake: that Diod. confused Phaios, whom he gives as archon for 469/8—the correct year of Leotychidas' death—with the earlier Phaidon, and that there is no question of confusion between exile and death.

22 Recent discussion of these Peloponnesian events may be found in Gomme's Commentary i 401–8; Meritt, Wade-Gery, and McGregor, ATL iii 158–80; A. Andrews, 'Sparta and Arcadia in the Early Fifth Century', Phoenix vi (1952) 1–5; W. G. Forrest, CQ n.s. x (1960) 221–41. Argos, always hostile to Sparta, was for all or part of this period under the regime which had seized control after Sepeia in 494 B.C.; there had been synoikisms at Elis (471/0) and Mantinea, accompanied probably by the establishment of democracies in both places; Sparta had to fight two battles against the Arcadians at Tegea and Dipaia about this time; and the Helots revolted at the time of the great earthquake c. 465.
reason to believe that Pausanias was alive until 467/6, his tampering with the Helots was a contributing cause of the revolt: he, like Kleomenes before him, had raised hopes the disappointment of which exacerbated the discontent, and the earthquake provided the favourable opportunity for the great revolt. It is probable that his intrigues with the Helots rather than his Medism moved the ephors to take the decisive action that resulted in his death. By contrast, the objection that the Spartan authorities would not have allowed his Medism to continue so long carries little conviction. Thucydides’ account does not suggest that it posed any serious threat; the ephors had suspected that he was in correspondence with the King of Persia ever since his return to Sparta, and forewarned was forearmed. On the other hand, Helot discontent was too dangerous a subject to be publicly aired and therefore Medism was used as the formal charge for which the evidence of his letters, and of the ephors who had heard his admission to his slave could, it was said, be produced. Moreover Medism had the advantage of being the kind of charge that could arouse a storm of patriotic indignation under cover of which he could be eliminated without a formal trial or too many questions asked. His Medism, then, could have gone on harmlessly for many years and provides no clue to the length of his residence in Sparta, but the Helot revolt of c. 465 is a more valuable chronological indication. If independent evidence suggests a date shortly before his death, the Helot revolt, far from presenting any difficulties, strengthens the probability that he died about 467/6.

The second part of our question should now be put: is there any evidence in the latter half of Thucydides’ disgression on Themistokles’ ostracism, condemnation, and flight that makes the date c. 467/6 for Pausanias’ death impossible? The implications for the chronology of Themistokles’ later career may be summarised briefly:

1. If Pausanias died not earlier than 470 and in all probability c. 467/6, the year 471/0, under which Diodoros (ix 54–59) includes his account of Themistokles’ first trial, ostracism, condemnation, flight, later life, and death in Persia, cannot be the date of Themistokles’ flight to Persia. If one of the events Diodoros records is to be assigned to that year, the ostracism is the most likely, but this is as much as can be said for Diodoros’ date.

2. Themistokles’ residence in Argos, his travels in the Peloponnese, and his suspected complicity with Pausanias continue into the early 460’s; they begin, of course, with his ostracism, whatever date be accepted for that.

3. If Pausanias’ death and therefore the condemnation of Themistokles should come down to c. 467/6, Thucydides’ account of the actual flight from Argos to Pydna, the voyage to Ephesus, and the sending of the letter to Artaxerxes late in 465 or soon after presents only one serious problem, Themistokles’ encounter with the Athenian fleet besieging Naxos. The other major problem of the flight has been to account for a delay of several years between its beginning, if Themistokles left Argos in 471/0 or a year or so earlier or later, and its end in 465 when he arrived in Ephesus and wrote to Artaxerxes. This problem disappears with the shortening of the interval. Those who

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23 As is maintained by, e.g., R. J. Lenardon, Historia viii (1959) 26 ff., with n. 15 for references to earlier advocates of this date, Highby, Busolt, and Jacoby. It should be added that Cicero (Lael. 12.42; Brut. 10.41) does support 471 or 470 as a year of crisis in Themistokles’ life; he says expulsus esse which may mean either the ostracism or later condemnation.

24 So W. G. Forrest, CQ x (1960) 241; cf. Gomme, Commentary 401 for similar scepticism about the value of Diodoros’ date. It is accepted as the date of the ostracism by Flacelière (see n. 2), and the list of earlier writers cited by Lenardon (see n. 24) 26, n. 14. Lenardon dates the ostracism even earlier, c. 474/3.

25 As in Pausanias’ case, one suspects that the real ground for Spartan apprehensions was the trouble with the Helots; they feared that Themistokles was encouraging Pausanias’ intrigues with them rather than his Medism; cf. Thuc. i 102.3 and Kimon’s dismissal from Ithome.
begin the flight in 471/0 or thereabouts have avoided the difficulty of the encounter with the Athenian fleet at Naxos, but have had to explain a delay of some five years on the coast of Asia Minor, with Themistokes travelling from one city to another or residing in Ephesos before he wrote his letter.26 W. G. Forrest, who dates the flight in 469/8, has had to deal with the Naxos problem and a somewhat shorter but still awkward delay en route, which he places between Themistokes’ departure from Argos and arrival in Pydna.27 But Thucydides’ narrative is, as Professor Gomme points out, inconsistent with a delay of four or five years on the Asia Minor coast,28 and one may add equally inconsistent with a similar delay in the western stages of the journey. His account suggests at most months rather than years at the various stages—certainly in Molossia Themistokes was an unwelcome guest whom Admetos accepted with reluctance and presumably speeded on his way as soon as possible (Thuc. i 136.2–137.1; Plut. Them. 24). Thucydides (i 138.2) mentions only the one year’s delay spent in learning Persian, and that was after Themistokes had written his letter. In short, any time earlier than c. 466 for the beginning of the flight stretches the whole journey as described by Thucydides to improbable lengths.29

(4) There remains, or rather we have exchanged for the delay, the problem of Themistokes’ escape from the Athenian fleet besieging Naxos (Thuc. i 137.2). The date proposed by the editors of The Athenian Tribute Lists and now generally favoured for the revolt of Naxos is 470; but Themistokes, if he fled from Argos some years after 470, obviously could not have run into the siege of Naxos if it was going on in that year. This problem admits of no easy solution; but in the various attempts to find a solution two points have been raised. First, how reliable is this date (or for that matter the proposed alternative of 467) for the revolt of Naxos? Secondly, was it the siege of Naxos Themistokes narrowly escaped, or the siege of Thasos, as one—and by general consent the best—manuscript of Plutarch’s Themistokes reads at 25.2?

The date 470 for Naxos is derived thus.30 The battle of the Eurymedon is placed in 469 because of the episode recounted by Plutarch (Kimon 8) that at the Dionysia in the spring of 468 when Sophokles won his first dramatic victory, Kimon and his fellow generals were acclaimed when they entered the theatre and were persuaded by the archon to be the judges. This acclaim and the honour of judging at the Dionysia were, it is suggested, the people’s tribute to the victorious generals for the Eurymedon campaign of the year before. The revolt of Naxos precedes the battle of the Eurymedon in Thucydides’ narrative of the Pentekontaetia (i 98–101), and each is assumed to occupy a campaigning season; so, if the

26 ATL iii 112, n. 7, where the delay is accepted and corroboration found in Number 20 of the Epistles ascribed to Themistokes; also Lenardon (see n. 24), esp. pp. 37–9.

27 P. 241 of article cited in n. 24.

28 Commentary i 397–8. Gomme himself tentatively suggests that Themistokes may have written to Artaxerxes before the death of Xerxes, but is not happy about the suggestion because it convicts Thucydides of error. He rightly dismisses the testimony of the authors quoted by Plutarch (Them. 27.1–2) who had Themistokes meet Xerxes, rather than Artaxerxes. Their aim was dramatic effect rather than the solution of a chronological difficulty.

29 Plut. Them. 24.4 quotes Stesimbrotos as saying that Themistokes fled to Hiero of Syracuse, asking for his daughter’s hand in marriage and offering to make him ruler of Greece. Plutarch rejects a journey to Sicily as inconsistent both with the rest

30 ATL iii 160; cf. Gomme, Commentary i 397–9.
Eurymedon is 469, the latest possible date for Naxos is 470. This is an attractive explanation of the incident at the Dionysia but, as the editors of ATL are careful to point out, it is not the only possible explanation of the incident and does not yield a firm date for the revolt of Naxos. Kimon's great prestige after his triumphant return from Skyros with the bones of Theseus some years earlier (Thuc. i 98.2-3; Plut. Kimon 8.5-6), and then his reduction of Karystos could be the reason for the acclaim at the Dionysia of 468.

The later date, 467, for Naxos has been arrived at by working back from the revolt of Thasos which follows the Eurymedon in Thucydides' account (i 100.2). The revolt of Thasos can be dated with reasonable assurance to midsummer of 465. Again it has been assumed that each event occupied a campaigning season; so that, if Thasos is 465, the Eurymedon cannot be later than 466, and the siege of Naxos not later than 467. But is it necessary to assume that each event took up a whole campaigning season, and therefore that the latest date for Naxos is 467? Is it impossible that the three events occurred within two seasons, so that the siege of Naxos could have been going on in 466? If Themistokles encountered the Athenian fleet at Naxos en route from Pydna to the Asia Minor coast in 466, his stay in Ephesos need not be unduly long. Thucydides (i 137.3) says that he had to wait at Ephesos for the arrival of his money from Athens and Argos to reward the ship captain before he went inland to the satrap who forwarded his letter to the king. This implies some delay; the year and a half between the meeting with the Athenian fleet and the dispatch of the letter, which our chronology assumes, is long but not incredible. At the other end similarly, if Pausanias' death occurred early in 466, the time allowed for the charges to be laid against Themistokles and the various stages of his journey to Pydna is not inconsistent with Thucydides' description of a precipitate flight.

The other solution proposed (by, e.g., Flacelière) for the difficulty is more radical. Plutarch's account of the flight (Them. 25-6) differs from that of Thucydides at one significant point: he has Themistokles land at Kyme not Ephesos, and the best manuscript S (Seitenstettensis) reads at 25.2 Ὁδοῦ  instead of Ναξοῦ as the place where he narrowly avoided encountering the Athenian fleet. Flacelière has argued persuasively that Ὁδοῦ is the genuine reading in Plutarch, that there were two traditions about Themistokles' route, Naxos-Ephesos and Thasos-Kyme, and that Plutarch chose the latter as geographically a more natural route from Pydna and chronologically less difficult, since the revolt of Thasos occurred not long before the accession of Artaxerxes. He believes that the reading Ναξοῦ in the other manuscripts of Plutarch was a correction of an original Ὁδοῦ to bring Plutarch's account into conformity with that of Thucydides and Charon of Lampsakos. The counter-suggestion has been made: that the text of Thucydides originally read Thasos rather than Naxos. But it is not easy to see why a scribe copying the text of Thucydides should have altered the much easier reading Thasos to Naxos. Moreover the Naxos-Ephesos route was found in the account of the flight written by Thucydides' contemporary, Charon of Lampsakos (Plut. Them. 27.1), and was followed later by Nepos (Them. 8.6-9.1) and the Epistles.

31 Gomme, Commentary i 390-1; ATL iii 175-6, esp. n. 57.
32 Flacelière (n. 2), 5-14; cf. Frank J. Frost, 'Thucydides i 137.2' in CR n.s. xii (1956) 15-16 points out that the prevailing summer winds in the Aegean are 'notherlies', the etesian winds, and that a ship sailing from Pydna caught in one of them would run for shelter between Naxos and Paros. Only a southerly gale of some duration would carry a ship off course as far north as Thasos, and even then a mariner would avoid the 'iron' coast of Thrace opposite Thasos. His objections to Thasos are valid if a straight voyage from Pydna to Ephesos or Kyme is in question, but Themistokles' captain may have been making for some northern port of call on the way, and found Thasos his easiest place of refuge.
33 Gomme, Commentary i 398; for the Epistles and their sources see R. J. Lenardon, 'Charon, Thucydides, and "Themistokles"', Phoenix xv (1961) 28-40, with the bibliography there cited; for Charon as Thucydides' contemporary see F. Jacoby, Abhandlungen (Leiden, 1956) 178-206.
This Naxos-Theseus problem is still to be resolved, if indeed it can be. W. G. Forrest has posed two alternatives: ‘We must either read Θάσων for Νάξου in Thuc. i 137.2 or assume that Thuc. is wrong.’ There is a third possibility, that the revolt of Naxos should be brought down to 466. The dates here proposed for Pausanias’ death and Themistokles’ flight do not settle the issue, but they do pose the alternatives clearly and sharply.

If Pausanias’ sons, and daughters if any, were born between 475-70 and c. 467/6, it seems probable that he was married about 475, and that he was born about 505. This makes him younger than has usually been supposed when he commanded at the Battle of Plataia, but in fact fits what else we know better than earlier dates. The following table with its accompanying notes sets forth five generations of the Agiad family with their known or probable dates.

(a) King c. 560—c. 520. The first dated event of his reign and therefore terminus ante quem for his accession is the putting down of tyrannies with the ephor Chilon, probably in the year of Chilon’s ephorate, 556 or 554; Rylands Papyrus 18, F. Jacoby FGrH 2A (Berlin, 1926) p. 504, 105 F. 1, lines 16–21 with commentary; cf. M. E. White, Phoenix xii (1958) 1–14 and G. L. Huxley, Early Sparta (London, 1962) 69–71 for recent discussions of the papyrus; for Chilon’s ephorate see T. J. Cadoux, ‘The Athenian Archons from Kreon to Hypschides’, JHS lxviii (1948) 108–109. His reign ended not later than c. 520, since his son Kleomenes was on the throne in 519 when the Plataians asked him for an alliance; Her. vi 108, Thuc. iii 68.5; see Gomme, Commentary ii 358 for a defence of Thucydides’ date of 519.

34 E.g., K. J. Beloch, Griechische Geschichte i. 2 (Strassburg, 1913) 171–8, esp. 172, in his full and valuable discussion of the Agiad king-list puts him at not less than thirty; Hans Schaefer, RE xviii. 2 (499-70)}
against the 509 date proposed by Grote and discussed by How and Wells, *Commentary on Herodotus* ii 109–110. Even if the date of the Plataian alliance is lowered to 509, the beginning of Kleomenes' reign cannot be dated later than c. 517 when Maimandros appealed to him for restoration; Her. iii 148 with discussion in 'The Duration of the Samian Tyranny', *JHS* lxxiv (1954) 39–40. A date as early as 525 for Anaxandridas' death and Kleomenes' accession is, I think, improbable in spite of Plut. *Apopth. Lac. Cleom.* 7 (Mor. 229 D) where one of the sayings of Kleomenes is assigned to the time of the Spartan attempt to depose Polykrates; cf. J. R. Grant, 'Leonidas' Last Stand', *Phoenix* xv (1961) 21, n. 3, who regarded the 525 date as possible. Herodotos, however, connects Kleomenes with Maimandros but not with the earlier Spartan expedition. Moreover Dorieus' colonial ventures in Africa and Sicily, which followed immediately on Kleomenes' accession, included participation in Kroton's destruction of Sybaris in 510 (Her. v 44–5), and therefore favour the later date of c. 520 for Kleomenes' accession; see Huxley, *Early Sparta* 77–80 and A. Schenk Graf v. Stauffenberg, *Historia* ix (1960) 181–215. Anaxandridas' reign, then, was about forty years and he was probably a young man when he succeeded, under thirty and not yet married.

(b) Anaxandridas' first wife was his niece, his sister's daughter; Her. v 39.1. The marriage is usually put c. 560 with the accession for no apparent reason. Note (a) suggests that Anaxandridas was under thirty when he became king; the marriage, then, was probably not much before 550. It had been childless long enough to endanger the succession (ten years?) when the ephors, failing to persuade the king to put away the wife he loved, insisted that he take a second wife. As soon as the second wife bore a son, the first wife became pregnant and bore three sons in quick succession: Dorieus, Leonidas, and Kleombrotos; Her. v 39–41.

(c) The second wife was the daughter of Prinetadas, son of a Demarmenos, and presumably related to Chilon the Ephor (Her. v 41), since Herodotos (vi 65.2) says that a Chilon, father of Perklatos who was betrothed to Leotychidas and taken from him by Demaratos, was also son of a Demarmenos; cf. Huxley, *Early Sparta*, Appendix B, p. 149 for a table showing the possible relationship with Chilon the Ephor. It would seem that the ephors both forced the second marriage and chose the bride.

(d) Kleomenes' birth has been placed at c. 540, assuming that Anaxandridas' first marriage (c. 550) was childless for about ten years before the ephors became sufficiently alarmed to demand the second marriage. The age of Kleomenes' only child, his daughter Gorgo, supports this. Herodotos (v 51) says that she was eight or nine in the winter of 499/8 when Aristagoras came to Sparta to secure aid for the Ionian revolt; she was born, then, about 507. Kleomenes, born c. 540, would be about twenty when he became king c. 520; if he married at the usual age of around thirty (c. 510), Gorgo was born when he was thirty-three or thirty-four. The conventional dating makes him ten years older, but it is surprising that if he had no child until he was forty-three or forty-four, his mother's relatives, the 'Chilonids', who had forced his father to take a second wife in like circumstances, had done nothing to secure the succession in their line.

(e) Dorieus was not more than a year younger than Kleomenes, since the first wife conceived shortly after the second wife bore Kleomenes; Her. v 41. His 'programmatic' name (as Professor Wade-Gery describes it) looks like a protest by Anaxandridas against the domination of Sparta's policy by Chilon and his group since his accession, and an assertion of his independence. On the death of Anaxandridas, Dorieus was bitterly angry that the succession went to Kleomenes as the eldest son; i.e., he repudiated his father's second marriage as no marriage and regarded Kleomenes as a bastard. See Her. v 42–8 and the discussions by Huxley and Stauffenberg cited in note (a) for the facts of this and his later colony at Herakleia near Mt Eryx and his death in Sicily c. 510. He may have had a son before his death, the Euryanax son of Dorieus whom Pausanias associated with himself in the command at Plataea (Her. ix 10.3). This young man presents difficulties; why did he not succeed Kleomenes in 490 instead of his uncle Leonidas, and why did he not become regent for Pleistarchos rather than Pausanias, son of Dorieus' youngest brother Kleombrotos? How and Wells, *Commentary on Herodotus* ii 290 advance two possible explanations: 'Perhaps Dorieus by going abroad forfeited the throne or renounced it for himself and his descendants, or possibly the Dorieus here mentioned belonged to a younger branch of the royal house.' The first is improbable in view of the official nature of Dorieus' colonies, and the later honour accorded to his memory (Paus. iii 16.4–5 and Huxley, n. 552, p. 140), the second because of the special name of Dorieus. It is more likely that Euryanax was illegitimate and for that reason ineligible for the succession. On the dating here proposed, Dorieus left Sparta in his twenties and was dead by the time he would have reached the normal age for marriage.

(f) Leonidas was conceived soon after the birth of Dorieus and born therefore 538-536 (Her. v 41.3). Kleomenes betrothed his daughter Gorgo to him; the marriage took place with no doubt in 490 in the crisis of the succession when she was sixteen or seventeen and Leonidas about thirty years older, i.e., in his middle forties. R. W. Macan, *Herodotus, the Seventh, Eighth and Ninth Books*, Vol. i, Part 1 (London, 1908) 307, suggests that Leonidas' marriage to Gorgo was to safeguard his claim to the throne against that of Euryanax. If Leonidas had had an earlier wife before Gorgo who had died or whom he had divorced, there were apparently no surviving children. He and Gorgo had only one son, Pleistarchos, who
succeeded on his death at Thermopylae (Her. ix 10.2). He was under ten years old at the time, probably not more than five or six, since Pausanias appears from Thucydides’ account to have been regent until his death (above p. 144). Pleistarchos was born, then, c. 485. This conclusion fits neatly with the fact that he died childless (or at least sonless) in 459 or 458 (Diod. xiii 75.1); this is entirely natural if he was still under thirty.

(g) Kleombrotos was so close in age to Leonidas that some said they were twins (Her. v 41). He was therefore not more than a year younger and born 537-33. If he married at thirty, his eldest son Pausanias was not born before about 505 (this presumes that the first child was a son). Pausanias was old enough to be regent for his cousin at Platea in 479, and so at least twenty, but more probably about twenty-five. His birth should accordingly be put about 505.

There was another son, Nikomedes who commanded at Tanagra for Pleistonax (above p. 140). Kleombrotos was regent for Pleistarchos for the brief period between Leonidas’ death at Thermopylae and his own death before the campaigning season of 479 began; Her. ix 10 and C. Hignett, Xerxes’ Invasion of Greece (Oxford, 1963) 273-4.

Dates about ten years earlier than these are usually proposed for the marriages of Anaxandridas and the births of his sons and grandsons. It is assumed that Anaxandridas was already married when he became king c. 560 (see above, n. a), and the second marriage and Kleomenes’ birth are placed at c. 550; the births of the three sons of the first wife are likewise ten years earlier. Kleomenes would then be about thirty when he became king c. 520, over forty when his daughter Gorgo was born c. 507, and about sixty at the time of his death. Dorieus would be in his thirties during his colonial ventures in Africa and Sicily, and a little over forty at the time of his death. Leonidas would be between forty-five and fifty when he married Gorgo c. 490, and fifty-five to sixty at Thermopylae. By the same reckoning Kleombrotos, born between 547 and 545, could have been married by 517-15 and his eldest son Pausanias in his thirties at Platea.

Whether these earlier dates or the later dates suggested in the notes be accepted, two things must be kept in mind: first, that the four sons of Anaxandridas must be considered together since all were born within about five years; secondly, that the conventional dating rests on an assumption supported by no specific evidence—that Anaxandridas was thirty and married when he became king. The strongest argument in favour of the scheme here proposed is that with which this paper began, that the age of Pausanias’ eldest Pleistonax renders the marriage of Pausanias himself more probable in the 470’s than the 480’s. Two other indications already mentioned point in the same direction: (1) the fact that Kleomenes had only the one child Gorgo is less likely to have excited no alarm about the succession if Kleomenes was in his middle thirties than if he had been in his middle forties before any child was born; (2) the fact that Dorieus left no legitimate son when he died c. 510 is more probable if he was in his early twenties and unmarried when he left Sparta than if he had been thirty and marriageable.

The probability that Pausanias was still in his twenties when he commanded at Platea and that youth and inexperience may have been responsible for his mistakes thereafter is a matter of some importance for the understanding of the critical years of the transfer of the leadership of the Greek world from Sparta to Athens. Thucydides enumerates the counts against him: the boastful couplet he had inscribed on the base of the serpent column at Delphi, his arrogance and discourtesy to the allies at the siege of Byzantium, his personal extravagances in the matter of fine food and soft living, of personal attendance and court ceremonial, and finally his Medism. He leaves no doubt that he considered Pausanias’ behaviour the chief reason why the allies turned from Sparta to Athens (i 131.2); yet there is curiously little harshness towards Pausanias himself, especially in the story of his last years and death. Indeed in the last sentence of his digression (i 138.6) Thucydides couples Pausanias and Themistokles together as λαμπροτάτοι γενομένοι τῶν καθ’ έαυτούς Ἐλλήνων. This is understandable if Pausanias was only in his middle twenties at the time of his great victory; the adulation heaped upon him would have been enough to turn an older and soberer head—small wonder that the temptations of power and luxury were irresistible to
a young man suddenly released from the austerities of Spartan life. For the more serious charge of Medism we have only the word of his enemies. There is one surprising detail in Thucydides’ account of the summons for his return from Kolonai: the ephors sent a herald to him with a skytale (i 131.1). Professor Gomme (Commentary i 433) points out that a skytale was only granted to a commanding officer sent abroad by the state’, and asks the pertinent question, ‘In what circumstances could a Regent leave Sparta for a long time on a private mission?’ It is tempting to conjecture that there was sufficient doubt about the first charge of Medism for Sparta to allow him to return to Byzantium in the hope that he might restore Spartan authority in that area, with the warning that if he failed the consequences would be on his own head. It was a hazardous venture, and he failed. He may have been drawn into his later Medism, if it is true, by his angry disappointment at the lack of support on that occasion.

What is certain is that Pausanias’ three surviving sons, the eldest of whom cannot have been born before 475, make it highly improbable that Pausanias died until sometime in the sixties—the probable date, everything considered, is some time during the year 467/6. This solves the problem of the date of Themistokles’ flight to Persia, and has interesting implications for the ages, of Pausanias himself and of a number of other Agiads.

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NOTES

KOITH AKTAIONOS

Pausanias began the ninth book of his Periogesis with a brief topographical observation on the contiguity of Boeotia and Attica and specifically of the Platais and Eleutherai, followed by two digressions, the first legendary and toponymic, the second historical; together they occupy the remainder of the first chapter. The only topographical facts to be gathered from these digressions are: (1) that there was a direct road from Thebes to Plataia, and (2) that there was a road from Thebes leading to Hysiai in the direction of Eleutherai and Attica (cf. Thuc. iii 24; Arrian, Anab. i 7), which gave indirect access to Plataia (ix 1.6). The topographical thread is resumed at ix 2.1: Hysiai with its unfinished temple of Apollo and oracular well, Erythrai, and—closer to Plataia—the so-called Tomb of Mardonios were all located to the right of the road from Eleutherai to Plataia.

All this is straightforward and presents no major difficulties. The relationship between the roads already mentioned and the passes over Mt. Kithairon
was the subject of a lively exchange between Grundy and Frazer in 1896, but it can finally be considered settled. The road from Thebes to Plataia via Pass 2 as Pritchett has shown. We have already noted that Pausanius’ road from Eleutherai to Plataia emerges into the plain at the mouth of Pass 2; at or near this point it must have joined the Megara-Plataia road (and probably a Hysiai-Plataia road used by Neokles). So when Pausanius mentions the spring and the bed of Aktaion in terms of a traveller from Megara to Plataia, he must be describing features located south of the Bocotian end of Pass 2, where those coming from Megara would still be distinguishable from those coming from Eleutherai (or Hysiai, Erythrai, etc.)..

A traveller coming from Megara today by way of Hammond’s ‘Road of the Towers’ and Pass 2 will pass near a number of wells and cisterns before emerging into the Plataian, but only one spring. This is the vrysi Vasilikis on the lower slopes of Gouros Lestori (elev. 985 m.), approximately 1 km west of Villia on the modern Villia-Aigisthena road. It is clearly marked by a large deciduous tree and a small whitewashed shrine (rikonostasion). Some 400 m. west of the spring the modern road to Aigisthena turns sharply to the south towards Ayios Vasilios, and the ‘Quarry Road’ branches off slightly north-west to begin its circuit of Gouros Lestori. Due north of this fork, at a distance of a little more than 100 m. is an abrupt outcropping of stone rising from 10 to 20 m. higher than the sloping ground around it. Together the spring and rock may be said to mark the entrance to Pass 2 from the south. The ancient route traced by Hammond passes some 50–100 m. west of the outcrop and not much more than 1 km. west of the spring (fig. 1). Since this is the only spring along the route Pausanias must have followed of Pausanias’ description of this road as ‘schwerlich aus Autopsie’, Hermes ix (1875) 320 n. 2 (followed by Pieske, RE Suppl. iv 905 and Hitzig-Blümner, Pausanias iii 394), is groundless. It is likely, however, that this portion of Book Nine was inserted here by Pausanias from notes made on an earlier occasion, probably at the time of his visits to Pagai, Aigisthena and Ereueia (i 44.4 f.).

Whatever the date of the various roads, this route was doubtless ancient: Bölte, RE ix 1172; Pritchett, 16. Note that the road from Thebes to Hysiai need not be identical with the road from Thebes to Eleutherai (pace Pritchett, 22), and neither Pausanias nor Thucydides says that such was the case.

This is what Pritchett, 16 ff., calls ‘Zikos’ road’. It would have been possible to turn off to Hysiai after emerging from the mountains but as the map shows (fig. 1), this route is longer. The Thebans under Neokles doubtless used a direct road from Hysiai to Plataia than did Pausanias, who returned to the Eleutherai-Plataia road in order to visit the tomb of Mardonios.

Pritchett, 18–20 with his fig. 8, pl. 9. Again, my own investigations have substantiated Pritchett’s conclusions on this point. ‘Pass 3’ is supposed to have crossed the saddle of Mt. Kithairon where the N of ‘mt. Kithairon’ appears on my map (fig. 1).

N. G. L. Hammond, BSA xlix (1954) 103–22; Pritchett, 19. Wilamowitz’ hasty characterisation the identification of Vergutiani and its nearby cliff as the spring of Artemis and the bed of Aktaion; these cannot have been to the right of any feasible route from Megara to Plataia via Pass 2, as Pritchett has shown. We have already noted that Pausanias’ road from Eleutherai to Plataia emerges into the plain at the mouth of Pass 2; at or near this point it must have joined the Megara-Plataia road (and probably a Hysiai-Plataia road used by Neokles). So when Pausanias mentions the spring and the bed of Aktaion in terms of a traveller from Megara to Plataia, he must be describing features located south of the Bocotian end of Pass 2, where those coming from Megara would still be distinguishable from those coming from Eleutherai (or Hysiai, Erythrai, etc.).
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and it corresponds well with his description, the identification seems not only plausible but necessary: 
vrysi Vasilikis must be the scene of Artemis' ablutions and the nearby ledge must be the bed of Aktaion.

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A Kabirion Vase (PLATES V–VI)

This black-figure skyphos, ornamented with pleasing burlesque scenes, is now in a private collection1 in London. Although it lighted some time after the publication by Wolters and Bruns of Das Kabirenheiligtum bei Theben2 it has all the characteristics of a Kabirion vase. These vases have been securely classified by Wolters and Bruns and most of them3 are now to be seen in a recently opened gallery of the National Museum in Athens. The general form of the cup, the ear-shaped handles, the white ground and the black bands, the heavy frieze of grapes and the unmistakably anti-heroic style of the drawing, make it impossible to doubt that it comes from the workshop of the Kabiros Painter.

The subject-matter of Kabirion vases is often mysterious. With undoubted representations of Kabiros and of local gods and heroes, it includes many scenes of abandoned comedy which have defied modern solution. It has been suggested that many of the paintings may be related to sacred mysteries, and it is certainly true that they often allude to the cult of gods and heroes. But the treatment of these subjects is almost always broadly comic. Only Kabiros himself, in his majestic epiphany on the inscribed fragment, Wolters-Bruns K 1, and the mixed scenes on the skyphoi K 2 and 3, which are his companion pieces, is above burlesque. He is drawn in a different and more glorious style than the other characters. This style extends to his bull and his personal cup-boy.

It calls for too great an imaginative effort to connect the comic narrative vases, of which the new cup is typical, with those intimate mysteries of which Pausanias speaks.4 As far as the stories have anything to do with religion, it must be an area of religion remote from the centre of the mysteries;

My sincere thanks are due to Mr Ben Swannenburg, the owner of the cup, who has most kindly supplied these photographs, and to the Hon. Robert Erskine, who had restored it, through whose accurate memory it was easily identified, and through whom I approached Mr Swannenburg for the photographs. I should also like to record real gratitude for help and encouragement to Mr John Boardman and Mr Brian Shefton.

3 Berlin, 1940. And see now Hemberg, Die Kabiren, Uppsala, 1959, on the gods.

4 To their list should be added Ars Antiqua (Luzern) iv (1962) pl. 49 139; and the group discussed by Mrs Ure, JHS lxxi (1951) 194 ff.

significantly, the Great Mother is never alluded to on these vases. Among the scenes already known there have been burlesques of the Odyssey, Kephaleos hunting, revels of satyrs and menadons, battles of pygmies and cranes, a fragment of a sphinx, Heracles and Achelous, feasts and races, a traveller6 terrified by a huge snake emerging from a river, a komos with a donkey-cart, a fight between Heracles (with Athena) and a giant (μαχαίρος, with Ares), Nereids with the arms of Achilles, the beheading of Medusa, Ajax and Cassandra and the Judgement of Paris. There is also the fragment6 of an ogre or ogress crouching open-mouthed, or perhaps running.

The new cup unfortunately throws rather a negative light on the existing uncertainties, and although it depicts two recognisable events, neither of them has yet been recognised. The main picture represents a large, wild, hairy ogress chasing a beardless naked traveller in a conical cap. He has dropped his baggage (like the figure on the Berlin cup identified as Cadmus), and his clothes, and he seems to be making for a tree7 in whose branches two interested naked men have already settled themselves. An object at the foot of the tree may perhaps be a herm sketchily drawn, or more probably a plough. If the hero (or anti-hero) were identified with the so-called Cadmus (who differs from him chiefly by his stringy beard), we should be presented with a series of adventures of a Thersites-like comic hero, surprised by enormous snakes and pursued by fearsome ogresses, but presumably coming to no harm. He seems to be neither Odysseus nor Heracles.

Who is the ogress? We have no way of knowing her name. Is she a localised horror like Sybaris? Is she a burlesque version of a more distinguished figure? Can she be Karko, Onoskelis, the many-shaped Empousa? The activities and identities of

5 Conjecturally identified as Cadmus. This cup is in Berlin (inv. 3284). On the theory that these scenes, this in particular, represent a dramatic enactment of myths which would be a cousin (once or twice removed perhaps) of Athenian comedy, cf. Breitholz, Die dorishe Farce im griechischen Mutterland (Stockholm, 1960). The gorgon vase has the strange ballet-like quality of other gorgon vases, cf. Pickard-Cambridge, Dithyramb, Tragedy and Comedy (1962) 167, Webster's note.

6 The fragmentary figure in Heidelberg (K 45) was taken for a gorilla (McDermott, The Ape in Antiquity, Michigan, 1938, no. 324) or even a domestic ape (Wolters-Bruns, 103 n. 4). Female gorillas were certainly the subject of sailors' stories at this time; cf. the lively adventures of Hanno, GGM i 13 f.

7 This tree is not the vine itself which drapes the whole cup. If it were it might represent the miraculous or at least timely assistance of Kabiros. There are a number of plants on the vases independent of these dominating vines, whose only function is to be revellled under. The vines are not usually rootless, but cf. K 7 and perhaps K 16.
these ladies are entangled. The monstrous Empousa, who had one leg of metal and the other of cow-dung, is one of the hazards encountered by Dionysos in the underworld of Aristophanes, whose amazing description of her brings home to one how little contact we have with these levels of folk lore and superstition. The fact that such a θηριος could flourish there is itself an indication that what seems eccentric in these vases may really have been almost universal in Greek experience.

If the ogress has to be named, and from names at our disposal, it may seem best to call her Karko. This has the advantage of a possible connection with the cults of the Kabirion which Hesychius records: Κάβιριον Καρκίνος θανάτῳ δέ τιμῶτα αὐτῷ ἐν Λήμνῳ ὡς θεός λέγοντα δὲ εἶναι Παθαϊκόν παιδεῖς. But this is an uncertain argument, and it seems better to accept simply that we are dealing with a personage in an unknown myth. That the Boccians had their own traditional stories, different from those of the rest of Greece, we know from Pausanias, and this whole class of vases confirms it.

The naked figures up the tree are unlikely to be a pair of Kabirioi. Acusilaus and Pherecydes, both reported by Strabo, agree that there were three Kabirioi, but the vases nowhere offer us a convincing group of three, and the three male characters in the story illustrated here are not like enough to each other to be credible as brothers without external evidence. The other side of the vase seems at first sight easier to understand. A god or hero drawn in the same burlesque style as the characters with the ogress is reclining on a couch as if for a feast. The vine frieze continues but he has no cup, and although he has a table, nothing to eat or drink. He is wreathed (apparently with bays) and offering a similar wreath to one of three women. Can they be the three sisters or daughters of the Kabirioi? The two outer women are fully dressed, the central one, savagely burlesqued, is more than half-naked, examining her face in a mirror and gasping at it. The character she is facing is as interested as she is.

Once again one is left in the most melancholy ignorance. Unless the gift of the wreath and the conversation piece with the mirror were different events, and I am not able to credit this, the two dressed women are helping to beautify the central one. There can be no saying how august they may be, since the caricature of the three goddesses at the Judgement of Paris is quite as ferocious as this. The all but naked one is presumably dressing or undressing for the benefit of the reclining hero and will share his couch. This must surely be an allusion to a well-known Boeotian story, but when one looks for myths to fit it the field is too open. It could so far be a burlesque version of any heroic couple in mythology. If it were Cadmus and Hermione the necklace he gave her would have to be wrenched into a wreath; but the word θηριος covers both. The hero's lack of a beard is probably meaningless.

PETER LEVI, S. J.

16 Wolters-Bruns M 18 (from the workshop not of the Kabiros Painter but of the Mystic Painter).

The Dedication of Aristokrates

JHS lxxxiii (1963) 115 n. 82 leaves reasonable cause for doubt about my views, even for those who noted that 1959 came after 1957. IG II 772 is lost, but presumably the dedication was a fluted column with a tripod mounted on the capital. Consultation of Raubitschek, Dedications will show that no other inscription on a fluted column is known in Athens after 480. It will also show that no other Athenian fluted column had its inscription horizontally across the flutes; the 22 Akropolis pieces have vertical inscriptions. What the chronological implications of the abnormality are is anyone's guess. The eyewitnesses reported some alphas with sloping cross-bar and three-bar sigmas, which push the inscription up, and an isolated eta, which might be thought to bring it down (several cases 460-440, Raubitschek D 46, 135, 136, 166, 174; D 191 is earlier, though not perhaps as early as Raubitschek's 500). More serious is the spelling Σκελις, with single lambda. The inscription has influenced editors into thinking the single lambda correct, but only one ms. of any author has it, G of Thucydides at viii 89.2, G, we can now see, is a recipient (cf. Dover, CR n.s. iv 76), and a study of Hude's apparatus shows that it is correcting the aberration Σκελις. Double lambda is protected by metre at Ar. Birds 126, and required by metre at Ar. fr. 31 Demianzük. Single consonants for double are 'rare after 490, non-existent after 480' (Raubitschek, 444-6). On these facts, it seems most reasonable to conclude, not that the inscription was
'plainly archaising in style' (and what are the parallels for such archaising in fifth-century Athens?), but that it was archaic.

Whether this is what Plato refers to, I do not know, but it should be noted that the stone was found between the Greek and Roman Agoras, nowhere near the Python. One can make a list of choreic fragments which have worked their way round the Akropolis from the south-east, but there is nothing positive to associate this stone with the Python. It is not good evidence for the late use of three-bar sigma.

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The Seal of Posidippus: A Postscript

Professor C. A. Trypanis has kindly pointed out to me that in my article in JHS lxxxiii (1963) 75 f. I have confused two different inscriptions. The decree of the Aetolian League from Thurium recording a grant of proxeny Ἡπειροτικὸς τῶν ἐπιγραμματουχῶν Πελλάιων and mentioned by me on p. 76 contains no mention of an Asclepiades. Both an Asclepiades and a Posidippus are mentioned in a mutilated Delphian psephisma of about 276–5 B.C. published in Foulées de Delphes iii.3 (Epigraphie, 1943) no. 192, granting proxeny and other privileges to a number of persons of different nationalities.

This Delphian decree, and not, as I say on p. 87, that from Thurium, is the one which Professor Trypanis suggested might explain the reference in ll. 9–11 of the poem discussed in my article. Luckily this error makes not the slightest difference to my argument. In saying to Apollo, 'If ever in time past you showed me favour', Posidippus is almost certainly referring to his past poetry. Among many passages that could be quoted to illustrate this formula, one particularly relevant to the present instance occurs in the Prelude of Statius' Achilleid (1.6.1)–

\[ tu modo, si iteterum digno deplexius haustu, \\
 da fonts mihi, Phoebus, nox, ac fronde secunda \\
necly comas: neque enim Aoniun nemus adua ad pulso \\
nek mea nonz primis auxescant tempora uittis. \]

There follows a proud reference to the Theaid.

P. 81, on l. 2 καθαροὶς ὁδοι: we may compare the stories told by Pherecydes (3 FGH) fr. 92 and Anticleides (140 FGH) fr. 17.

P. 86, n. 13: Professor E. Fraenkel adds Catullus 105, 1 and Statius, Silv. i. 4.26.

P. 87, on l. 10 ἐκδιηρήτωρ: cf. Aristophanes, Thesm. 972 (Fraenkel).

P. 91, bottom: cf. Tacitus, Dial. 13 statuaerque 
tumulo non maestus et atrox, sed hilaris et coronatus. . . .

HUGH LLOYD-JONES.

Homer in Arabic

It is notorious that the Arabs knew almost nothing about Homer beyond that he was a poet. In a

book entitled sawān al-hikma (the cupboard of wisdom) by Muhammad b. Tāhir b. Bahram al-Sjāzī († A.D. 980) a chapter is given up to him. It says that Aristotle relied on his poems continually and all praised him for his skill as a poet, his solid knowledge and excellent counsel. One Stephen (probably the son of Basil) translated some of his poems into Arabic. To the question, 'Who is the greatest Greek poet?', Diogenes replied, 'Each one of them—in his own estimation; but most say Homer'. A brief dialogue is quoted:

HOMER. I wonder that men, who can copy God, do not do so but copy animals.

DISCIPLE. Perhaps they do this because they reckon that they die like them.

HOMER. For this cause my wonder is greater in as much as they feel that they wear a dead body and do not feel that in that body is a soul which does not die.

The rest of the chapter consists of maxims, mostly commonplace and often inconsistent. Such as:

An upright woman is the safety (υ. I support) of her house.

Marry a woman, not her dowry.

If you think of marriage, look at the neighbours. A woman knows nothing except what she wishes.

Bury a woman, do not marry her.

Write the oaths of liars in water.

Do not choose an ungrateful man as a friend.

A bad man creates obligations for his associates.

Lovers of money have no freedom.

Hunger and poverty destroy love which goes with satiety, not with hunger.

God hears a sincere prayer.

When things go well, think only of God.

Time destroys everything or causes it to be forgotten.

Pleasant is the thought of calamities to one who is safe from them.

The above is taken from a manuscript in the British Museum, Or. 9033 f. 35r.

The History of the Learned by al-Qifti adds a few details.

Anābū, the wag, said to Homer: Lampoon me so that I may lampoon you and boast of it for I am not worthy to praise you. Homer refused so Anābū threatened to publish abroad his refusal; whereupon Homer said: A dog in Cyprus wanted to fight a lion which refused out of pride; the dog threatened to tell all the wild beasts of this refusal. The lion replied, 'It is better to let all know of my refusal than to stain my whiskers with your blood' (p. 68). In a similar situation Homer told the parable of the sow and the lioness. The sow boasted of the number of piglets it produced so the lioness said, 'You speak truth; I give birth to one at a time but they are lions' (p. 70).

Homer spoke of things divine (p. 209).

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The list of contributors is security both for the quality of the writing and the nature of the conclusions. Stage by stage, the poems are traced from Homer to our editions. Prof. A. B. Lord shows that a poet of genius can arise within an oral tradition and can dictate a text superior to his usual performance. Miss L. H. Jeffery provides alphabet and writing materials by the second half of the eighth century. Prof. L. R. Palmer distinguishes early and late usages within a Kunstsprache. Sir Maurice Bowra presents the poems, each an artistic and considered whole, in style and composition typical of oral poetry, and probably the work of one Homer. Prof. J. A. Davison takes Homer's dictated or written text by an unprovable but not improbable succession of accurate copies: the Homeridae, Athens, Alexandria, Byzantium. Prof. J. A. K. Thomson follows the star of his influence from Hesiod to Joyce. The case is powerfully argued. The neophyte will return to the O.C.T. convinced that he has Homer's very words, and he will certainly read with much greater enjoyment and understanding. Doubts creep in, of course, at every stage. Prof. Palmer himself thinks the 'late' forms Peisistratean, though only the spelling ἐπείκεια points to a date after the eighth century. Prof. Lord and Miss Jefferies are doubtful if any incentive could have been strong enough to produce a written text so early. Even a text would, in an age wholly without scholarship, have been liable to corruption and 'improvement'. The Alexandrian critics do not sound as though they had the simple task of transcribing the Panathenaic text. Finally, the poems contain passages, long and short, which many will continue to think more characteristic of overworking at each of these stages than of the original oral poet(s). The strong argument for thinking that the disturbance was comparatively superficial is that novelties were freely admitted into the tradition down to c. 700 but not later; this argument is destroyed in the archaeological sections.

Part 2, on the Homeric world, is dominated by the originator of the Companion, Prof. A. J. B. Wace, and his opinions are shared, fully by Dr Stubbings (on whom his death in 1957 laid a heavy burden), Prof. C. W. Blegen, Prof. G. E. Mylonas, and Mr N. G. L. Hammond, and with large reservations by Prof. H. J. Rose and Prof. T. B. L. Webster: the poems are Mycenaean in content, with a very few 'anachronisms', which are mostly confined to the similes. But if the tradition is thought to have passed almost uncontaminated from the twelfth to the eighth century, the absence of elements later than c. 700 loses its value for dating. The only alternative mentioned is that nothing is Mycenaean except a few garbled memories. The assumption, implicit in Part 1 (and surely right), that the tradition grew and changed as long as it was creative, is ignored. The reader is given no opportunity of judging between them, since the history stops at 1000, without even a mention of the Anglo-Turkish excavations at Smyrna or of T. J. Dunbabin's The Greeks and their Eastern Neighbours, and no Proto-geometric or Geometric object is illustrated except one tripod, not even a single vase by the Dipylon Master.

The Companion is not for the Greekless, and it assumes considerably knowledge of the history of Greece and the East. The long and lucid account of the language requires also some acquaintance with philological terms, and the reader who has reached this stage will want to know, for instance, who proposed a new theory of a South-Greek unity, and why, and where to find it. Adequate aids to further study have apparently been confined to the chapters by Prof. Lord and Prof. Davison. If space was limited, we could have better spared the history before the Late Bronze Age and the geology of Greece, and simpler sections on language which did not take up a fifth of the book might have been more useful to the beginner. Paradoxically, the Companion is weakest in the qualities expected in a work of reference, impartial statement of the problems and guidance to readers whose interest has been roused, and strongest in the freshness and vivacity which will rouse that interest. These are shining virtues, achieved in defiance of war and mortality, and perhaps they should disarm criticism.

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The first problem that presents itself to the student of scholia is the circumscription of the subject-matter. Only in favourable circumstances do scholia preserve a regular commentary, a coherent text with its own problems of transmission and interpretation. Even where there is a discernible tradition it will normally be bedevilled by accretion, condensation and contamination, besides accidents of transmission high in frequency and deep in corruption. The Iliadic scholia, apart from the scholia minora (or D-scholia), the interlinearia of Venetus A, the σχόλια νεότερα of Venetus B, and similar matter (mostly glossing) in the Townley and Geneva scholia, are the membra, with
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additions and more or less disiecta, of the critical commentary known to Eustathius under the names of Apion and Herodorus and of the chiefly exegetical commentary from which the B-scholia (inflated with excerpts from Porphyry's Quaestiones Homericae) and the related Townley and Geneva scholia descend. These commentaries will only yield their full contribution to Homeric studies when the customary tools for the speedy probing of their contents are available. Unfortunately the text itself does not yet exist, though Prof. H. Erbse (Beiträge zur Überlieferung der IIassochilen, Munich, 1960) has published an introduction to what may one day be the definitive edition.

What Baar has indexed are the editions of Dindorf (A and B), Maas (T), Nicole (G), (owing to the economies of Nicole's publication a G-reference is given only for scholia not in AB), Laskaris (D), plus the six papyrus fragments of commentary known to Pack (Greek and Latin literary texts from Greco-Roman Egypt, Michigan 1952), omitting the matter relegated to appendices by Dindorf and his successors. These editors were aware of, and marked, other signs of lack of homogeneity in their material, the separation into marginalia and intermarginalia in A, or the three methods of referring scholia to text in B. Baar does not reproduce these distinctions. The conventional marks, one or more stars, could have been easily introduced, and would not have increased the bulk of a work of this kind. Baar limits himself to the principal grammatical, rhetorical, and aesthetic terminology of the scholia, a limitation interpreted strictly, even mechanically. The entries are thus not a necessarily complete record of the occurrences of a given term, but only of its use in a critical sense, e.g. γνωμικος, ιτημα, ιστορια, ιστορία and λόγος are not noted where they serve as headings—the last two not at all.

Such selectivity prompts a question: to what sort of queries is this Index intended to provide the answers? In practice it will be Homerists who will use this book, curious to discover quickly what line ancient scholarship took on a matter, and where. Such users will meet some quick frustrations: proper names (except 'Ομήρος and the equivalent ὁ ποιητής) are omitted. (There are eight grammarians, two editions, three mythographers and five poets cited in Schol. A to I.1—100 alone), though more than half a page is found for ἀεὶ and οὖν ἀδότητο: adverb is separated from adjective, but the noting of other special forms is sporadic—i.e. τίνι or τίνι (Schol. A to I.1) or σιτικος (Schol. A to I.217) etc., which must be sought under the 1st sing. pres.: scholia such as A to I.18 Σνρέβδος: σφών γράφων are listed (under γράφω) but not such as A to I.15 τίνι τινών 'Αρείδος: (in such a text as the scholia there is a strong case for indexing 'ideiic γράφων -οις -εντο); there is no cross-referencing—no 'cf. pro-. ssc.' under ὁθείον: some compound terms are indexed, e.g. ἀρετή ποιητική, but there is no knowing, save by reference to the epithets, whether e.g. σύνεσις is ἀγλατικός, δαββεβαιωτικός, παραπληρωματικός, etc. A little use discloses some omissions: βασιλικός, (contrasted with ἀριστοκράτειος—listed as ἀριστοκρατειος—by Schol. B to II. 370); λίθους, (e.g. τα έπος ας λίθον, δια τον μελημαν Schol. A to I.86, where μέλημα is indexed); μεμονωμένων (Schol. B to I.43): σπαργυρίς, (linked with γυμνικος by Schol. A to III.212); perhaps ἐπολαμβάνει (Schol. A to I.63 etc.), παραλαμβάνει (Schol. A to I.24 etc.), χράματα (Schol. A to I.169), are not regarded as sufficiently technical.

But the drudging lexicographer is too easily abused, and this is a tool to be welcomed: the format is clear, the paper good. One final point: this is a slender volume, and it is hard to envisage the production at reasonable cost of an Index to the Odyssey Scholia of less than half its size. The Odyssey's status of poor relation, from which Dindorf almost rescued it for the scholia, is in danger of becoming permanent.

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In this stimulating and closely-reasoned work, the author inquires whether Homeric man can ever be said to act himself, or whether all his actions are to be ascribed to (a) some psychic organ, or (b) to divine instigation? In reply to (a), Lesky insists, after discussing earlier answers, that Homeric man could and did treat the personality as unified, and that mention of the 'organ' confirms rather than contradicts this: Iliad 9, 403, εστε προς δω μεγαλητερα θυμος, and 407, την οι νωτα φιλο τελεσα το θυμος; refer to the same psychic event. In reply to (b), he quotes several examples (e.g.Od. 9, 339) in which divinely-motivated action and humanly-motivated action are contrasted: in neither poem does everything happen both in heaven and on earth, though certain things do: e.g. Phemius is both αὐτοθετικος and taught by a god, and the same action can be ascribed to Agamemnon's yielding to his θυμος (Iliad 9, 109) or to Zeus και μοιρα και ἡροορισης 'Ερυθνα (Iliad 19, 87). Lesky argues that the latter examples represent two sides of the same coin. Agamemnon's attitude determines which is uppermost: in 9 he is in deepest depression, in 19 less so. Lesky rejects explanations in terms of 'Göttterapart', 'overdetermination', or 'prästabillerte Harmonie', and affirms that what we find in Homer is an 'Urhäomen menschlichen Empfindens', by no means foreign to us today. This is a sane and balanced treatment of a much discussed subject, and deserves careful study.

In an interesting sketch at the end of the work, Lesky briefly traces through later authors the idea that both man and god may be held to be the cause of the same action. He maintains that Aeschylus has deepened this idea, and that Eteocles (Septem 653–719) and Agamemnon (Agam. 218–223) both show a man
resolving to do what he must do of necessity. Lesky argues that such a man is thereby guilty of doing what he does, because he wills it, and that this is a doctrine of Aeschylus. But surely Aeschylus (like Homer) does not consciously restrict responsibility to actions performed of one's own free will, or even of one's distinctly unfree will: except when Aeschylus brings the question of free will and responsibility into the foreground, as he does in Clytemnestra's plea, Agam. 1497 ff., it is still the result of an action that determines the appropriate treatment. In Aeschylus' time, and for some time thereafter, it is not necessary that one's will should be involved in order that one should be guilty: Sophocles' Oedipus did not wish to kill his father and marry his mother—he took precautions to avoid doing so—but nevertheless (O.T. 1360) he is ἰδὼ τοῖς and the child of ἀνυόων, just as Agamemnon's decision is ἀνυόων ἀνυόων. The interest in the portrayal of Eteocles' or Agamemnon's decision is surely psychological: the question of their responsibility for their actions is not raised, in the sense that responsibility is assumed, whatever the circumstances, just as Agamemnon's responsibility is assumed in Iliad 19, no matter how many gods he mentions as causes of his ἀγηγηγηγία. The reason why Clytemnestra may not claim to ἀνυόων is clearly that the acceptance of such a plea (once raised) would make a difference to her liability to be punished, whereas it makes no difference to the liability of Homer's Agamemnon to compensate Achilles; but where the plea is not raised, in Aeschylus too liability to punishment is assumed: it is only in the O.C. that Sophocles' Oedipus can be rehabilitated, and much hard thought—by non-dramatists—has been needed to do so.

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Except for Bacchylides, who has ranked since 1897 as a 'substantive' author, and for Sappho and Alcaeus, provided for in 1955 by Lobel and Page's Poetarum Lesbiorum Fragmenta, this volume, including as it does some new fragments of Alcman and Stesichorus, provides an up-to-date version of the third volume of Bergk's Poetae Lyrici Graeci, the text of which was last revised in 1882. Professor Page's arrangement of the text differs considerably from that of Bergk; whereas Bergk put all his authors, regardless of their relative importance, into a single more or less chronological sequence, followed by the mainly anonymous popular songs and scolia and by the Adespota, Page gives separate sections to his six major authors, Alcman, Stesichorus, Ibycus, Anacreon, Simonides and Corinna (so 'the tenth of the Nine Lyric Poets' wins at last her first team cap), puts thirty-five other named authors into a section headed 'melici Minores', adds sections for the Carmina Popularia, 'Carmina Convivialia' and Adespota, and ends with three indexes (names of poets, titles of poems, Greek words, including proper names).

Of the forty-eight authors recognised by Bergk, Page has omitted Polymnastus, Arion, Erinna, Pittacus, Bias, Chilo, Thales, Cleobulus (his principal fragment can be found, by reference to the index verborum, in the context to Simon. 581), Echembrothus, Sacadas, the Anacreontea and their Appendix, Phrynichus and (of course) Bacchylides. He has added Sophocles, Ion, Euripides, Pronomus, Lamynthius, Oeniades and Aristoteles, and has divided Philoxenus (somewhat hesitantly) and Stesichorus into two. Bergk's last author, Hermotodus, is now called Hermolocho. No instructed person is likely to wish to dispute with Page about most of these changes; but since so dim a figure as Xanthus is allowed a place (p. 363, fr. 699), it seems illogical to omit the better attested Polymnastus, Arion, Echembrothus and Sacadas—and since Pratinas, Sophocles, Ion and Euripides are in, it is hard to see why Phrynichus should be out. The Anacreontea are evidently excluded for lateness, and Erinna presumably because she wrote in hexameters; both of these exclusions can be defended according to the strict interpretation of Page's editorial principles, but there is an equity in these matters as well (witness the case of Corinna), and if we want to consult these works where are we to find them?

I have studied this expensive book with very mixed feelings. On the credit side, one must put down first of all the very fact of its existence: this is a remarkable feat of editorship and a credit to the courage of the publishers and to the skill of their compositors and readers (but not to that of their designers: why is not the format identical with that of Poetarum Lesbiorum Fragmenta?). Next I should put Page's full and helpful reporting of the contexts of quotation-fragments; the student can now see at once why the fragment was quoted, and can derive such assistance from that fact as it may provide ("even negative information may be of the highest value"). Thirdly, the very complete index verborum greatly simplifies reference to individual fragments (provided, that is, that one knows at least part of the text), and must have already proved enormously helpful to lexicographers and other 'harmless drudges'. Fourthly, and I fear lastly, I should put the very helpful thought by which each fragment is indicated by a serial number printed in the left hand margin in bold type (the numbers run from 1 to 1045). On the debit side, I should put first the absence of a concordance with Bergk and Diehl; the references to these are given, where appropriate, at the head of the individual fragments, but there is no reverse table by which the possessor of a Bergk or Diehl fragment-number can easily find the text which he wants—and when he does find it (and this is black mark the second) it may prove to have been changed in wording almost beyond recognition and pierced with more daggers than was Duncan's body. Indeed Page's treatment of the texts is so drastic, and at the same time often so unreliable
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(witness his treatment of the papyrus fragments of Alcman, where he makes arbitrary changes in dialectal forms, giving an a priori ‘grammarian’s’ form in the text and either putting the transmitted form in the apparatus criticus instead of vice versa, as in his treatment of fr. 1, or omitting it altogether, as in fr. 3.62 óávato, where the scribe certainly wrote ἄνων, as any one who consults the editio princeps carefully can see), that the casual consultor of this book is likely to be misled almost as often as he is helped, and the serious student will have to spend a great deal of time in collating the information offered by Page with that given by his sources. If, as I have suggested elsewhere, the main virtue of a collection of fragments is to act as a signpost to the sources, Page’s work will pass muster; but it could have been more trustworthy than it is and less bulky. Much of this bulk comes from the pages in which the editor has reproduced all the pathetic debris from the papyrologists’ publications. No doubt he put in all these dots and brackets meliora expectantia; but the user of such a volume as this is unlikely to have any means of using this sort of information intelligently, and it would have sufficed to refer inquirers after the fragmenta minuissima to the appropriate papyrological literature. Lastly, Page has followed Bergk and Diehl in omitting testimonia in spite of the excellent example set in this field by Edmonds (in Lyra Graeca) and in others by Lasserre (in the Budé Archilochus) and more remotely by Diels-Kranz and Jacoby. The fact that we still cannot dispense with Edmonds is a sufficient measure of the ultimate inadequacy of Page’s editorial performance.

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The first edition of this book was published in 1927. Professor Webster has brought it up to date by addition and deletion, a delicate task which in some passages is impossible without contradicting or at least undermining the arguments of the original (e.g. pp. 60 [on the date of Supplikas], 103, 159, 167, 177) but in general has been done with masterly judgment of just what to insert and where to insert it. The major additions consist of archaeological evidence, including some Minoan and Mycenaean representations (I miss here, in connection with animal disguises, a reference to the procession of ass-headed men in the Mycenaean fresco, Ath. Nat. Mus. 2665), and papyri of Epicharmus. Pickard-Cambridge’s very detailed criticisms of the speculations of Cornford, Rigdeway and others are rightly curtailed, and many of his quotations from minor poets are omitted. If the book is to be regarded solely as an enquiry into the origins of drama, the section on the dithyramb ought perhaps to have been more drastically reduced; if, however, it is a study of a complex of related genres, a case could be made for retention of the paragraphs on the Hellenistic dithyramb. All quotations are given in the body of the book in English, and a selection of them in Greek in an appendix. I am sure that this is right; there is no good reason why books on the history of Greek literature should not be readable by students who may know three or four languages but not Greek.

Pickard-Cambridge had a sceptical and positivistic temperament, and many of Webster’s additions are in the same vein, e.g. p. 196, on the possible Attic influences on Spartan deikellistai; p. 199, on the irrelevance of the Syracuse performances described by Schol. Theocr. (it is a pity, from the point of view of literary history in general, that Pickard-Cambridge’s error in calling these performers βουκολικαται still goes uncorrected); p. 167, on the Ortheia masks and ugly old women in comedy; p. 171, an important reminder that we must assume close cultural interconnection between Greek states in the sixth century B.C.; p. 186, on the dating of ‘Megarian comedy’; pp. 257, 264 ff., 277, rightly questioning the inference that three speakers participate in some scenes of Epicharmus.

There is, however, one process, of increasing importance in the discussion of literary origins, in which greater scepticism and a more rigorous separation of different kinds of evidence would be salutary: the process by which (i) the archaeological evidence is interpreted (and the phenomena which it presents named and classified) in the light of the literary evidence, and (ii) the literary evidence, which necessarily contains a high proportion of fiction and error, interpreted in more detail in the light of the archaeological interpretations. There is some danger for all but the most painstaking reader in such statements as (p. 96) ‘Dioscorides seems to have known that in a play of Thespis Dionysus led a chorus of satyrs or fat men’ (what Dioscorides says [A.P. vii 410,3] is δίκης ὑπὲρ τριθησθείσης κατάγονος χοροῖς) or (p. 129) ‘Arion’s dithyramb in the late seventh century increased the range of subject-matter and developed the part of the chorus as distinct from the execharon. It was probably sung by fat men and hairy satyrs, and their likeness to goats’—i.e. the likeness of hairy satyrs to goats, and the extension of the name of one species of satyr to another species of the same genus (pp. 114 ff.)—‘gave it the name “tragic”’: these performances were known to the Athenian Solon’. It seems to me that Webster gives too great a prominence to Arion, on the strength of Johannes Diaconus, τῆς δὲ τραγῳδίας πρῶτον ὑμῆν Ἀριων ὁ Μηθύμναιος εἰσήγησε, ὀπίσθεν Σόλον ἐν ταῖς ἐπιγραφομέναις Ἑλληνίδαις ἔδιδας. Surely if Solon had really used any word derived from τραγός (Webster [p. 113] is sure that he did) someone earlier than Johannes—Aristotle, for example—would have spoken up for Arion against the claims constantly made, and accepted, for Thespis? And would Herodotus (i 23) have called Arion the inventor of the dithyramb?

Some of the miscellaneous comments which follow
refer to the additions, some to the original text. P. 61: if it was really an innovation of Aeschylus to begin a play with the entry of the chorus, Ar. *Ach. 11* cannot be the traditional formula which it appears to be. P. 84: is not the reference to Thespis in *Wasps* 1478 f. simply a familiar element in the Aristophanic treatment of old men? P. 85: ought we seriously to consider the possibility that Thespis's language was 'rude and grotesque' when the language of the lyric poets of his time and of earlier times was anything but that? P. 181: the 'Megarian trick' of *Ach. 738* may reflect an Athenian belief that Megarians were cheats (cf. 770–773), not a use of disguise in Megarian plays. Pp. 157 f.: 'improvisation' is a hallowed but misleading word; people may be funny in public without having a written text by heart, but do they not give any previous thought to what they are going to say and do? P. 185: the suggestion that Susarion was from Megara but lived in Attica may be 'an obvious resource of the reconciler', and reconcilers may be as *spiacentantia* a *Dios* as political trimmers, but they are neither more nor less likely to be right than anyone else.

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Dr Patzer offers a simple and unified picture of tragedy as heroic conflict conducted within a frame of religious belief; it arises out of dithyramb and develops along with, though independent of, the satyr-play without fundamental change even in the fourth century. But this account is open to serious objections, and Patzer is no more able than anyone else to combine all the evidence into a coherent whole.

Patzer's sheet-anchor is the notice on Arion in the *Suda*; that Arion formalised the dithyramb is confirmed by Herodotus, that he introduced something that could be called tragedy, by Solon. Because these two statements can be taken as reliable Patzer is confident, though it does not necessarily follow, that we can believe the third statement, namely that Arion introduced satyrs *ēmētetai léorgōtas*, the satyrs being identified as the 'Dickbäuche' of contemporary Corinthian vases. The change Arion is alleged to have made was to give choric song to dancers who had previously performed in pantomime. But whoever wrote the words *ēmētetai léorgōtas* must be presumed to have meant by them 'speaking verse', which cannot on any accepted view be true at this date. The words of the *Suda* may be due to a misunderstanding of a source who said what Patzer wants him to have said, but they cannot be made naturally to refer to choric song, and the value of the statement is dubious. Given that Arion made these innovations, doubtless as part of Periander's policy of encouraging the popular Dionysiac cult, the theory is put forward that Arion's tragedy was a combination of the mimetic element from the old satyr dancers and the element of hero-saga from his new narrative dithyramb. This dithyramb was assimilated to the pattern of the seventh-century hymn in which, as Patzer sees it, the hero-myth which was the main content was 'theologisches Paradeigma' revealing the nature of the divine order; as the divine was seen as one, it mattered very little if one god rather than another was a figure in the story, and οὗτος πρὸς τῶν Δίωντων had no relevance.

Thus in the early sixth century the cult of Dionysus was supported by three types of performance; dithyramb and tragedy were alike in that both offered serious treatment of mythical subjects, but tragedy, unlike dithyramb, was mimetic. It is not indicated how a chorus renders a myth mimetically, but there is a hint of sung dialogue. Tragedy and satyr-play were alike in that both were mimetic, but the satyr chorus gave ludicrous performances while tragedy was serious from the start. In the course of the century all three forms spread to Attica where Peisistratus followed a policy similar to Periander's. There the tragic chorus was reinforced by an actor. The innovation was due to someone bred in the Ionic-Attic tradition of spoken verse, though, for a reason which will appear, Patzer does not think he was called Thespis. Following the example of tragedy the satyr chorus too acquired an actor, a change which may be connected with the name of Pratinas and with the proliferation about 520 b.c. of satyrs on vases in scenes which suggest heroic burlesque, though this is an early date for Pratinas. Thereafter the formal development of tragedy is approximately known; its content remained essentially unchanged, the heroic conflict whose resolution vindicates the divine order. Even in Euripides *Tyché* is not a force which excludes providence; it is providence working in a manner beyond man's comprehension.

This is a tidy scheme, but there are difficulties. The relation between Arion's tragic choruses and those of Adrastus at Sicyon is obscure. It seems to be assumed that Adrastus' chorus had been borrowed from Corinth, which gives good sense to Herodotus' *ἀπέδοκε* 'gave them back to their natural owner', but one gets an impression that the cult at Sicyon had been practised for some time. Indeed on p. 20 we read that Adrastus had been thus honoured 'seit alters', but if so, how can the tragic chorus be the invention of Arion? Further Patzer has not only to treat Aristotle's account of tragic origins as an unfounded hypothesis, which it may be, but to suppose that Aristotle ignored, or was unaware of, crucial evidence for the early history of tragedy.

Patzer's belief is that Aristotle had no firm evidence about tragedy before Phrynichus and that he reached his conclusions by a process of extrapolation from tragedies of the fifth and fourth centuries. Tragedy
belonged to the cult of Dionysus, so it is taken back to his earliest cult song, the dithyramb; before the three actors of Sophocles, and the two of Aeschylus, there must have been only one, the exorchon; as the Phoenissae is longer than the Persae, so the Persae must be longer that the original μυϊκοὶ μεθούς. The derivation from τὸ σατυρικὸν is ingeniously explained by Aristotle’s mistaken notion that the trochaic tetrameter is intrinsically less dignified than the iambic trimeter and associated with dancing, a notion which may have seemed to be supported by the traditional connection between tragedy and satyr-play. It is maintained that it was only the diction that was ever ludicrous (a lacuna is inserted with Gudeman after μεθούς at 1449a 19), though the question how ludicrous diction could be appropriate to any but ludicrous matter is not clearly answered. Further it is asserted that Thespis belongs only to the Alexandrian reconstruction of the tragic development: had Aristotle accepted the tradition that Thespis was the first tragic actor, he would have regarded the fact as too important to be omitted. Yet in view of the scrappy nature of the Poetics it is bold to ignore the positive statement of Themistius. After all, if Patzer is right, Arion is a figure of crucial importance in the development of tragedy, and he is known to have been mentioned by Aristotle in connection with the dithyramb, yet he has no place in the Poetics. And it cannot be argued that the account in the Poetics is schematic rather than historical, because no one composing such a scheme would include a satyric stage in the development of a form whose main characteristic was seriousness.

All theories of tragic origins discount or evade some evidence which deserves prima facie to be given weight, and Patzer is no exception. What is attractive in his view is that it accounts for the close connection between satyr-play and tragedy without encountering the difficulties involved in deriving one from the other.

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This is the third of a series of studies, of which the object, says Professor Webster (Lustrum vi (1961), 28) ‘is to provide a conveniently classified list, with firm dates and conjectural dates clearly distinguished; a brief history of staging, costume and masks is prefixed’. The task is being completed, if the present reviewer dare say so, with devotion and accuracy; and future historians, not only of Greek vase painting but of Greek theatrical production, will be greatly helped. General students should consult these lists with reference to the author’s earlier Greek Theatre Production; and they might have been grateful for an index of proper names. Recently my attention was called to Jebb’s statement on Antigone 438 that the Guard was ‘a δοῦλος of the family’, and also to Hypispyle’s apparent description of herself as δεσμια (Page, Greek Literary Papyri, p. 98); it seemed to me that Webster’s T V 3 and 8 might possibly be regarded as relevant; and, though I found them easily enough while reading through, an index is the natural tool for such a purpose.

I have only a very few criticisms. I am bound to construe the statement on p. 3, that ‘a stage building with ekkyklema must be assumed for Persae, Eumenides, Ajax etc.’, as meaning that the existence of the ekkyklema at the time of these first productions is certain—despite an excellent article by Professor Dale (Wiener Studien lxix (1956), 81 ff.), ‘fairly probable, is the highest I can go. And I do not think it is even “possible” that AV 13 illustrates Persae; and shall not until I have evidence that a burning pyre could represent the tomb of a king six years dead. Professor Page’s interpretation (Proc. Camb. Phil. Soc., 1962, 47) is at any rate not open to such crippling objections. Concerning the aedulia on fourth-century vases, it might be asked if it is not more likely to be a painter’s convention than evidence for actual staging; any columned projection from the σκηνή would surely tend to obstruct the view. Can we say with any confidence that it was Astydamas who inspired the Antigone painting on T V 3 (p. 7)? The presumption in favour of Euripides can be sustained if we postulate a first intervention by Herakles after the famous burial. Why has the satyr Eumas pretending to be a sphinx on T V 25 ‘presumably nothing to do with the satyr play”? Is it just that the odds are heavily against it? Is there any written evidence for the notion that actors may have worn more than one mask while playing the same part (p. 16; and cf. Greek Theatre Production, p. 50). In default, I should be inclined to say with Mr John Jones (On Aristotle and Greek Tragedy, p. 275 n.) that ‘those who assume that masks were occasionally changed in classical Greek Tragedy have failed, as I think, to be serious about masking’. The identification of P V 3 (p. 83) is altogether uncertain, to judge from Pickard-Cambridge’s photograph and comment, though I have not seen the actual vase. It is of course a virtue of such lists that they provide us with the opportunity to disagree, where necessary, with Professor Webster’s own conclusions; I congratulate him on the approaching conclusion of a task which would have been quite beyond my powers.

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There are, according to the author of this book, three great stages in human society: ‘the forest’, the urban community, and the modern city with its
worship of sheer mass, the calculus of expediency, ‘gigantismo’. In ‘the forest’ man knows himself only as a member of a kin-group. With this stage go compulsive ideas and practices, such as talio, belief in punishment by relentlessly avenging spirits (Erinnyes), and in the necessity for recurrent redemption through sacrifice. Man emerges from under these crushing and sombre forces into the next stage, when he betakes himself into the polis (but this is not the modern ‘city’, for it embraces, and shares a true community with a surrounding ager), lives there in a house (oikos), brings his religious cults and his courts out into the open, and above all renounces talio for logos.

This great development is depicted allegorically in the Oresteia. There the talio-principle is exchanged for reasoned justice (since the killing of Clytemnestra is finally justified, not as an inevitable step in the vendetta-sequence, but because she was qualitatively a criminal, who flouted the humane institution of marriage and the reasonable subordination of woman to man in the oikos, upon which the polis was based), the process is effected through the founding of the citizen-court of Areopagus, and the physical nature of the change from the old order to the new is signalised by the fact that the Erinnyes are persuaded to quit their habitation in limbo and settle in the city (to migrate, as Del Estal somewhat exaggeratedly puts it, ‘de la caverna Erinna a la casa de las Eumenides’) and to become spirits ‘of good-will’, co-operating in the maintenance of morale in the new urban community. All this has taken place according to a great predisposing principle of reason. The characters, struggling in the dark wood of their own psychological compulsions and predestinations, could not envisage this logos. In the end it was found to have been ‘with’ Zeus from the beginning. Zeus allowed the action to develop as it did, in order that he might eventually put an end to the law of talio. The community based upon logos—responsible reason, reason which recognises and depends upon a principle of high authority, such as the will of Zeus in the Oresteia—is the height of civilisation; and it is made possible by man’s belonging to an oikos, a condition which enhances his sense of personal identity, and equips him to partake in a reasoned dialogos with other, similarly-conditioned human beings, sharing in the great ‘simposio urbano’ which constitutes the polis. In the modern deterioration of society, which set in sometime in the eighteenth century, man has in large measure quit the oikos for ‘the street’, has thus become depersonalised, and has reverted, in desperation, to the primitive intuitions of ‘the forest’.

The reviewer hopes that he has not misrepresented a theme of such complication, and that he has done justice to its gravity, as well as to the admirably serious purpose of its author. Now he would like to make two criticisms. The first is that to which generalising interpretations of history most commonly tend to be prone: that they do not always rest upon a sufficiently hard basis of fact. Del Estal, for instance, has an interesting discussion of the catharsis passage in Aristotle’s Poetics, but reveals that he has no real idea of the problems of the text and construe of Aristotle’s sentence (p. 43). By a curious perversion of language, he makes Aeschylus, Sophocles, Euripides, Aristophanes, Socrates all into ‘aristocrats’ (p. 65; but he is unaware of any challenge to the story that Euripides’ mother was a grocer). He believes that sexual union with the god was, in ancient belief, an integral part of the gift of prophecy, and, in consequence, gives Apollo’s love-relationship with Cassandra an importance far beyond anything warranted by the text of the Agamemnon (the sexual theory of prophecy makes a good story, a good ‘significance’; though a sober and objective study of a prophetess in action, such as that by Amandry of the Python, does not bear it out). The abbreviated translation of the Oresteia which is printed at the end of the book seemed reasonably accurate, but I felt uncomfortable with some of the author’s paraphrases in the text. On page eighty some lines are attributed to Cho. 760 ff., which do not occur there (nor anywhere else, I think, in the trilogy.)

My second criticism concerns the general thesis of the book. There is a great deal in Del Estal’s contention that the family-household was the basis of civilisation both in antiquity and in more recent times, and that modern civilisation has suffered from certain incommodities into this organism (for an English treatment of the same theme cf. Mr Peter Laslett in the Listener LXIII No. 1619 (1960) 607 ff.). But surely that is not the whole story. There were also other (more collective) organs of civilisation, such as the school, the public gymnasia, the agora, the ecclesia, the Roman forum. (And what forces created majority-decision, and the rule of law?) In so far as he refers to such things, Del Estal implies that they were just functions of the oikos. That seems to me one-sided, and to be not so much a reasoned judgment as the expression of a deeply-rooted prejudice against any type of collective organisation as such. It is a bias that should be allowed for, before one accepts what he says either on Aeschylus or on modern society.

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J. H. KELLS.


This is excellent—the product of wide reading and acute critical judgment. It represents such an advance on previous English editions that one regrets any factor which may militate against its being widely bought and consulted. It is rather expensive; and, although one appreciates that it could hardly be cheapened to serve a wider market without impairing its thoroughness, I think it contains quite a lot we could do without. B. includes a large number of his own emendations, six in the text and many more
mentioned in the *appendix criticus* and commentary as having a ‘suggestive value’. I should have been inclined to place the former in the *appendix criticus* (at 483 κριναίον γιανόης could be right; but the combination of ἀμφι and ποιοίστες seems disagreeably pleonastic, and B. does not explain what is wrong with the tradition), and omit altogether a high proportion of the latter with attendant exegesis (at 233 περίδοιμον is inferior to the ms. θρόνοις, which suits the idea of a distant prey particularly well). There are other things which could perhaps be pruned: excessive quotation will be found, for example, on 114 (Ag. 160–1) and 135 (e 231).

B.'s introduction deals with *Persae*’s tragic qualities, dramatic techniques, original production and textual history. He argues strongly that Aeschylus conceived his theme as the tragic downfall of a great empire, and he reinforces this conclusion by neutralising various passages which have been held to betray a more satirical approach. Conspicuous among these is the end of the Darius scene, where a quite disproportionate interest is shown in Xerxes’ torn robes. Here I would go further than B.; I cannot doubt that, as Hermann succinctly maintained, it arises from Aeschylus’ need to present Xerxes on stage properly clad—in *Frogs* he censures Euripides for doing otherwise.

B., I think, has correctly assessed the general tone of the play, though I cannot avoid the suspicion that Aeschylus may have been less single-minded than he supposes, and Blomfield’s judgment (quoted) may not be far from the truth. The rest of the introduction is sound; perhaps B. takes some speculations about the tetralogy too seriously. Murray’s theory of a *σαραγος* of Masistios is absurd; the body, far from being cut to pieces, was carried around and admired for its beauty; and there is not the least likelihood that the skirmish covered Potniae, which lay north of the Asopus.

The commentary is full and penetrating; from an impressive bibliography two notable absentees are Maas’ *Griechische Metrik* and Kumanicki’s *De Elucizations Aeschylii Natura*. Outright mistakes are commendably few—33. Blomfield favoured ‘multos alens’, but naturally conceded *πολλόκεφαλον* as support for ‘beluis scacentis’. 190–2, πόρον ἁμείθαν (I.A. 144) has nothing to do with crossing water. 185, M has ἀμοῦρον; B. has been misled by a comma. 406–7, ἐπεράζοντες governs the dative. 492–3, Pytheas was not killed; he eventually returned to Aegina (Hdt. viii. 92). 607–10 Iphigenia made libation to the shade of Orestes; for ‘Oedipus’, of course, read ‘Agamemnon’. 691–3 what is said in *Choephoroi* is that Agamemnon would have been κατα θρόνος σεμνότατος ἀνδρὸς if he had been killed in action. If we look hard enough, we shall find misprints like ‘infortunium’, ἅζε (Ion 498), γνακώτισι κ. and ‘Danaidai’.

The following occur to me as I read:—3–4, refer to Pearson’s note (C.Q. xxii. 180) on gold exciting divine jealousy. 162, ἐπινικίς may contrast with ἀς ἔμας (which is more emphatic than ἕμιδ): ‘I will tell the tale to you (representing the people), being not unalarmed for myself (representing the royal house).’ 220, comma or no, we have in effect a prayer to Darius, which B. denies at the cost of making φιλῶν in 229 weirdly proleptic. 235–6, the connexion of thought would be a little less obscure if we interpreted ἀποθεσθείη in the light of ἀνέχεσθε ἵστε, and referred it to the quality of the Athenian army. 321, the introduction of ἄρθρος should be referred to Porson, whose treatment will be found in Blomfield’s commentary. 339–40, if the fleets numbered 900 and 1000 ships, we must assume a mistake in the Hypothesis, a misunderstanding in Herodotus and an irrelevance in Aeschylus, who is concerned to show that the Persian fleet was *numerically* superior. 553, it should be noted that M’s accentuation supports the later Mss. 454–7, Heath’s rendering surely is what we require: perhaps read Ἀρείος μένων οὐ. 795 ff. B. misunderstands this passage. The messenger told the Chorus what he believed to be true, that all the survivors had returned (or were returning; I doubt if the tense is important); now, when they suggest a further attempt upon Greece with a select force, they learn that just such a force has been left behind. Why then, they ask, is not the whole army (i.e. all that survives of the original expedition) recrossing the Hellespont? Very few, replies Darius; events have so corroborated my oracles that I am emboldened to assert (a common gambit in prophecy, Ag. 1194. P.V. 824, as in medicine, Hippoc. *Prognosticon*) that, led on by illusory hopes, Xerxes is leaving behind a picked force. Failure to see that in 803 ff., Darius is talking about the present, not the future, causes B. to construe the couplet in defiance of the word-order. 911–14, Headlam surprises B., but he has seen the essential truth that what afflicts Xerxes is not shame in the presence of the bereaved but horror at the decrepitude of his remaining subjects. If this is understood, we can relate τί πιθέο; to what precedes without any real difficulty, for the sight of the aged burghers is symbolic of the disaster.

Appendix I consists of *Supplementary Notes to the Commentary* and contains much of value, along with a certain amount (particularly in defence of emendations) which could be dispensed with. Appendix II is on *Metre*. Appendix III is on *Neocomyancy*. B. rejects Headlam’s view that magic was employed in the evocation scene; but *Frogs* 1028 ff., which are almost certainly a description of it, seem to me to tell strongly in Headlam’s favour. That the Chorus immediately clapped their hands and cried ‘Iaioi’ suggests a set magic formula rather than random signs of distress attendant upon libations and prayers, and in 1029 Mr Gow’s οἰονίς *φελετορ* (though far from certain, has at least this merit, that, if misread as οἰονίς *φελετορ*, it would account for the violent corruption of the text. The other Appendices, dealing with types of *Kommas*, with Persian *Names* and with the scene of the battle of Salamis are judicious.

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This detailed and scholarly edition of the fragmentary Hypsipyle is a most distinguished addition to the Oxford Monographs series, and scholars will rejoice at its appearance. Not only is the commentary packed with information on Euripidean usage, Greek mythology, discussions by other scholars on a multitude of points, but the whole structure of the play is examined afresh, with important new conclusions on the sequence of fragments. These are mainly based, as Mr Bond gratefully emphasizes, on the expertise of Dr Barns in the evidence of fibre-correspondence; it is clear, however, that he has been at pains to equip himself to make reasonable judgments on all matters of papyrology. In particular he has done more careful work on the document found on the recto. For the actual decipherment, as others have found already, there is little to modify in the extraordinarily accurate work of Grenfell and Hunt.

For the inexpert it is not very profitable to say 'I don't believe it,' and to proceed to argue from probabilities based on the text alone, especially where crucial passages are only present in fragmentary half-lines. Reconstructions on that basis are already available, and are discussed in the commentary. The best thing is to admit the new order and consider whether some adjustment of our ideas can make it acceptable. Of the three new collocations given by the fibres (p. 2) numbers (ii) and (iii), even if at first unfamiliar, need not cause much difficulty. Much the hardest to take is (i), the lining up of fr. 61, 70 + 96, and fr. 71 in three adjacent columns in that order, near the beginning of the play. We know the three opening lines, starting a monologue in the genealogical style; further, H. Lloyd Jones in an Addendum has made out a strong case for adding another 15 + lines of further genealogy from a Hamburg papyrus. In fr. 61 we find the speaker (clearly Hypsipyle) addressing the baby and sighing over her lost sons in much more intimate and emotional style; and the position of this fragment leaves room for only 14 preceding lines at the top of the column. To make more room for the transition Mr Bond postulates 10–15 lines at the bottom of a preceding column, following on title, hypothesis and dramatis persona. But now that the Hamburg papyrus (a last-minute addition to the problem) has shown the extreme unreliance of the opening account one would really like a good deal more space to play with—which may, however, be difficult to reconcile with the stichometric numbers. In any case the prologue must be of unusual length—by no means impossible in a play of at least 1700 lines—and the opening monologue must divide into two sections, one bald, one containing an address to a baby. (Fr. 70 seems to return to something in between but is too scanty for safe interpretation.) Since Hypsipyle retires into the house during the next scene and re-emerges, with the baby, to talk to the youths and then dance to the castanets and sing, it had been supposed that the monologue was likely to be sans baby, but this is not serious.

More worrying is fr. 61, which anyone would supplement at sight as τοιούθεν ἐγὼς νεανίς, and naturally refer to the sons—who on this place have not yet appeared. Mr Bond proposes to take this as = 'these sons I speak of', or even 'my sons'. There are two objections here: no mother would refer to long lost babes as 'youths' (Ion 57 is no parallel) and τοιοῦθεν can mean neither 'these I am speaking of' (which would be τοιοῦτος) nor 'nostros' (a will o’ the wisp of H. Hunger, WS 1950). There is a growing tendency to say 'it is nonsense to claim that δεῖ must refer to persons on the stage' and think that the end of the matter. Of course they need not be on the stage, but the pronoun is deictic, evolved by a people used to talking with their hands, and there must be some reason for a gesture of immediacy. The great majority of apparent exceptions refer to people 'in the skene here' at the time of speaking, or as in Cyc. 30 τῶν δυσσεβίβλων Κύκλωπων to one who normally lives here. In Sept. when the enemy champions are so indicated the Messenger on the citadel is pointing down to each gate in turn as a way of actualising the scene for an audience deprived of 'scenery'. (For the home-team see Fraenkel, Die Sieben Redepaare, SBAW 1957 on 408 and 473—which he is not arguing about the use of δεῖ.) Others, said by Hunter to be 'persons referred to in recent words', or 'noster' are special cases of people deliberately brought into the presence, as it were: Hel. 98 ff. 'You know Achilles? 'Yes, Helen's suitor.' 'Well, he—' cf. Or. 1183 ff., again in lively stichomythia. Or contrasted with one other: E. E. 646 ἐκείνη τῶν τινος τ' ἐν ταύτῃ κτισμῷ. All the regular cases and the 'exceptions' are demonstrably within the same order of phenomena (IT 558 and HBl. 793 have long been seen to need emendation).

Here the best I can do is to suggest self-apostrophe—'whenever I see a noble-looking pair [graphically described] I think "but for your sad loss τοιοῦθεν ἐγὼς νεανίς—"'. It is not very happy, and doubtless someone else will do better, but Dr Barns has set us an awkward problem.

There is space for little more. Fr. 764 Ν ἐξαμελοῦμα: ἀμιλλόμα, or ἐγεμ. is used of fast running (not 'hurried or toilsome bodily function'), usually competitive, like our 'race', whether literal or (frequently) metaphorical. ἐγεμ. has also a transitive sense, Or. 38, 431, 'keep him on the run', 'run you out of', to which the passive of Cyc. 628 also belongs—his sight is to be 'chased out of' his eye. This is irrelevant here: not 'strain your eyeballs out of their sockets', but 'race with your eyeballs'—which of us can first decipher? I would emend not as Nauck but as Musgrave + Valckenara, κόραισι and προσβλέπειν. Fr. 20/21, 8: the note is puzzling; ἐνεργός refers to Hypsipyle, not the Chorus. Fr. 34/35: what, on Mr Bond's theory, becomes of οὐσε; Surely, with ὅς and ὁμοι, it must imply a feminine participle? Perhaps
spoken by Eurydice inside ὥς ἂν ἔξως δὴν ἔγιν.—, then emerging: τί δήτ' ἔτει ἔξω—φωτόν δόθων, 'Why is the nurse not back yet, letting the child run about outside', etc.

In the parodos I ii 35 cannot be dactylic, even with a question-mark; why not resolved aoeic, as also I i 8 = I iii 9 (rather than iambic, cf. Bach. 903, 905 which are not 'trochaic')? And at 64, 77 Wilamowitz's τέκτην should certainly be read (as 73) because the line is not 'anap. dim.' but that sort of dragged enoplon where the opening double-short is part of the concept (cf. 81). The speaking son throughout this exchange is Thoas (as in fr. I i) and 69 is spoken by Hyspisyple. No matter if the papyrus gave 69 and 70 to the two sons; it is totally impossible for Hyspisyple to be so rude as to ignore Amphiarous' farewell without response.

A. M. DALE.

FRAENKEL (E.) Beobachtungen zu Aristophanes.
PP. 223. L. 3.000.

This latest product of Professor Fraenkel's long enthusiasm for Aristophanes consists of observations on some fifty individual passages, with two longer essays. Textual criticism is well represented. Not surprisingly there are few radically new emendations. In Lys. 899 εἰκα for εἰκή the confessably speculative restoration of Them. 242 πρὶς ἀντιλαμβάνομαι τῆς ὕρας, μην τίνι πλάγια is worthier of Ar. than previous attempts. For Plut. 689, noting that ΣΥ implies a compound of στερεῖ, Fr. proposes εὔσαρσε (sc. εἰς τήν χέριν). But would the old woman stick her hand into the pot to protect it? Sense cries out for συσσαρσεῖ, but metre forbids. Perhaps εὔσαρσε, closest to the transmitted στερεῖ, is worth consideration; 'she slips her hand underneath (me)' would fit the situation in which Carion is bending over the pot beside the recumbent woman.

Some old emendations are improved. In Them. 107 ἄλλες (Bentley) μοῦνα (Bergk) is convincing. Often new arguments are adduced in support of old solutions, whether MS readings or emendations. A favourite method is to demonstrate the formulaic character of a phrase; thus parallels for ἀδρά (ἀετών) ἰδιή from tragedy and prose, as well as comedy (Them. 1189 should have been mentioned) support V in Ach. 393.

Fr. is as much concerned to interpret as to emend. Misunderstood idioms are elucidated, parody is spotted, and significant word order pointed out. The associations of certain phrases are explored. In Ap. 133, for example, μὴ ἄλλοις ποιεῖ is a formula of invitation of remarkable longevity. In Ach. 1175, however, I doubt whether ἅρπα ἅρπον would lead the audience to expect a fire; the context suggests that the iteration is paratragic. In Them. 384 the phrase σίγα σιγά, πρόσεχε τὸν νόημ is part of the stock in trade of heralds, although the line must be attributed, after Fritzsche, to the corypheus and not to the 'Heraldess' who spoke the preceding lines; Fr. does not consider the possibility that the two may be identical, as suggested by Van Dale in the Bude translation, p. 31 n. 3, and now by C. F. Russo, Ar. Autore di Teatro 302 f.

Them. 381 leads incidentally to illuminating remarks on Ennius as a translator. Fr. serves as a starting-point for other more general observations. In Ap. 446 ἄρσιν ταῦτα fulfils its original function, the solemn confirmation of a prophecy or wish; in tragedy however and the later dialogues of Plato it is adopted as an alternative to the prosaic ποιῶν ταῦτα, merely to express compliance with a command or request. Ap. 99 prompts the hypothesis that ὡς ἦν, ὡς ἦν have the same predilection as the enclitic pronouns for the second position in the sentence. Ach. 206 f. and 239 f. illustrate Ar.'s anticipation of New Comedy motifs. There seems to me, however, an essential difference between the opening of Lyncestes and of Menander's Eunuchus: in the one ἀλλά marks the continuation of thoughts unspoken but expressed in gestures visible to the audience, in the other of a conversation supposedly begun offstage.

The chapter on 'The Structure of Frogs' attributes the play's structural peculiarities not to any change of plan or revision by Ar., but to his desire to prepare the groundlings for a sophisticated literary contest. In 'The Parabasis Odes' Fr. observes that the earliest odes mostly resemble cult hymns in their content and simple metres, whereas the later ones adopt (and parody) the style of serious choral lyric. The first was probably the traditional form, the second perhaps an innovation of the musically gifted Ar.

The two final chapters in particular contain eloquent tributes to the genius of Ar., comic dramatist and poet. The author's sympathy with his subject is however everywhere evident in many a penetrating comment and in the zest with which he writes. He both instructs and inspires, the hallmark of a great teacher.

The book is well indexed and handsomely produced; I have noticed very few misprints.

J. C. B. LOWE.

RUSSO (C. F.) Aristofane autore di teatro.
Price not stated.

We all know that plays have to be approached with a willing suspension of disbelief, but we do not generally expect books about plays to require the same credulous attitude. Unfortunately Russo's new book on Aristophanes requires it rather too often.

His aim is to show us the theatrical qualities of the plays and to discuss how they were performed. His first chapter, on 'the two theatres of Aristophanes', is a reprint with only minor changes of an article published several years ago (in RAL VIII xi [1956] 14–27) in support of the view that at the Lenaian
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festival plays were still in the time of Aristophanes performed at the Lenaion, not in the theatre of Dionysos at the akropolis. This view, though damned by Pickard-Cambridge, has had some distinguished supporters ranging from Dörpfeld to Bieber, and it deserves attention; the existence of texts like Ar. Amb. 504 and Pl. Prt. 327d, containing the expression ἐν Αντίπατρο, clearly puts the onus of proof on its opponents. But Russo's arguments do not contribute much of value to the discussion. For example, even if it is true that the Lenaiai plays do, and the Dionysian plays do not, contain expressions like 'come up' suggesting that an actor steps up from one level to another, that does not prove that the Lenaiai plays were performed in a different theatre unless it is shown thatler theatre of Dionysos lacked not only a raised stage but even steps. Again, Russo maintains that in the Lenaiai plays the scene is always just private houses, whereas in the Dionysian plays it is often more exotic; I should have thought few plays would require more elaborate scenery than the journey to hell in the Lenaiai Fros— if realistic scenery was ever used at all, but of course it was probably not, and so Russo's argument goes for nothing. Some of the arguments in this chapter are better than these two, and the original article was no doubt worth publishing, but it was hardly worth this reprint.

Of the next twelve chapters one deals with the chronology of the early lost plays and one with each of the eleven survivors. They have some praiseworthy qualities. Russo's writing is lucid and lively, and occasionally witty. He is quite right to stress the importance of the details of stage performance, and the fact that more is to be learnt about the performance from the text of the plays themselves than from external evidence. He constantly asks the right questions. The trouble is that he often goes on to give the wrong answers.

Sometimes the reason why he goes wrong is that he lets his imagination run on beyond anything for which there is evidence in the text. For instance, he soon makes up his mind that there were only three main actors in each play (a view untenable for comedy, in my opinion), and then proceeds to say exactly which parts were played by which actor; though there is of course no evidence whatsoever to show that (say) Xanthias in the Fros was played by the same actor as Aeschylus rather than Euripides, or that Dionysos was played by the protagonist.

But sometimes he actually misreports or misinterprets evidence which does exist. To take examples from a single section, about the house in the Wasps (pp. 202-4): the roof is not covered by a net; Bdelykleon does not manoeuvre from below by strings the net covering the window, since the net in 208 is a birdcatcher's net; Philokleon does not remove the chimney-cover in 143-7; 211 shows not that Xanthias was barricading the door during 206-10, but that he was shooing Philokleon; it is not natural for the door of Philokleon's prison to open outwards, since the building was not designed as a prison but as a private house; 1154 is not evidence that the scene is outside the house (how does it differ from orders given indoors?), and in 1251 the slave is not necessarily inside; 120 does not show that the house is in the centre of Athens, since we are not told where Philokleon's Korybantic initiation took place nor how far he ran to reach the New Court (does 122-4 prove that Aigina was in the centre of Athens?). When there are so many mistakes even in a typical couple of pages, one hesitates to say that the author has made an important contribution to the understanding of Aristophanes, despite the validity in principle of his general approach.

There is a final summarising chapter, which is followed (according to the Italian custom) by the table of contents. There, I regret to say, the book ends; the entire absence of an alphabetical index is inexcusable.

DOUGLAS M. MACDOWELL.

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Mr MacDowell offers us the first edition of De Mysteriis with English commentary since Marchant's edition of 1889. His text is based upon fresh examination of the difficult Crippsianus. After carefully describing the different coloured inks used by original copyist and correctors, he demonstrates that the readings of A are not conjectures (as Blass, Fuhr and Dalmeyda believed), but corrections made with reference to the archetype. Following out this line of thought, and abating from the nineteenth century penchant for normalisation, he arrives at a text which justifiably keeps closer to the MS than the Teubner and Budé editions. Original emendations are 61 εἰδέατα for εἴδεατα and 127 Καλλίϕορος for Καλλίλος.

Besides the Introduction and Commentary, this edition contains no fewer than sixteen appendices of 49 pages in all. The historical and legal background is well covered, with some attention to prosopography, e.g. a careful investigation of the different persons named Meletos, with suggestions as to their identification. Appendix C argues plausibly that Andocides had no part in the Hermokopia—indeed, that his elaborate defence against this charge is designed to divert attention from his real complicity in the profanation of the Mysteries.

Recent archaeological evidence bearing on our text is mentioned in the Commentary, with references to Hesperia; for example, at 13 the sales of property recorded on stelai of those condemned for profanation of the Mysteries and Hermokopia. That attractive book by Ida Thallon Hill, The Ancient City of Athens, is occasionally referred to, but a warning ought to have been given that it contains gravely erroneous statements about De Mysteriis. The bibliography
ought to include R. E. Wycherley, The Athenian Agora III, Testimonia, Princeton 1957 (which is used, however, for the commentary) and J. Travlos, Πολιτισμική Εξέλιξη των Αθηνών, Athens 1960. This indispensable work by Travlos will soon be published by the University of Chicago in English translation by Professor Wycherley. On the Eleusinian, Mr MacDowell's note at 110 is too brief, and refers to an out-of-date authority. Recent work makes it now possible to evoke the scene of Andocides' alleged offence with the suppliant bough: see especially Homer A. Thompson, 'Activities in the Athenian Agora: 1959,' Hesperia 29 (1960), pp. 327–368 'The Eleusinian,' pp. 334–338. On the Theseeon referred to at 45, Mr MacDowell rejects Professor Wycherley's suggestion that a Theseeon in the Long Walls is meant, but this suggested location for assembly fits the context better, which is the reason why it was made.

I cannot find any allusion in commentary and appendices to Isocrates 16, 6, which locates Pythomaios' denunciation of Alcibiades in the Council, not in τοῦ υἱοῦ 11 and 14 of our speech. This contradiction by a contemporary needs to be explained away.

Introduction VI. The Style of Andokides gives a careful account of our orator's peculiarities of language and expression, and shows that he stands at a transitional stage in the history of Greek oratory, being correctly described as a 'gentleman orator,' who apparently never made a serious study of rhetoric, but cannot have remained entirely uninfluenced by the new class of professional orators. Mr MacDowell goes some way towards correcting exaggerated notions of Andocides' naïveté: for example he dedicates Appendix P. Asyndeton to showing that Denniston's explanation of asyndeton in Andocides as 'naive awkwardness' is probably wrong. He proves in fact from numerous examples that it is used for rhetorical effect. But this discussion of style has weaknesses possibly due to compression. Having included rhetorical procedures under style, Mr MacDowell sometimes neglects to treat them technically. This can be seen most clearly on p. 22, where too much is explained as keeping the jury interested. For example, Kallias' love life appears as 'narrative', whereas technically it is ἠγοικότος. Similarly in the commentary at 19 ff. the argument is carefully summarised with no attention to the use of τοῦ. It is true that editors of ancient orators (Professor R. G. Austin is an honourable exception) seldom feel the urge to write rhetorical notes, but these are more important for Andocides in that we have only internal evidence for his possible rhetorical training.

Some remarks on the commentary may be made with reference to the paragraphs:

38. The note on ἄνωντρομοι may well give a wrong impression through failing to mention the significance of the new trend towards industrial slavery.

58. The syntactical note on ἀπώλειαν and the similar imperfects is over-Iaconic.

86. ἔσωμεν ὧν ὄνετ. The abnormal optative.
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uses in Hymn V. (p. 45). It would be more in keeping with the Callimachean approach to mythology and his ὕμνος ἑμοῦ ἀλθῆς, if this were a rare local version, which became known to later writers through Callimachus, and for that reason has not been recorded before his day. Mr McKay also puts forward the view that in Hymn V Callimachus is trying to revive the Doric Threnodic Elegy, and that this was the reason for the choice of elegiacs as the metre for that poem. This is by no means convincing, nor is Callimachus Hymn VI, 7 ff., which he offers in support of his view, of any help. The mixing of content and form, which traditionally belonged to different literary genres, is a well-known feature of the Alexandrian poets, who were driven to this in their desire to ‘innovate’. Just as in his Σωφρινή Νίκη (fr. 384 Pfeiffer) Callimachus is casting in elegiacs a ‘Pindaric’ ode, in Hymn V he is casting in elegiacs a ‘Homeric’ hymn, conceived, of course, in Alexandrian terms. There is no more desire to ‘revive’ the Doric Threnodic Elegy here, than there is to ‘revive’ Doric Comedy in Hymn VI, though allusions to both should not be excluded.

In connexion with what the author says about the ‘setting’ of Hymn VI, it is interesting to note that the Eryssichthon myth has survived to the present day in the island of Cos, as can be seen from the Coan folk-tale The Fairy’s Revenge, published by R. M. Dawkins in Forty-Five Stories from the Dodecanese (Cambridge 1950, pp. 334 f.). In spite of these objections the sensitive and detailed examination of Hymn V by Mr McKay will help the reader to grasp and enjoy many of the complexities and subtleties of the poem, and of Callimachean art in general.

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The Loeb Moralia has now progressed two-thirds of the way towards completion, and to judge by recent volumes one may be confident that the series will be completed in a satisfactory manner. These recent volumes, in fact, do much to compensate for some of the slipshod work of Loeb translators in earlier generations. In the present volume, comprising three independent sections, Quaestiones Convivales Books vii and viii are translated by Edwin L. Minar Jr. and Book ix by F. H. Sandbach; the Amatorius is by W. C. Helmbold.

Each of the translators has based his text generally on that of Hubert in the Teubner series, but Helmbold has also made use of Flacelière’s edition of the Amatorius, and for Book ix of the Quaestiones Sandbach has provided a valuable apparatus containing detailed reports of Renaissance conjectures and their sources. Only a few new conjectures are offered by the translators themselves. Minar’s γενναλιά, at 706E (codd. κάνων) is commendable, as are Sandbach’s ἐμοὶ ἔκτης (737A, codd. ἐκνήσθη), ἔκτος (739B, codd. ἐπολογία), μετάπιστος (743D, codd. μεγάλη εἰπότος), and his deletion of ἡ ὑθερία φαίνεται (746B) and the addition of λαμβάνει at 748A. On the other hand, Helmbold’s addition of μὲν at 765C and his είναι (767C, codd. μετὰ) are unnecessary; so too is his ἐταίρας (769D, codd. ἐταίρας), which is not adopted in the text—and the translation, ‘a rival’, does not make it clear which reading is actually being translated; and at 757D παῦσιν (codd. πά σιν) with a genitive object is hardly possible.

The translations are on the whole very well done. They have the merit of breaking up Plutarch’s often turgid periods into shorter units that are easier to read than the original, despite some cumbersome expressions here and there, e.g.: ‘permission is given to use as criteria . . .’, 697E, ‘is there not sometimes more impulsion for a man to say . . .’, 707E, ‘a number . . . who are no one in particular and the least bit rustic’, 710D; ‘My words seem to have amounted to a removal of inhibitions for the rest’, 728B; ‘He does not . . . smuggle himself in to reap a harvest of honours to which he has no right, which would make him liable to indictment for illegal registration as a god, and bastardy’ (!), 756C. A number of minor points may be questioned: ὃς θεός (697F) is not ‘on a summer evening’; ἣμερομνοικό (707B, 712A) means more than ‘important personages or the great’—it usually refers, in the Roman period, to Roman provincial governors and their staffs (Minar rightly points out the meaning of ἡμερομνοικός at 708B, p. 63, note a); περιφέρεια (720C) is not ‘sounded through’; άπανταπήμενοι (720D) appears to have been wrongly construed with τὴν φωνὴν—‘the voice . . . spreading out’; at 758A–B θέος . . . τῆς περὶ τοῦτο χρείαζαι καὶ δόναμεν εἰλεχθῆναι is not ‘there is a special god whose mission it is to bring help and strength at such a time’, but rather ‘the god to whom has been allotted this function and power’; οἱ περὶ Πεμπτιόν (760E) is probably a conventional Hellenistic periphrasis for ὁ Πεμπτιόν—hence not ‘Pemptides and his party’. There are some small but important omissions in the translation, e.g.: συνέχει (700F), γιαναρός (749E), οἱ μὲν οδός (758A–B), τρόπος τῶν 759E, τὰ στοιχεῖα (764A)—referring to the Muses at their shrine on Helicón—πλάκτορες (765A).

As in other volumes in the series, the translators have provided some useful footnotes. Sandbach’s information on the στρατηγία in Imperial times, p. 219 note b, might have included a reference to the other, perhaps more important, functions of the στρατηγός ἐπὶ τὰ ὀλίσθη Athens in Imperial times, and his references to IG could be modernised. In note a, pp. 366–7, Helmbold seems to suggest that what Strabo says about the condition of Bocota refers to the district in Plutarch’s day; such generalisations, however, contain only half the truth, and the social historian looks elsewhere for his most reliable evidence. Note e on p. 359 is perhaps not quite accurate:
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kai τά τοιαύτα refers only to death, birth being mentioned earlier in the paragraph. Note g on p. 389 is inaccurate: Sappho’s poem beginning φαίνεται μοι κήρος has four stanzas, and not ‘only three’. Finally, note i in the apparatus on p. 372 will be found to be misleading.

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This is a most useful volume, containing the Lives of Solon, Publicola, Themistocles and Camillus. The text, newly edited by the translators, is good and the translation readable. There is an introduction to each Life, in which Plutarch’s sources are discussed—this is particularly valuable in the case of the Roman Lives—and in addition to explanatory footnotes the volume contains notes complémentaires, an excellent feature of the Budé series.

In both text and notes the hand of Flacelière is conspicuous. It is mainly proper names that are corrected in the text (e.g. 101 B Ταρχίας Flac.: Ταρχίας codd.; 149 A Σάτρικον Flac.: Σατρίκον S., Σουρήδιον Γ.), but in places, perhaps inconsistently, the suggestions of other scholars are not accepted when it is felt that Plutarch himself and not simply the text is being corrected. Thus at 107 C εἰς Παλάτιον is retained (εἰς Καπνίδιον Ziegler; but the translation is inaccurate here), and at 118 E the θεωρία of the MSS. is preferred to Reiske’s θεωρία.

The translation is generally good, though sometimes loose and occasionally inaccurate (e.g. φωνή τις αὐτοῦ περιφερομένη πρότερον εἰσόδος; 85 D, is rendered as ‘le premier mot de lui qui se répandit dans le public’; at 87 F ‘le peuple qui jusque-là en était exclu’, for ζος οὖν δόμιον μετέχειν, appears to have translated Ziegler’s οὐκ εὐπρό, which the text does not accept; ‘le sort’ as a rendering of αἰτίαν in 94 B misrepresents both Solon and Plutarch; διαπλέωτας, 107 A, is not simply ‘qui naviguat sur la rivière’; at 107 C άνακτησαι δὲ τὴν ἑραίαν ὁδὸν περιφερομένως εἰς Παλάτιον αὐθαίρετος... is inaccurately rendered ‘on élève sur la Voie Sacrée, en vue du Palatin, une statue...’).

On the whole, the notes and introductory essays are excellent. But one may question the note on 91 F–92 A (Solon 24, 4, p. 213), where Plutarch’s ἀπόρριπτο refers to οὖν δόμιοι (i.e. the fact that Solon did not grant citizenship to anyone except the two classes of individuals mentioned here) and not to ‘la première catégorie’. Again, the statement on p. 142, ‘le plus souvent, il (sc. Plutarch) se montre entièrement crédule...’ is a harsh judgment. The example given in support of this statement is not, in any case, particularly convincing, since Plutarch qualifies his remarks about the statue of Juno (Camillus 6, 1, 132 A) with φαίνεται and concludes his discussion of divine marvels (ib. 6, 6, 132 C) with the observation: ἡ δ’ εὐλαβεία καὶ τὸ μυθὲν ἄγων ἄρτων.

There is a fair sprinkling of misprints, mainly of wrong accents and breathings in the Greek (including στράτος twice, p. 111 note 2, despite στρατός in the text), but nothing serious.

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The purpose of this essay is to examine the occurrence in Greek and Latin literature of what the authors call ‘the appositional mode of expression’, and to relate it to temporal experience. The term ‘apposition’ is used in a wide sense, including all circumstances in which a central theme is elaborated, while the sentence-structure remains paratactic. Since the authors are concerned with the aesthetic impressions obtained from style rather than purely grammatical considerations, there is little cause to take exception to the latitude of their definition.

The first two chapters deal, respectively, with the Greek and Latin authors whose styles are thought to be primarily appositional. The Greek authors examined are Homer, Hesiod, Parmenides, Empedocles, Pindar, Herodotus and Thucydides; the Latin authors are Ennius, Cato, Lucretius and Vergil. Attention is drawn to the variety and complexity which can arise within a fundamentally appositional framework. Homer is found to prefer to state the main theme of each sentence at its outset; and that this straightforwardness is also a characteristic of his treatment of subject-matter is shown by reference to his handling of the death of Hector, in which he makes no attempt to conceal the outcome until the end, but foreshadows it clearly. Pindar, although he is paratactic in his sentence-structure, uses the appositional style to generate suspense by the promotion of descriptive words and the postponement of words which are essential to the sense. Of the Roman authors, Lucretius is represented as having constructed his whole poem in an appositional style, Books 5 and 6 being ‘appositional expansions supporting and enlarging upon the main content of the work’ (pp. 63–4). It seems doubtful whether any useful analogy can be drawn between this kind of ‘apposition’ and structural apposition within sentences. Vergil is said to use apposition as only one of a variety of modes of expression. This statement might be applied equally to several great stylists whom the authors seem to regard as being outside their scope. It is also rather misleading to draw a chronological line between the ‘early’ and the ‘periodic’ mode of expression (p. 43), since authors like Xenophon and Lysias are at least as paratactic and appositional as Herodotus and Thucydides. Moreover, by concluding these two chapters with Thucydides and Vergil,
the authors leave unconsidered the question of rhetorical amplification and tautology, two characteristics of Attic oratory which reappear in Roman poets like Lucan and Ovid, who came under the influence of their own schools of declamation.

But there is much valuable literary criticism in these two chapters. In the crucial third chapters we pass from discussion of classical literature to the psychology of language. The main thesis is that speech and language unfold themselves in time, and that assimilation by the listener involves a process of memorisation, 'oblivescence' and recollection. It is suggested that the appositional style causes this process to function in a particular way: because it lacks clear distinctions of past, present and future, while constantly referring back to the main idea, it gives rise to a maximum of recollection and a minimum of suspense. Two Greek sentences are contrasted with one another, the one containing suspense, the other unfolding itself simply. The stylistic effects of hypotaxis and parataxis are well known: in this chapter they are described in psychological terms. It is surprising, since an English sentence has been used to introduce this analysis, that the peculiar power of an inflected language to produce suspense is not more fully discussed.

In chapter four, entitled 'Qualitative Time', the authors differentiate between the nuances of meaning which the various Greek and Latin words for 'time' carry, with particular interest in pleasant and unpleasant connotations. They conclude that a development from a qualitative to an abstract conception of time can be traced from earlier to later literature. The relationship between this conclusion and the earlier discussion is that appositional style affords the greatest possible means of positive response in an audience, whose mood will therefore be in accord with a qualitative conception of time. But this chapter might also be read with profit on its own as a useful discussion of the idea of time in the classical world. In the concluding chapter the authors give a brief survey of the occurrence of appositional style in Sanskrit, Old Persian, Hebrew and Anglo-Saxon, thus supporting their contention that apposition is a characteristic of language in its early stage of development.

H. and A. Thornton have written a provocative and fascinating book. In writing for both classicists and psychologists they have, no doubt consciously, limited their scope in both subjects. But this book will undoubtedly contribute towards a better understanding of the ancient mind in its most fertile period of literary achievement.

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The year 1662 marked the tercentenary of the birth of Bentley, and the occasion was duly celebrated in England by the Classical Association and the University of Leeds. The most useful contribution, however, to the commemoration comes from the University of Toronto in this reissue, with an introduction by G. P. Goold, of the Epistola ad Millium, 'the production', to quote Professor Goold, 'which not only marked the author's brilliant début but also inaugurated the great age of English classical scholarship.' The text is a photographic reproduction of that in Dyce's edition of Bentley's works, published in 1836. The type used for the introduction harmonises well with that of 1836 and the book is agreeable to read and to look at.

The introduction outlines the circumstances under which the Letter was written, describes its contents and ends with an estimate of its quality. It is admirably done. My only criticisms, and they are very minor ones, are that it does not mention the date of publication of the Epistola (to find this one has to look to the small print on the title page reproduced from Dyce) and that it describes William Lloyd, who played some part in encouraging Bentley to write the work, as bishop of Lichfield (and subsequently of Worcester), whereas at the time when the Epistola was written he was still bishop of St Asaph. As there were two contemporary bishops both bearing the name William Lloyd, there is a possibility of confusion here.

Classical scholars will be grateful to the University of Toronto for making available a work which, in Professor Goold's words, is 'a classic among academic productions and still... charged with power to instruct and inspire the scholarship of another age', and one which is also, unlike many old works of learning, both short and entertaining. They will look forward too with keen anticipation to the new biography of Bentley on which Professor Goold is now engaged.

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Mr Raven has written an introduction to Greek metre which assumes no previous knowledge of the subject and aims to bring some knowledge of the rhythmical flow of Greek verse to every reader of Greek poetry. He seems to me to have made a great contribution towards this by the really remarkable lucidity with which he expresses metrical concepts. He avoids technical terms in his first explanations, while gradually adding the most familiar and ending with a fairly comprehensive index of metrical names. The addition of the common abbreviations, including Schroeder's short-hand, would be useful. The book is designed for use as a reference book. After an introduction on 'Structure', 'Quantity', 'Prosody and
Basic Rhythms', his excellent cross-referencing ensures that his chapters on individual metres can be read in any order.

Mr Raven’s main interest is in the more complicated lyric metres, especially in drama. But he gives the main prosodic facts about the commonest stichic lines and simple stanzas. There are some rather arbitrary omissions. The relation of Porson’s canon in comic iambics to high-flown παρασαραγωδία and the part its non-observance plays in debunking tragic quotations as Frogs 1477–8 is of general interest; and the not uncommon omission of diaeresis in the comic trochaic tetrameter and the instances of complete metron diaeresis here and in comic iambics noticeably differentiate them from their tragic counterparts.

On choral lyric etc., the work is based, broadly speaking, on what I believe to be the right principles, accepted now by most experts. Unlike previous English introductions it aims to give most of the cola extant in each genre, not merely the most familiar. This will greatly help those wanting to make the jump from metrical theory to reading texts.

Only in his chapter on Aeolic are his lists of isolated lines likely to prove daunting. More examples showing the relation of rare variants to familiar forms seem desirable. This chapter also contains some theorising out of place in an introduction. The doctrine of the four-syllabled ‘Aeolic base’ and its converse, followed by the exceptions, seems hard on a beginner.

In earlier chapters the avoidance of historical treatment and the mixing of different types of verse works well, but the mixing up of different genres of Aeolic seems to me a mistake. Lesbian monody is easier to grasp apart from its complicated posterity.

I am sorry that Mr Raven appears unconvinced of the importance of A. M. Dale’s ‘Metrical Units of Greek Verse’, CQ XLIV, p. 138, N. S., I, p. 20, p. 119, which I find most cogent. Her notation can greatly facilitate the memorising of Lesbian cola, and her essay has much increased my enjoyment of Pindar’s ‘Aeolic’. The distinctions she makes between single-units and the two sorts of junction are, to my ear, a clue to the checks and flows of Greek metre, which are fundamental in Pindar where the quantitative rhythm is not overlaid by other factors. If false colometry is discarded, much of Pindar’s ‘Aeolic’ metres can be enjoyed simply by scanning, since phrasing is largely by blunt junction ... ο ... ο ... which unless obscured by resolution is automatically audible.

Anyone can see a difference in movement between e.g. the Asclepiads and an Alcaic deca-syllable. But the flowing movement which scholars have diagnosed in Dactylo-epitrite, and which therefore I assume should in some degree be audible in ‘Aeolic’ lines phrased by link-anceps, I can recognise only when thinking in phrase-units and aware of the ancipita as make-weight syllables. A. M. Dale’s notation shows this phrasing and also reveals rhythmical echoes.

In analysing Ol. IX 1, Mr Raven should explain that his line division reflects period-ends, i.e. breaks in the synaphe, here objectively known.

In Ol. IX his colometry separating off the closing rhythms ι.-ο. ... ο. is 3–6 could lead to monotonous phrasing, nullifying the effect of Pindar’s varying word-division. Pindar first marks it off by diaeresis ι. ... οικίων ἀπὸ τότης; regular diaeresis carried through the poem comes only in the next line οικίων — οίκων — οικόν — οίκων — οίκων — οίκων —. Finally in l. 8 it is enunciated as a separate period τοιοῦτος ἔλεγεν.

In Ol. I the second ‘dochmiac’ l. 10 Κράινεν παιδί ἐκ αὐτοῦ ... νεκροῦντος is bound to suggest a phrasing cutting across the quantitative flow most unlikely in Pindar, and incidentally obscure the echo of the Paeon α οικάδειν from l. 9.

On tempo I wish Mr Raven had said that metres were written in the convention that one long equals two shorts rather than suggesting that they were equal in performance. The evidence suggests some adjustment and the rhythms can be lost by attempts to keep a precise relation. He rightly discriminates against Jebb’s attempts (unfortunately regularly reprinted in Goodwin’s Greek grammar) to say what they were.

On cola he should mention their relation to the printed page, bring the skeleton out of the cupboard and admit that scholars, besides occasionally giving metrically different readings, divide their texts differently, which may change the metre.

There is now much common ground, and different colometries may merely reflect more or less probable guess-work between different possibilities, where the absence of song and dance leaves ambiguities. From a beginner’s point of view many texts are sound enough to attempt reading metres, though, as Mr Raven recognises, some, not necessarily the best, are easier than others. But readers should be warned that there are texts, mostly old, where the colometry ruins the structure.

Mr Raven’s metrical notes on texts assume proficiency in scansion. References to, e.g., Schroeder’s Cantica, where his private theory can be disregarded, and Snell’s edition of Pindar, which make it possible to learn quantities by reading lyric, would be valuable.

As an introduction I strongly recommend the book, and can report that several guinea-pigs with little metrical knowledge who kindly read the book for me found it easy to understand and enjoyed reading it.

I have never seen a book on metre so beautifully printed and produced.

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Paul Maas’ erudite, austere and difficult treatise is indispensable to students of Greek metre and textual critics as a guide to those statistics and ‘rules’, our only substitutes for the knowledge of what ancient
Greek poetry sounded like. English scholars are fortunate that this new edition appears in the form of a translation by Professor H. Lloyd-Jones, who has added some useful notes.

The translation contains a few roughnesses and imprecisions. In § 2 the omission of the parenthesis ‘bei der üblichen Verwechslung von “kurz” und “unbenton”’ makes 2 appear to contradict 1. At 34 ‘... dactylic lines that ended with a disyllabic biceps’ it would have been better to avoid ‘line’ as a translation for ‘Reihe’, since Lloyd-Jones usually uses ‘line’ to translate ‘Vers’, while Maas avoided ‘Vers’ here. At 56 the reader may be puzzled by ‘longa occur together in ones and twos’. There is a mistranslation in 116: ‘nur unter Umständen zu beanstanden wäre’ means ‘would only be objectionable under special circumstances’, not ‘would be impossible, except in special circumstances’.

One of Maas’ greatest services to metrical studies has been to get rid of a mass of obfuscating terminology. However, his use of his own term ‘biceps’, ‘a long equivalent to two short, or two short equivalents to a long’, is rather lax. In 37 he states that biceps is avoided in the antepenultimate place in the hexameter and in the tragic trimeter and in the proantepenultimate place in the elegiac ‘pentameter’. But, according to Maas’ own definition, the antepenultimate element of the hexameter is biceps, although it takes disyllabic form more often than monosyllabic, while in the ‘pentameter’ the proantepenultimate element is shown by internal responson to be in theory biceps, although it regularly takes disyllabic form. The principle underlying the multiplicity of phenomena described in 36, 37 is that the latter part of a ‘period’ or ‘verse’ tends to be stereotyped in rhythm. Whether biceps should be used for the long of iambic and trochaic is very questionable. These longs, which may sometimes be resolved, but may not when resolved be split by word-end, are clearly different in nature from the constituents of the anapaest or the second element of the dactyl. By applying the same term to them Maas comes dangerously near to introducing a false concept.

One reason for the value of Maas’ work is that he alone of metrical writers fully appreciates the importance both to the metrician and to the textual critic of knowing not merely what happens, but how often it happens. His figures, however, are not infallible. In 84 the proportion of disyllabic bicpitia to monosyllabic (i.e. of ‘dactyls’ to ‘spondees’) in the first four metra of the Homeric hexameter is stated to be ‘1:2:1’. This is clearly a mistake for the ‘1:2:1’ of the German editions, but it is still a very wide estimate, and Maas should have stated on what it is based. In two passages of 100 lines each from the Iliad and Odyssey I have found the proportion to be approximately 3 dactyls to 1 spondee. In 87 Maas estimates that Hermann’s Law is violated once in a thousand lines in Homer. Van Leeuwen (Homerica IV, Mneso-syne 1890) lists over 70 examples. The Iliad and Odyssey together amount to some 27,800 lines, so that Hermann’s Law seems to be violated about once in 390 lines. On caesura ‘after the sixth element’ in the iambic trimeter (103) Maas says that there are about 25 examples in Aeschylus and Sophocles together, while in Euripides there are about 100, but all with elision. He omits to say whether the 25 examples in Aeschylus and Sophocles include those with elision, and, if not, how many examples there are with elision. For Aeschylus the actual figures are: at least 11 examples without elision and 46 with.

Maas’ brief remarks on dochniâs (56 (c)) miss the essentials. His example of a common form of δ, —- — — occurs 250 times in tragedy, but — — — — occurs 534 times and — — — — — 530 times. His statement that — — — — — is avoided is unhelpful. — — — never occurs as δ, — — — — — occurs 15 times in Aeschylus and Sophocles together and 12 times in Euripides. The rarest forms of δ are in fact those which end with double short preceded by long ances (x — —). Maas’ treatment of the question of dactylic sequences ending in disyllabic biceps (34) is sketchy and confused. As Lloyd-Jones points out in his footnote, — — is not disyllabic biceps.

Not all the additions made to this edition are improvements. On dactylo-epitrite metre (55) Maas adds ‘Pindar may have been its inventor’. This unnecessary conjecture is rendered extremely dubious by some of the fragments of Stesichorus. In the appendix on the ‘bridge’ Maas originally added Horace, Ep. 17 and Seneca as evidence for ancient awareness of Porson’s Law. For Seneca he now substitutes ‘Seneca in his Octavia’. In fact a notable stylistic difference between Seneca and the author of the Octavia is that Seneca allows violations of Porson’s Law without elision and the author of the Octavia does not (See F. X. Bill, Beiträge zur Lex Porsoniana).

Section XIII was never faultless and has deteriorated. In 135 Maas lists pronouns among prepositions, but accented pronouns are independent words and unaccented ones are postpositive, like µν in Maas’ own example. The note on prepositions before pause in Pindar added to 135 should have been incorporated in the beginning of 136. The references added to the end of the first paragraph of 136 are not instances of prepositions at the end of the line, but miscellaneous examples of enjambement of the hexameter and pentameter in elegiacs, which are out of place here. Pers. 331 and Eum. 595 have been added to the list of prepositions before caesura when they are actually examples of postpositive after caesura and are correctly listed as such in 137. ‘O.C. 78’ seems to be a misprint for 278, but here it is only Dettweiler’s not very probable conjecture that has postpositive before caesura. Of the examples listed of two prepositions before caesura, four (Aj. 1228, Or. 577, 889, Theoc. 21, 47) are correct. Of the rest I.T. 670 and Hel. 267 have caesura after one prepositive, and I.T. 696 and Or. 704 have caesura before postpositives.
This edition contains a number of misprints and minor errors. The following metrical schemes are wrong: 33, 4(b) Anacreon 54, str. 1, 2 should be ‘+ + – – + – + – – + + – – – – + ’; 33, 5, 2, 3, 777, the seventh element of the anacrusis is printed as ‘x’ instead of ‘+’ and the last long is missing; 48, 3, long missing before ‘caesura’; 48, 4 for ‘tremat’ read ‘trimeter’ and scan ‘+ + – – + – + + – –’; 54, 8, the last element of Sappho 94 should be – (Cf. 54, 1); p. 40, n. 2, the eighth element of the encliticus is ‘x’, not ‘–’; 70, the first colon of Simonides 4 should be ‘+ + – – + – + –’; 100, Roman 1 should stand after thea. Some aids to the reader have been omitted: 85, caesurae are not marked; 128, long marks are needed over συνέχεις, συνακλητες, Στίγμα τής; 112, for Ἀυ. 1706–1; 9 read 1706–19. 116, Ῥαν. 4, for τοῦτο read τοῦτο. 137, Cho. 481, for σου read σοι. P. 95, 1, 26, for ‘(see 135)’ read ‘(see 137)’.

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In this munificently produced (and priced) new work the comparatist of Florence University sets out to give us in over 400 quarto pages the results of his lifelong reflections on the problem of the genesis of the various Indo-European peoples and languages. The basic ideas are not new. The author has presented them before, but never on this scale, and never applied to the whole IE group. Philologist readers of this journal will be especially interested in his views on the origin of the Greeks, and, perhaps, the Romans, but the archaeologists will also wish to see Devoto’s analysis of the problems of the whole of prehistoric Europe and Western Asia.

The work is divided into eight chapters but, essentially, it consists of four main sections. First (3–70), the problem of IE and its original homeland is circumscribed with the help of comparative philology—on which the very notion of IE rests. After a rapid survey of more recent reconstructions of Pre- and Proto-IE stages, D. examines whether geography, anthropology and ethnography can contribute to the solution of the problem. Next (71–156) archaeology is drawn upon. On linguistic grounds we can, by the method of elimination (e.g. Greece is not originally inhabited by IE-s), narrow down the homeland to Central and Eastern Europe (80 ff., see also 43 ff.). Chronologically the bronze-age, in Europe from the early 2nd millennium, is too late; the IE period must be placed in the late neolithic, but not much earlier than 2500 B.C., since agriculture had been introduced already (77). Devoto finds that the only archaeological culture that will satisfy these conditions of time and space is that of the ‘Bandkeramik’ (82, 91). But this was only the beginning. The formation of the IE peoples in the 2nd and 1st millennia B.C. cannot be understood without appreciating the part played by the battle-axe culture, the cultures of Unetice and Lausitz (Urnfelder), and many others, and Devoto guides us with amazing skill on this bewildering and treacherous ground.

The main section (157–340) analyses the linguistic evidence from a viewpoint which is still largely unknown outside Italy. ‘Areal linguistics’, an Italian refinement of Joh. Schmidt’s wave-theory—for a detailed survey, see Bonfante, Language 23, 1947, 344–375—demands that linguistic facts should be viewed as the results of a process in which innovations supersede earlier elements, either completely eliminating them or relegating them to the periphery of the linguistic area. It also claims to possess certain ‘norms’, with the help of which the static picture of the data can be turned into a dynamic one of true history. Devoto believes (187 f.) that of the five ‘norms’, one at least is highly significant for IE too: the norma delle aree laterali. It asserts that if lateral, or marginal, areas exhibit a linguistic feature contrasting with some other feature in the other areas, they preserve the more archaic form once in use throughout the area. The fact that *egnis ‘fire’ appears in, e.g., Latin and Aryan, while other languages have cognates of Greek νέφ, proves that *egnis was at one time the Common-IE word, later superseded in most areas by a new word. ‘Marginal’ and ‘central’ thus become dynamic contrasts, important because ‘marginal’ also means ‘more archaic’.

D. then examines the vocabulary of the IE languages in three groups. First, the vocabulary shared by (almost) all the IE languages (vocabulario compatto) is discussed (195–262). Then come (263–291) the lexical differences between West (and North) and East (and South), e.g. (264) *neck’ is *kwol- in Latin (collum) and Germanic (e.g. Germ. Hals), but *gewera in Greek (βηθος), and *gurvi in Sanskrit (ग्रव) and Slavic (griva). They warrant the important conclusion (291) that the East did not innovate in the terminology of ‘nature’ and ‘technology’, and there is therefore no reason for assuming a period of Eastern ‘unity’; a Western unity dissolved in a heterogeneous Eastern environment, and not vice versa. Even more important results are yielded by the examination of the lexical relations between centre and periphery (292–340). The fact that the terminology of social structure and religion characteristic of Italic-Celtic and Aryan is not found in the central areas is interpreted as evidence for a ‘democratic’ revolution in the centre which eliminated these institutions and the vocabulary connected with them (298 f., 318 f.).

We are now ready for the last section (341–403) which describes how the various IE peoples were ‘constituted’. In each case, (A) the archaeological evidence, (B) the linguistic material is evaluated and correlated, and (C) the pre-IE element is considered. For Devoto believes, rightly, that each ‘nation’ was constituted as a result of the prolonged symbiosis and mixture of various ethnic elements. We cannot
discuss here the whole IE group. But the ethno-
genesis of the Greeks (375-382) is sufficiently interest-
ing and typical of the method to merit a fuller description.

(A) The first IE elements—but probably only small groups—can be found in the Dimini-culture. Early Helladic signals on the whole an anti-IE move-
ment, dominated by Anatolian influences, although here again Northern elements are clear. But it is only with Middle Helladic, characterised by the 
Mynian ware, that we are forced to assume a subst-
tantial IE-isation of Greece; D. calls this stratum 
Pelasgic, and assumes that it penetrated the mainland as well as the islands, including parts of Crete. Its 
bearers came not from Asia Minor (Pisani) since there too, they went from the Balkans [2]; these first ‘Greeks’ must have followed the same route as the 
Hittites, along the Western shore of the Black Sea. Late Helladic is clearly recognisable as completely IE-ised. To the Pelasgic stratum we can now add 
what may be called Proto-Greek. Its end is signalled by the destruction of Mycenae and Tiryns, caused by 
the Dorians who followed a Western route, down the 
valleys of the Morava and Vardar. Their vanguard 
becomes part of the Greek linguistic community while their rear forms Macedonian.

(B) In the language, too, we can discover more than one IE component:
(1) Elements of the all-IE (compact) vocabulary, 
 marginal elements and Graeco-Aryan innovations; 
these can be associated with the Pelasgic stratum.
(2) Central lexical elements, imported along the 
same route (Eastern Balkans) by the Proto-Greeks.

But as yet no labels can be attached to these various elements, e.g. Achaean and Ionic to the Eastern, 
Aeolic to a Western invasion.

Ventris’ decipherment shows that the fusion of all 
these elements preceded the Mycenaean age. 
Mycenaean Greek shows the presence of 
(1) the compact vocabulary (e.g. δόκει ποσ᾽ ἵνα 
μην νέος ἢ τοῦτο πατήρ, etc.);
(2) South-Eastern (Graeco-Aryan) elements (e.g. 
κεκείμενος, κεκαίμενος, κυριόν, ἰερόν);
(3) Central elements (θέσις ἐσδοπ πριστό).

Pelasgic and Proto-Greek fused in Mycenaean Greek 
all that is Greek—except Doric. This ‘First Common Greek’ 
is characterised by the athematic infin. 
-ωαι, the particles ἄν, κε, ἐι, ὅτε, the verb ‘will’ with 
o (βούλομαι), the change -τις -τι, preposition ποιός, 
and (negatively) preservation of the labiovellars.

In contrast to this ‘First Common Greek’, Doric 
brings the athematic inf. -μεν, the particles αἱ κα., 
verb ‘will’ with ε (δέλημαι); moreover, such IE 
differences (not Greek variants!) as 1st pl. ending -μεν 
(v. -μεν), -τι (not -τι), and especially the labialisation of 
the labiovellars. In other words, the Dorian 
invassion brings, not just the ‘Doric dialect’, but a new 
IE element; from its fusion with Mycenaean arises, 
ca. 1000 B.C., the ‘Second Common Greek’ with later 
ramifications which can now be legitimately labelled 
Achaean, Aeolic, Ionic and Doric.

The ‘stragglers’ of the Dorian invasion did not join 
in shaping Greek. Staying behind in Macedonia, 
they later adopted a different treatment of the IE 
aspirates (κεφαλή: κεφαλή), but this does not turn them 
ton Illyrians.

(C) The direct evidence for pre-IE is scanty 
(Lemnos, Eteocretan) but the Aegaean influence 
is amply documented by place-names in -ορ-, -οτ-, -πρ-, 
-λ-, -ς- (Περιότ, Ἰμβαίας, etc.). The influence of 
the new environment shows itself in πάντα ‘sea’ (IE 
‘road’) and the creation of θυγατέρας, replacing the old 
word for ‘brother’. Direct influence of the substra-
mum is seen in άμελος, άλμα, ἀλος, ἀλός, λέον, 
σφιξ, Πώλας, βασίλεως, τύραννος, λίθος, φύλος, 
φασίος.

We must wholeheartedly approve of D’s aim to 
give a dynamic picture of IE prehistory, envisag-
ing the historical languages as the results of very complex 
processes of amalgamation. The question is whether 
the (linguistic) evidence will bear the structure 
imposed. In view of the fundamental importance 
attached to the concepts ‘marginal’, ‘central’, etc., 
one would have liked more in the way of justification 
than the unsatisfactory paragraph at pp. 187–8. The 
fact remains that the argumentation is based on the 
historical distribution of the IE languages. All IE 
peoples migrate from a central area (the heart of 
Europe) to their historical habitats. The reflux of 
the Celts from the far West, and of the Scythians 
from the far East, is regarded as exceptional and atypical. 
But is this assumption safe? To take a concrete 
example. The medio-passive -ς-endings are 
regarded as archaic because they are found in ‘mar-
ginal’ languages; it follows that the ‘central’ languages 
must have lost them. But this view stands or falls 
with the primeval character, or otherwise, of the 
historical distribution. If we assume, e.g., that the 
forebears of the Latins, Celts, Hittites, Aryans (?), 
and Tocharians, once lived in a compact area some-
where in Central and/or Eastern Europe, their 
ς-endings can be explained as an innovation which 
did not affect such ‘marginal’ languages as Greek, 
Germanic and Balto-Slavic. This collocation would 
at the same time account for the peculiar lexical 
agreements between Italo-Celtic and Aryan—but 
would deal a deadly blow to the hypothesis of a 
democratic revolution in the centre. The pre-Latins, 
for instance, would then be free to migrate westward, 
establish those undeniable, but late, agreements 
which exist with Germanic, and move on to the 
South; they might even go down the Balkans and 
cross into Apulia—if we wish to accept D’s view that 
they came to Rome from the South.

These general difficulties are compounded by 
similar defects in the reconstructed lexicon, where the 
labels ‘all-IE’, ‘central’, etc., are assigned too easily. 
D. relies heavily on Walde-Pokorny and Pokorny, 
and ignores accidentally uncodified work. For 
the word ‘neck’, mentioned above, he establishes a clear 
contrast between East and West, but fails to notice 
that his Eastern root appears in Old Irish *bríghait 
‘neck’ and MHG kraje ‘neck, throat’, destroying his
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argument. He assumes (303) that the Centre created a new word for 'singing' (*sengh-*) — but the root exists in Indian, too, and also in Celtic. He thinks (339) that in contrast to peripheral *met- 'harvest' the Centre innovated a word *(e)mē- (Gk. ἀμέω, Engl. mow). But the two are surely connected, and it is enough to envisage the aorist of *met-, *mē-, to see how easily *mē- could be extracted.

The synthesis of archaeology and philology is often uneasy. Even the philological analysis is at times forced, and forgetful of awkward detail. In D's view the 'Dorians' arrived after Greek was (almost) completely formed. This must mean that their language was still more or less the original IE speech-form. But are we really to believe that these new arrivals made all the necessary adjustments to existing 'Greek', noticing and understanding all the forms that showed h- for their s, nil for their y, th etc. for their dh etc., and accepting all these novelties, except for a curious retention of τι for the 'Greek' ατ, etc.? Or are we to assume that the prestige of 'Greek' made them give up their own language, although somehow they managed to retain such oddities as -τι-, -με-, δηλομαι? Neither of these alternatives will do, and D. could have seen the formidable difficulties in his path by simply looking at Chadwick's article in Greece and Rome 3, 1936, 38 f. (see now CAH, rev. ed., II Ch. XXXIX, 1963). A little care would also have prevented him from suggesting that the Dorians brought the labialisation of the labiovelars, since it is Aeolic that is (wrongly as I shall show elsewhere) thought to exhibit this feature, not Doric. We may also remark here that D's fondness for attaching various linguistic phenomena to certain cultures (e.g. labialisation to Urnenfelder people) leads to pure fiction.

It was perhaps too ambitious for a single scholar to expect to be able to keep abreast of the spate of new work coming out year after year. With a work started in 1938 one could understand that minor recent articles are overlooked. Amazing though it may sound, in a work of this size there is no bibliography, and even the generously used abbreviations are nowhere explained. As a matter of fact, work after 1955 or so is hardly ever noticed, and even earlier important work is often missed. Thus Georgiev's Isledovanija (1938!) though treating the same problems, is not used. The problem of Germanic is discussed with no knowledge of Frings's fundamental Grundlegung einer Geschichte der deutschen Sprache (1948!). Greek prehistory is treated without noticing Mellaart's synopsis at AJA 62, 1958, 9-33. The hoary salmon-argument is still exploited although Thieme (quoted 284 f.) recorded Ossetic lasag. For the Asianic and Greek place-names in *ś-, Kretschmer's unhappy Caucasian comparison is accepted (375), in spite of Laroche's work (1957, and later). D. still finds (164, 201) a break in the numeral system between 60 and 70 (*ξήθοντα-έφθομinkeronta) and believes in Babylonian influence: even if my work on Numerals (1960!)

came too late, the thesis had been demolished in 1959 by Sommer whose discussion is quoted (164 f.) but without revealing that his conclusions destroy the statement in the text.

In a work based almost entirely on the lexicon, one could expect some new etymological connexions. There are some but only semantic, and quite unconvincing. The (near) homonymy of *geni- 'be born' and *genē- 'know' (Gk. γίνομαι: γνωρίζω) is explained on the assumption that 'to be born' is 'cominciare a prender conoscenza del mondo' (204). The group of Lat. sterlis is derived from *ster- 'extend' because of the 'ventre constantemente disteso, mai sporgente in seguito a gravidanza' (257). The noun *pekū (Lat. pecus) is made the basis of a denominative verb meaning 'agire con il gregge', 'coglierne il frutto = tosare', and so we get pecto (256), although usually, and rightly, the opposite direction is adopted. Similarly, although the IE-s did not know the sea (43 f.), their word for 'ploughing' (арал) was from the verb 'to row' (Gk. ἐπικός, etc.) as 'remare la terra' (261); if there is any connection between the two roots (but note α- v. e-) it is surely the other way round: 'rowing' is transferred from 'ploughing (the sea)', a development illustrated by ample historical evidence. Even worse are the cases where D. slips up on elementary facts. Greek θεός is still traced to *dhaves- (310) although Myc. to is known to him (370). Myc. tereta is derived from *k*ēl- (320), kotona is equated with γήθεόν (ibid.); σάγα is from *dhayei- (263), although Doric has σαγα. Lat. hortus is derived from *ho-sarnos (210) without a word on how this is possible. An early (instr.) hū vēre might be conceivable syntactically, but phonetically how do we get hōr? (Cf. Glotta 36, 1959, 114).

The book is furnished with several indices, partly in the body of the work, partly (but with continuous pagination, 437–522) as a separate fascicule. The latter contains the IE lexical material, listed under ten headings; the items give the main evidence (but by no means in full) and a statement whether they are Western, Eastern, or marginal, etc. It is claimed that printing this index separately may be useful for seminars. I found the arrangement irksome. One had to waste time in locating the evidence on which certain, at times important, conclusions were based.

In addition, both the index and the main text are marred by a large number of misprints; at times even footnotes are lost.

In spite of all these criticisms, and others that might be raised, it should be stated now that the work is a hopeful beginning, however imperfect it may be. A more dynamic, and historically more true, picture of the formation of the IE 'nations' must be conceived on the general lines here pursued. But it will have to be written with much greater circumspection as to the possible vagaries of distribution, and a much fuller command of all the facts.

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The author sets out to dispute and replace the current doctrine that the three-gender system of the Indo-European languages developed from an earlier two-class system of animate and inanimate nouns by a later differentiation of the animate group basically along the lines of sex division. He does not believe that foot is masculine in the Indo-European languages because of its strength or hand feminine because of 'ses qualités réceptives', and rejects those 'forces femelles' which Meillet invoked to explain feminine formations: 'La mythologie, chacun le sait, n’a point fait le langage; elle en est une maladie' (p. viii).

While the general line and details of attack are clear, the author's own views are much more difficult to ascertain and reduce to a brief résumé. As will be clear from the title and sub-title, he attempts to reconstruct the evolution of the Indo-European three-gender system with the help of Greek alone, confining his investigation to the →-stems. Both limitations make it questionable whether the undertaking can be brought to a profitable and solid conclusion.

The suffix →, says the author, forms abstracts, i.e. substantives, from adjectives. The term 'abstract', in his usage, has nothing to do with the usual contrast of abstract and concrete, it is a purely formal concept. The adjectival forms, which look like feminines, are not elliptic. The Grecian might admit this as a general thesis but he will be reluctant to accept the assertion that δεξιά, γραμματική, πολεμίς, πολιά, κρήνη, etc., do not represent, and originate in, such phrases as δεξιά χείρ, ή γραμματική τέχνη, πολεμίς γῆ (χώρα), πολιά τρέχει, κρήνη θύρα, κρήνη θφύτη (σμ., etc.), and these will prompt reservations concerning the general thesis, too. G., however, concludes that the sole function of → seems to have been to form substantives from thematic adjectives, as exemplified by τομής: τομή. Such adjectives are often lost: πέδι (and πέδω) are from *πέδον: *πέδεστρο, from *πεδό-/πεδών: θύρα from *θυρός: *θυρείος 'chasseur', from θφύτη, etc. Accordingly, a root-noun frequently contrasts with an → derivative: ἀλει/ἀλλήθ, κρίτθ (θ)/κρήθη. This relation obtains with more complex suffixes, too: -οι- (ακτή < ακτός 'pointu'), -ο- (ζένη: ϊζην), -ο- (θερώς: θερήν), -o- (θερώ-: θερί-), etc.

The development of the masculine/feminine dichotomy was triggered off by a twofold complementary process: the nouns in → became adjectives and the adjective in → could become a substantive. The relation τομής (noun): τομή (noun, from τομής adj.) is the basis of the wholesale redistribution of the Indo-European lexicon. Thus gender is a pseudo-category, the contrast of 'natural' and 'grammatical' gender is irrelevant (grammatical gender is only linguistic, nothing more), the development of the three-class system is not due to semantic reasons but to a formal accident or series of accidents.

It is hard to believe that the purely formal contrast of -o/-a- nouns (both are 'abstracts') could have produced the male/female opposition so frequently observed, even though the author says that this naturally arose from the contrast of species/individual. But no reason is given, and it is indeed hard to discern one, for the statement that -o/-a- expressed such a contrast. Nor is the thesis demonstrated that monosyllables became feminine because of their form: πολιά Zei is obvious counter-instances which cannot be explained away. It seems to me that the identity of the formation in nominal -ε α- (from -ει- -ει- -ει-) with pronominal *σι, *η, definitely speaks in favour of the view that -ει- was an indicator of the female; the real problem is not the emergence of the masculine/feminine contrast—which is quite natural, though extra-linguistic, and matched elsewhere—but the attribution to these classes of nouns which we cannot visualise in terms of this opposition.

The theoretical conclusions of Part I (11–55) are followed up in Part II (57–104) by a discussion of the semantic aspect. G. thinks that in the type τόμος/ τομή the terms are opposed as 'le geste et sa portée, l' « acte » et son « efficience ». This definition is then applied to, and checked on, some 70 pairs of the type τόμος/τομή but also ἠδονή/ ἠδύνα, ὄρος/όρα, and ὀλίκος/οἷκα which in G's view present the same, constant, semantic relation. These studies, though at times over-subtle, will be most useful to the Grecian. In some cases, of course, one will have misgivings about the author's interpretations. Since ἁγοράς is used by Euripides alone, and only in choruses, it is unrealistic to treat the pair ἁγοράς/ἁγορά as an instance of the contrast in question: ἁγοράς is obviously the poet's coinage, who attempts to reproduce the old meaning of ἁγορά while avoiding this prose-form. The same applies to τόσος/τοιχός of which the latter only appears in a 2nd c. A.D. author. Nor can the difference in question be recovered from the texts for ἄφορος/ἄφωρή, or πάθος/ποθό. In other cases, the distinction is more real and its discussion valuable. Thus τυχή is 'blew, wound' only, while τύχε is 'the effect of a blow or pressure' ('frappe': 'empreinte'). The discussion of, e.g., ἀλοκός/ἀλοκή, νόμος/νομή, σπάρος/σπάρω, τρίμος/τρίμβη, τρίπος/τρίστη, ψηφός/ψηφάρι, φώς/φωνή, as well as of the slightly different morphological pairs ἐδοκεῖ/ἐδικαί, ἐδύος/ἐδύμα, νέος/νεφέλ, ὄρος/όρα, etc., will be read with profit by anyone interested in the often very elusive differences between these and similar 'variants'.

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GUIRAUD (C.) La phrase nominale en Grec

The book starts with a good account of earlier studies of the nominal type of expression both in Greek and in Indo-European. Guiraud criticises the work on Greek above all on the ground that it has not revealed a functional plan. He bases himself on the view of Benveniste (pp. 19–20), that the functions of the nominal phrase and of the verbal phrase with ‘be’ must be different. It is, of course, an old observation that general truths and proverbial expressions are frequent in the nominal form, and Guiraud would see in them the essential core. But we also find both general truths with ‘be’, and particular statements expressed nominally. We may then adopt either of two radically opposed courses. We may say that there is formal contamination, give up the pursuit of difference of meaning, and content ourselves with listing the various contexts in which the two forms of phrase are found. Or, we may insist that there are still everywhere differences of sense to be discovered. Guiraud takes the latter course and pursues it with great thoroughness and sensitivity in the analysis of Homer, of the Homeric Hymns, of Hesiod, of Theognis, of Pindar, of Herodotus, and of the three tragedians.

First, phrases are dealt with in which the verb form is ἐστι or εἰσί, or would be so expressed; the great majority come under this head. They are treated as appearing in the expression of general truths, and the remainder are classified formally; they are sub-divided into attributive principal clauses (with the verb, if present, in the sense of the copula), non-attributive principals (verb in other senses), and subordinates. Lastly come cases where the verb (expressed or understood) is other than ἐστι, εἰσί.

For the general truths the nominal phrase is favoured, its frequency rising as high as 93% of all cases in Hesiod. In Herodotus, however, it reaches its lowest figure, at 50%. Guiraud explains some of the cases with ‘be’ as being more particularised, being bound up with or derived from the context. He contrasts (pp. 49–50) the nominal phrase which is used of a truth well-known and accepted, which explains and justifies (and so shows a causal relation, often marked by γάρ); and that with ‘be’ which is not equally to be assumed and denotes a conclusion. In other cases with ‘be’ the verb emphasises, ‘it really is so’. There are exceptions to his rules, but he claims that there are few (p. 55). Unfortunately he does not say just how many.

The more difficult part of the subject lies with the phrases not expressing general and permanent truths. The most numerous are the attributives in principal clauses, and I take these as an example of his method. Guiraud makes a fundamental division between (a) appréciations particulières, judgments which are close to general truths but are not so completely timeless; and (b) phrases giving description or information, which are concerned with a particular situation. For type (a) absence of verb is normal, and where the verb occurs the reality is stressed. In type (b) the verb is usually found, and here its absence indicates a weaker presentation. But this is not all. Other factors may disturb the working of the system (pp. 156 ff.). The nominal type may be used because traditional in certain phrases (e.g. with φρονίζω, ἀφάντη, γρηγορὸς, λόγος plus infinitive clause), or from a stylistic desire for brachylogy. Gnomic authors, such as Hesiod or Theognis, may extend the nominal use, but a descriptive writer, Herodotus, that of ‘be’. Metre plays a not insignificant part.

It is possible that Guiraud overestresses the operation, and consequently the value, of his functional system (which he thinks works best in Tragedy). It is liable to be upset by so many other factors that one wonders how clear an image of the system the user of the language possessed. The existence of certain favoured patterns of usage (to which Guiraud rightly draws our attention) suggests that convention may be more dominant than he proposes. Note for example ἑκατέρος in subordinates: it favours the use of ἐστι in Homer (there under the influence of metre, pp. 216, 237), but is used nominally in Sophocles (p. 269), so that the convention is reversed—but in each case there is a convention. It would also have been valuable if other prose authors had been studied besides Herodotus, to get away from the effect of metre: it is rather disturbing that Herodotus is the author least well explained by the scheme. However, despite such reservations, this subtle and stimulating study notably advances the understanding of a difficult problem.

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In this country, which is well furnished with Greek textbooks and in which Greek and German are often alternative options in school curricula, Menge’s Repetitorium der griechischen Syntax, of which the first edition appeared in 1878, is not likely to be widely used or even known. Its purpose and character are purely pedagogical. It aims to provide material for the learning and practice of Greek syntax, and is arranged in two parts. The first consists of exercises—questions on the rules of syntax and sentences for translation—covering the whole field, while the second part gives the answers to the questions, amounting in effect to a systematic exposition of Greek syntax, and the versions of the sentences.

The sentences seem to be mainly suggested by or adapted from a wide range of prose authors. Adaptation in the versions seems now and then a trifle arbitrary; for example, sentence 16 of exercise 124, adapted from Thucydides I, 102 gives ὥς ἐμφάνειν ἁπάντος ὁ πρὸς τούτον ἐν Ἡθώμα μεσανίας πόλεμος for the original ὥς αὐτὸς πρὸς τούτον ἐν Ἡθώμα ἐμφάνειν ὁ πόλεμος. Perhaps the modification may be justified as closer to the kind of norm with which a textbook is primarily concerned and as better representing what may be expected from a student. The translation exercises are both numerous and long, and the
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individual sentences vary from one to five or more lines (an exceptional specimen of twelve lines on pp. 232–233 amounts to a short prose) and exemplify a wide selection of style and vocabulary. The answers to the questions give an account of Greek syntax which is generally full, concise and clear, and also agreeably free from attempts to explain or rationalise. No doubt close search would reveal some minor inaccuracies. The statement on the position of το (p. 93) is perhaps over-simple and dogmatic; there is mention of οικείος, κοινός with genitive (p. 196) but not with dative; the section on the relative pronoun (p. 169) omits the use of δοτς with definite antecedent. It may well be argued that the inclusion of such details goes beyond the scope of the book and would have obscured its firmness and clarity of presentation. Certainly anyone who works patiently and thoroughly through this book may expect to acquire both a sound and extensive knowledge of Greek syntax and, as a bonus, familiarity with virtually the whole range of normal Attic prose vocabulary.

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The present work is in three main parts: (a) General characteristics of Greek prose style, (b) translations from French into Greek, (c) syntax.

‘Les Grecs possèdent une langue claire, simple, expressive assez riche pour épouser toutes les formes de l’idée, même très moderne.’ (5) The authors examine style not in relation to the individual but in light of the features common to all writers of a given period. Their aim, in what is essentially an introduction to the problem, is to draw attention to ‘les différences entre les modes, non de pensée, mais d’expression, des langues grecque et française’. (29) It is interesting to compare this approach with that of Carrière, Stylistique grecque pratique, ch. VIII (see JHS 1962, 170) who analyses three passages from Xenophon, Lysias and Plato. The attitude of both languages towards abstract and concrete words introduces the theme (cf. Denniston, Greek Prose Style, ch. II). ‘La langue grecque ayant un caractère verbal accusé, il est naturel que, d’une manière très générale, on y rencontre un verbe là où le français emploie un mot abstrait.’ (9) In the composition of continuous Greek prose the student is taught, in the earlier stages, to avoid the use of abstract nouns as far as possible (cf. Denniston, op. cit., 23). A closer examination, however, shows that Greek is not opposed to the abstract in vocabulary, but the overriding influence of the verb tends to limit its use: the participle has an important rôle. This contributes to clarity of thought and surely promotes ἐναργεία—the quality especially ascribed to Lysias (D.H. Lys., 7). In place of technical words in the strict sense periphrases are used to define ideas. (14) In sentence construction parataxis is frequently employed instead of subordination: here again the participle has many uses which are discussed and illustrated in some detail. (17 ff.)

The passages for translation are chosen from standard French authors from Montaigne to Valéry: they cover a wide variety of subject-matter which is of interest for its own sake as well as for the range of inherent problems it presents to the translator. To each passage and version are appended critical notes which deal with the difficulties encountered in translation and cross-references are given to the appropriate sections on style and syntax. Translation from a modern into an ancient language is an important and rewarding exercise warmly commended by the authors ‘peu d’opérations sont plus fécondes pour l’esprit.’ (5) This is not the place for a detailed analysis of the versions which provide a mine of interesting information but a few general observations may not be inappropriate. In spite of drawing attention to the essential differences between the two languages, the authors try sometimes too faithfully to reflect the form of the French construction: this is most noticeable in the use of periphrasis. Antithesis, although by no means Gorgian in character, is occasionally unnaturally strained. The vocabulary is from a variety of sources. Words, or expressions, often need the qualification ‘un peu poétique’ (cf. μέριμνα IV, πανεράξ ΧII, σχηματιζό μεν XVI)—although the nature of many of the passages ensures an inevitable debt to Plato. There is also an occasional debt to Comedy (βόδα λέγειν IV, και λέγονται ΑΧΙ, XIV)—the latter being merely quoted in Plato, Apol. 196). Rare compounds (ἐπανάρρητος—suggested as an alternative—XXIV), late words προτομείν XI, and non-classical usages (εκφκος XXXIII) are found in the versions. Likewise poetic words may even be used in preference to common prose equivalents. Some expressions might with advantage be modified: perhaps ἐριμων ποιεῖσθαι (στὶ καθὼς) τι γιὰ τοῦτο ἐριμων χρῆσθαι VII, 3? On the whole, however, there are relatively few shortcomings in the versions although some minor alterations are needed in one or two places (e.g. XXXIII, 1 and VII, 2). Actual misprints can readily be corrected from the two lists of corrigenda supplied separately by the publishers.

The third part deals with syntax (135–89). Invariable words (conjunctions, prepositions, the syntax of ἃς, and negatives) and variables (the article, reflexives, adjectives and cases) are discussed. The sentence is examined under two main headings: (A) Propositions principales (ou indépendantes) and (B) Propositions subordonnées. A wide field is covered and some omissions (e.g. the infinitive expressing a wish—B [37]), or cursory treatments (reflexives B [24], 144, special case usages B [28–31], 145 ff., and the use of the infinitive in place of the
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imperative—B [35], 149-150) are unavoidable. Observations on the distinction between the different forms of consecutive clauses B [77-9], 169, and on absolute constructions B [115-7], 188 f.) are among the many interesting topics included.

Two further misprints may be noted: on p. 4, l. 24 read préhémérique and p. 83, n. to (8) indirecte.

There is a bibliography (191) and a detailed statement of contents.

The present book is a useful complement to Carrière, Stylistique grecque pratique and provides much information likely to be of value to students in stimulating their further awareness of style.

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An admirably produced book, notably free from technical blemish, very generously illustrated, marked by a Gallic clarity of thought and expression. Its purpose, stated by M. on p. 89, is to assemble ‘toutes les inscriptions syllabiques de quelque importance qui sont actuellement accessibles, ainsi que les légendes monétaires syllabiques les plus intéressantes, et de présenter ces documents dans leur cadre géographique et historique, avec un commentaire épigraphique et linguistique’. On the same page he disclaims any intention of producing a corpus—as his lack of access to the inscriptions of Kofizin, Old Paphos and Rantidi has rendered not indeed inevitable but prudent. M. has thus reduced his task to something both sizeable and congenial: he may say what he likes—and as a pupil of Chantaine and a specialist in Greek ‘onomastique’ he is very well qualified to say—about the inscriptions of his choice admittedly a most representative collection. This approach, practical but vaguely meretricious, may induce in the critical reader a less generous appreciation of the book’s achievement, a harsher eye for what, in his view, may be its demerits.

Let be said, however, at the outset that we may now no longer refer to any inscription included in this series by any other than M.’s number, since it has naturally superseded the venerable—and un-illustrated—Sammlungen of W. Deecke in SGDI i (1883), of R. Meister, Die griechischen Dialetke I (1889) and of the admirable O. Hoffmann, Die griechischen Dialetke II (1891). Indeed, any further reference is in general rendered superfluous by the thorough and accurate documentation which accompanies the texts, alike those of these ancient collections and the many additions of the last seventy years. We may also emphasise M.’s profitable exploitation of the Nachlass of Ernst Sittig, an epigraphist of distinction who maintained a life-long interest in the inscriptions of Classical Cyprus; and his diligent, if less successful, pursuit of the letters and unpublished papers of other scholars. Very welcome also are certain important discussions with which the texts are prefaced. These include a chronology of archaeological research in Cyprus from the year 1800 until the date of M.’s publication—to be read, however, in conjunctions with Myres and Richter’s Chronicle of Excavations in their Catalogue of the Cyprus Museum (1899)—and a study of the origins, character and use of the Classical syllabary, in which of particular value is an analysis of the ‘règles d’emploi’, pp. 68-78, superseding all previous treatment of this difficult topic. Much useful, if unoriginal, material is also to be found in the brief introduction to each city or important site, and again in the numismatic summaries which accompany the texts of Paphos, Curium and Mariam. A resumé, p. 189 f., of the problems occasioned by Cessola’s fabrication of his ‘Treasure of Curium’ is exemplary for thoroughness, clarity and good sense.

Some will not welcome, however, the arrangement of texts under the modern administrative districts, resulting inter alia in the insertion of non-Paphian Dhroumi (nos. 84-88) among the sites of the Paphian χωρία and in the separation by the intrusion of Mariam of Paphos from the Curium to which until the close of the Archaic period it was intimately related—but this is a relationship of which M. was then perhaps unaware. Nor will the reader be unduly impressed by the six tables of signs, figs. 1-6, for these are in fact perfunctory and defective (nos. 2, 5 and 6), have no palaeographic value and are, with the exception of no. 4, le syllabaire étrochypriote, not indebted to M.’s own researches.

From these minuities we must turn to criticism of a more fundamental nature. M.’s avowed intention to furnish each inscription with an epigraphic commentary may perhaps mislead, since the reader might well suppose that the texts as here presented have become, as the outcome of M.’s own collations, in some measure definitive. Nowhere does M. explicitly inform us that he has revised any single inscription in the original. On the contrary, he frequently protests—for example, under nos. 4, 212, 213a, 253a, all important documents which are thoroughly accessible—that ‘une revision serait nécessaire’—as if, having thus rubbed his button, it was now incumbent on some subordinate spirit to materialise and perform this humble task. Two important inscriptions of Agia Moni, nos. 90 and 91, which are carefully protected and may readily be reached on an afternoon’s excursion by car from Klima, M. has not seen and would have removed to a museum because ‘acces est fort incommod’. No. 352, doubtless the most interesting as it is certainly the longest syllabic document to appear within the past forty years, is preserved in a private collection in Larnaca, less than an hour distant from Nicosia: M. rejects the published text at a crucial point, not on the strength of autopy, but on the alleged evidence of ‘plusieurs observateurs’ (anonymous). It
would be tedious to press this point further. M.'s *modus operandi* is frankly of the study, and it is there that he marshals his texts, those of his predecessors with a trust they do not always justify, those of his contemporaries with an asperity not always deserved —using to control his judgments the photographs, often technically excellent, furnished by many museums. This work is in itself a handsome testimony to what may thus be achieved—but its value can only be enhanced by a frank recognition of the limitations of a method which on final analysis is not epigraphical. For it renounces *ab initio* the sound practice of modern epigraphy of studying each document three-dimensionally, so that monument and inscription may be mutually illuminating. With any difficult or damaged syllabic inscription, moreover, photography is no substitute for a study of the original, since it is in the nature of the syllabary that the apparent value of signs can fluctuate wildly with the direction of light. Museums, finally, tend to reproduce prints of their original negatives, so that a vicious circle may develop which can be broken in certain cases only at a punitive cost.

That this is valid criticism can be shown by a consideration, for example, of the syllabic inscriptions acquired from Cesnola by the Metropolitan Museum of New York, perhaps even today the most important collection of these in existence. Here M.'s texts are basically still those of that controversial amateur, Cesnola. But, we shall be told, all these inscriptions were collated by R. Meister in 1911 and again by J. L. Myres in the year following. Meister died in 1912, leaving his unpublished collations at the disposal of Myres—but the value of Meister's work has been severely criticised among others by M. himself (p. 124); while Myres is instructed by a letter in the archives of the Metropolitan Museum to devote no great care to the inscriptions since these were the special concern of Meister. And those admirable photographs, which the museum—for a consideration—distributes? Their subjects were posed, in some instances chalked (no. 179) and even pencilled (no. 254) seemingly by Cesnola himself. In these circumstances a certain diffidence with regard to the Metropolitan's syllabic texts is inevitable. That it is amply justified was impressed upon the reviewer by his recent examination of the originals in New York. To confine our scrutiny to the ten inscriptions of Curium, M.'s nos. 176–181 and 184–187, which include the famous inscribed pieces of silver and gold from Cesnola's treasure: of these one text only, no. 176, is correct, and nine in varying degrees defective. Furthermore, the silver bowls have merely to be handled to disclose two further syllabic inscriptions, never published simply because they are not shown by the illustrations of Cesnola's Atlas—and consequently by the reproductions of these which the museum has long issued. The inscriptions of Golgoi, with some three exceptions, are in a condition almost equally deplorable.

Nor should the reader look too expectantly to M. for elucidation of the obscurities which still abound, more particularly in certain of the longer inscriptions recently published. M. moves with authority among the opinions—and especially the philological opinions—of a past generation. He assembles accurately and critically the whole achievement of scholarship on this neglected front—and herein lies the great merit of his work—but is reluctant to assist at the birth of a new inscription, unfamiliar in his reception of new ideas. In his no. 354 NU.MU.pai.is = Νευρας of the original editor is rejected—in favour of θεορης; (!) suggested by another—because 'la notation de la nasale est impossible'. 'Impossible' is a dangerous word to use in syllabic epigraphy, for precisely this 'notation' is to be found in M.'s own no. 231—and in an unpublished inscription in the reviewer's possession. In no. 92 it is not easy to agree with M. that the κωδιγγωτος of Hoffman 'parait sür', since it can be achieved only by surgery too severe to be justified. M.'s reticence in the face of no. 318, almost the longest as it is certainly among the most obscure of Cypriot inscriptions, is very pardonable; but neither the text nor the interpretation of no. 327, its rival in length and its superior in value, have benefited from M.'s attentions. The *inedita*, nos. 166a, 342, 343 and 367, although of no great significance, are welcome.

St Andrews.

T. B. Mitford.


This most recent compilation of the Pylos material comprises some 1,180 texts, labels and sealings. The presentation is attractive, the print clear, the margins ample. The conventional editorial diacritics are carefully used, if less adequate than the Wingspread code; however, the Latinised transcription of ideograms of that code was inspired by this book, and Bennett has issued an index of Gallavotti's (as of Ruijgh's) specialities. The latest tablets in this corpus are the Blegen-Lang finds of 1960, yet Chadwick's Racine paper of September 1961 on Un 1322 is cited (but Geiss on Jn 725 was apparently inaccessible). The classification of the texts is revised in that Bennett's major classes are re-divided (but not re-lettered) on the basis of the presence of place names or key terms (e.g. *oromo*, *qaastra*) or specific groupings (*oka*, *akora*, etc.) or totals, and so on. Some curiosities (e.g. *kapara*δε, *kinidi*α, *korki*α as 'types' of women on pp. 5 f.) may slow up search for a text; but there is a comprehensive index of tablets. The signs and ideograms are depicted at the front of the book; and at the end the syllabary, the signs used ideogrammatically, and the Latin ideogram-names are listed and the ideogram occurrences logged. A copious bibliography precedes each major section, and the apparatus criticus gives the source of all
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conjectures. The reader will find his fingers con- 
sumingly in several pages at once—but always so in 
this kind of book.

The transcription of signs is conservative and the 
numbers alone appear for 34, 35, 56, 63, 64, 79, 82, 
83, 86. ųu is written for 85, whose chequered career 
is kept dark; as some reserve this value for 79 Gallav- 
votti is ready to substitute xu, at least the ninth 
suggestion for 85. Two is regular for 66, although 
Bennett’s corroboration of this value from the 
correction at An 261.1 some time around to prefer a separate 
91, keeping 66 in e.g. rauaratija An 298.1 etc. (rauara- 
tija An 830.11) where Gallavotti writes rauaratioo. 
At Ma 397.1 a two is read; if the reluctance of 
Bennett to recognise 85 in the second sign is regarded 
as justifying here a variant of asiatija (Palmer’s value 
for 85 being disregarded), why write two finally?

The new arrangement has anomalies (cf. An 199 
among ‘women’ tablets) and still obscures the 
affinity of An 1281 to Fn 50, 867 and the identity of women’s 
ethnic and numbers on An 292 and on Aa 788, 792, 
354. In the Ma series, 378.2 should probably read 
‘C 1 D 2’, and at 393.2 (as well as the scribal vagaries 
described by Chadwick, Decipherment, p. 123) the 
tablet offers ‘F 550’, not the expected ‘F 450’. On 
Pn 50.2 read ‘LECT 23’. The last sign of Xa 
1419.1 looks more like the cited jo on the tablet itself 
than on the drawing (see Lang, AFA 65, 1961, 
plates 58, 59).

Already, as this notice is written, the 1961 and 
1962 excavations have added 23 new tablets (1423– 
1445) and enlarged five known texts (Mb 1366, 
1402, Un 6; Ma 216, Na 527). A dozen articles 
need adding to the bibliographies, all speedily found 
in the pages of Nestor, 176 to date.

University of Durham.

N. E. COLLINGE.

DEROY (L.) Initiation à l’épigraphie mycénié- 
enne. (Incunabula Graeca, 2.) Rome: Edi- 
text figures. L. 2,500.

This book, the second volume of a new series 
sponsored by the Centre for Mycenaean Studies of the 
University of Rome under the direction of C. 
Gallavotti, is intended, according to the preface, as 
an introduction for beginners in the study of Linear B 
texts. Its first seven chapters accordingly describe 
the documents, list the editions, indices and vocabu- 
laries, and give a detailed account of the script and 
the system of classifying and numbering the texts, 
with a brief history of the decipherment. The 
type and quantity of information supplied in these 
chapters seems well suited to the book’s expressed 
purpose, although there is room for a number of 
corrections and additions of detail. For example, 
the tables of syllabic signs and ideograms might with 
advantage have shown the local variants; not all 
would agree that the value of sign 82 has been estab- 
lished as se₂ (p. 92); ai-ka-su-ma could usefully have 
been quoted in connexion with the spelling of con- 
sonant groups (pp. 98–9) and da-mo-de-mi pa-si 
(PT Ep 704.5) to illustrate the principles of word- 
division (p. 100). Chapter VIII, ‘Les flexions 
nominales et verbales’, has some regrettable features. 
In presenting the declensions the author unfortun- 
ately decided to use a single stem-word throughout 
each paradigm, even when this meant inventing un- 
attested forms. Many of these are fairly harmless, 
though they should have been starred, but some imply 
controversial assumptions or false statements. For 
example, since tu-ka-te thouro is used for the 
paradigm of r-stems, the genitive singular and dative 
plural are exemplified by invented tu-ka-te-ro, 
tu-ka-te-si, explained as thouetary, “thouetary, 
with footnotes to point out that Attic has thouetary, 
“thouetary.” Thus to foist on thouetary forms attested 
in other types of r-stem is unjustifiable. tu-ka-te-re, 
a genuine form, is explained by thouetary, and 
the more probable *thouesperes is overlooked (p. 103). 
ne-wo-i, ne-wo-(-i) (both fictions and the latter a 
conflation of two forms) are confusedly glossed 
vicos, vicos respectively (p. 101). The fabrication of 
te-ko-to-si, glossed tēkōsoi, implies an opinion about 
the dative-locative of nouns in -ow in Mycenaean, 
while the alternative suggestion ‘ou encore tēkōsoi’ 
proposes a form that never existed and could not, if 
it had existed, have given rise to tēkōsoi (p. 104). 
For comparison mainly Attic forms are used, with a 
few Homeric forms or reconstitutions where these 
are thought more appropriate. Recourse to 
these is inconsistent: vicos for ne-wo (p. 110), but 
*ēkōsoi for e-ko-si (p. 107). The declension of 
neuter n-stems is grafted onto pe-ma; in consequence 
the dual exemplified by a-mo-te is overlooked (p. 
105). Among verb-forms-a-pe ἀπη is listed with no 
hint of reserve (p. 108). The following chapters deal 
briefly with the characteristics of the Mycenaean 
dialect; place names and personal names, with a 
useful account of the criteria by which they are 
recognised and a glance at the geography of the 
Pylvan kingdom; the scribes, their method of work 
and the possibility of distinguishing their hands. 
Strangely, there is no unified select bibliography to 
guide the beginners’ next steps. The last chapter 
lists the bibliographies of Mycenaean studies, giving 
much less than its due prominence to Studies in 
Mycenaean Inscriptions and Dialect; apart from that, 
a number of books and articles are mentioned in the 
list of abbreviations and in footnotes. This lack of 
system means not only inconvenience for the student 
but also the inevitability of omissions, such as that of 
the Mycenaean appendix in Thumb-Scherer. The 
exclusion of matters connected with the contents of 
the tablets leaves the reader without a guide to the 
literature concerned with the interpretation and 
explanation of the texts. If further editions provide 
opportunities for revision, useful additions would be 
an account of the different classes of text (personal, 
land-tenure and so forth) with their characteristic 
words, ideograms and formulæ, and a more detailed
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account of Mycenaean paleography and the idiosyncrasies of individual scribes.

The book is handsomely printed and bound, and contains a number of well-produced tables of signs and ideograms, facsimiles of tablets and one photograph, and a sketch-map of the western Peloponnesus.

D. M. Jones.

Westfield College, London.


This admirable publication might well serve as a model for any Museum Director undertaking to publish an inventory of the epigraphical contents of a local museum. We are given first a brief survey of the growth of the collections of Greek and Latin inscriptions now in Vienna, of which the beginnings can be traced back to the sixteenth century, followed by an account of the methods adopted in compiling the inventory and of the plan which guided the arrangement of the contents, which involved extensive reorganisation.

Each entry shows the dimensions, state of preservation, finding-place, date and source of acquisition, and all essential references to previous publications, with a note on the location of each (e.g. Ausstellungssaal, with room-number, Depot, Lapidarium, etc.). The few, and mostly unimportant, Greek inscriptions which are unpublished are transcribed, whilst there is a brief summary of the contents of the others, which will suffice for identification with the aid of the lemmata. Ten of the most interesting items (four Latin, one bilingual, five Greek) are shown on the Plates.

The texts on marble or stone are arranged on a geographical basis, followed by those on bronze (nos. 419-433), of which the first is the Amnestydecrees (fourth century B.C.) from Olympia (Schwyzer, DGE 424); the others, all in Latin, comprise the SC De Bacchanalibus, set in an elegant late baroque frame dated to 1727 (when it arrived as a gift to the Emperor Karl VI), two fragments of the opistographous stelae with the Lex Aelia of 123/2 and the Lex Agraria of 111 B.C. of which there are more fragments at Naples, and eleven Diplomata covering the period from Nero to Severus Alexander (all published in CIL xvi).

The Greek inscriptions, comprising about a quarter of the total of 433 items, come from various regions of the eastern Mediterranean, of which Asia Minor and adjacent islands furnished upwards of 60, and of the 25 among these from the Austrian excavations at Ephesus all the more important have been published in Forschungen in Ephesus or the Jahreshefte. An eastern origin may also be inferred for several of those classed as 'unbekannten Fundortes' (nos. 77--107), mostly tomb-stones on which a common formula is -"γερνστε χαίρε

These come, with four exceptions, from a collection made late in the eighteenth century by the Marchese Tommaso Obizzi of Cairo near Este, but only acquired by the Museum in 1923, and they all are to be found in CIG. The few stones from Athens and Attica are of no special interest, but among those worth a passing mention are the long and complete Hellenistic decree passed by the Mityleneans in honour of judges from Erythrai (Schwyzer, GDE 623), the decree from Olynyhos recording the treaty between King Amyntas and the Chalkidians (Syll. 135), a tomb-stone from Carnuntum with a Greek elegiac distich (no. 372, Peak, GVI 832), and a copy from Ephesus of the apocryphal letter from Abgar to Jesus Christ (no. 59).

The format is handy (20 x 14 cm); the text, in ‘offsetdruck’, is easy to read and commendably free from errors (I have noted only Hinks (for Hicks, no. 61) and equities for equites (no. 113); and an occasional slight unevenness in the spacing, e.g. TUR TURZ (no. 59)).

A. M. Woodward.


This volume makes another substantial contribution towards completing the Corpus of the Greek and Latin inscriptions of Syria. In upwards of 700 entries the editors have covered the region of Hom (Emesa, which alone accounts for 300 items) and sites to the south and east of that important city, on both sides of the upper valley of the Orontes, which can be easily located on their sketch-map. The area covered has already been indicated in SEG XVII, 773, and two of the more important texts from Hom are published there (nos. 755, 756). The learned and ample commentary attached to each item, whether a text of historical interest or a fragmentary tomb-stone, or merely an inscribed gem or an amphora-stamp is a feature which distinguishes this work from the normal conception of a Corpus, and the full lemmata and elaborate indices deserve special commendation.

A. M. Woodward.


Professor Turner has taken over from Mr Edgar Lobel as editor of this famous series. He is responsible for most of the dramatic pieces and for the fragments of Satyrus, while Mr Rea has edited nearly all the other numbers. Dr Koenen and Dr Pomar have
each contributed the edition of one document, making the team truly international. The high scholarly level known from previous volumes has been maintained throughout.


There is no Aeschylus this time, so the new dramatic fragments begin with Sophocles. 2452 is assigned to him, numerous fragments from his Theseus, but only five of them sizeable. Next to nothing was known of this play before 1962, just one two-word fragment, ὁμμίτων τέρπεις, fr. 246 in Pearson's collection, vol. i, pp. 184-5. These words have not been found in the new fragments but may well come from the same scene as fr. 2 which deals at some length with the sky, mentioning άστρα (7), άστρεψ (9), άστρατήχες (14). I am not sure if Turner is right in his transcription of lines 7-9:

ο μὲν τις άστρον προς
κακός τ’ έγωνος κομάς
έδεινημ’ άστράτηκον

In his copy of what he has been able to decipher he prints in 8 κακώτ’ εγώ . . κα . . of since the two letters after εγώ have practically gone. As there seems to be an antithesis here, ο μὲν τις on the one hand, the speaker on the other, a δε should follow the word εγώ rather than a γε. 'Many a man [is ignorant] and unskilled in star lore, I however thus understand the . . . of the stars.'

In fr. 3 where Theseus is the speaker about two-thirds of a dozen trimeters have been preserved. I quote lines 18-19 about the sow of Krommyon with the lacunae partly filled in by the editor:

ατέν έμπορόν δηλημένον’ ἢν [’έγω ὧ—
ἐπανα δενοῖς κάσωσετάτοις πόνος.

In fr. 4 both Ariadne and Eriboia appear in a lyrical passage, the former lamenting in trimeters the δύστηρα τέκνα, the latter responding in doximasia.

2453 contains a great number of small fragments from the Polybios or Mantis of Sophocles and from other plays, a continuation of 1063, for which I refer to Turner's Introduction. Although the title-page mentions only the translations and notes, these introductions to the various pieces are also of the greatest value, and the student would be just lost without them.

2457 Hypothesis of Euripides, Aelous, line 26: one of the daughters of Aelous has given birth to a child but hides it τω νοσεν τη . . with τοι at the beginning of the next line. The editor translates this as 'by pretending sickness' and in a note states that the restoration τροποφοβηθαυ and advord would give a considerably longer line than usual. I see only one remedy here, τροποφοβηθαυ τοι, 'by being ill by way of pretext'. This is awkward instead of the simple νοσεν τροποφοβηθαυ, but seems inevitable.

The Cressophon of Euripides was admired by Aristotle, and the appearance of a couple of lines in 2458 is welcome indeed. There is a stichomythia of some fifteen lines with the proper names Polyphontes and Cressophon to guide us. For details see Turner's learned introduction pp. 73-77. There is a stichomythia dialogue again in 2461, Euripides, Theseus (?). So Turner in the title, but in his short introduction he says that the play might be the Cretes, and Professor H. J. Mette (Hamburg), writing in Hermes 91, 1963, 256, has since shown that this is so in fact: the very small fragment 4 is part of fragment 472 N, lines 12-15.

Among the new prose texts Satyrus, On the Demes of Alexandria is remarkable. Fragment 1 gives a long genealogy from the god Dionysus via Deianira down to Perdikkas and Aeropos, fr. 2 regulations for the cult of Arsinoe Philadelphos. Lines 18-19 τού δὲ βασιλικος ποιητικος πάντας ες δικον, a strange material for the building of altars.

Extant Classical Texts: after Menander, mentioned above, comes Plato, Politicus 261D-262C and much of 257 B. 2469 is from an obscure historian, Aristodemus, No. 104 in Jacoby, Fr. Gr. Hist. II A.

There are ten documents ranging in time from the first to the sixth century. 2474, edited by Koenen, acquaints us with the will of a Roman citizen from which I would like to quote two specimens. The testator does not forget his mother and gives her address too, l. 13-14 ἡ δὲ ἡμιτέρα μητήρ Ἀσκληπιδάρου ἐν βέων Ψέλλων λεγομένη. L. 25-26 say with a euphemism ἐν δὲ, ἢ μη εἰ, ὄποτες τῶν αὐτῶν παιδῶν μον ἢ ἀπόρρεινται ἄτεκνοι ἤ και ἀδιάδεδοι κτε.

Careful indexes on pp. 201-224 conclude the volume.

W. Morel.

London.


As a result of collaboration between British and Swiss scholars the papyri forming the archive of Flavius Abinnaeus, a garrison commander in the Fayyum in the middle of the fourth century, are here published for the first time in one volume. It is a pleasure to welcome the appearance of this book which has for various reasons been long delayed.

There is a substantial introduction with sections on the discovery of the archive, Abinnaeus himself, military questions relevant to the collection, and Abinnaeus' correspondents, including a study of
epistolary formulae. Many interesting points arise, among them the suggestion that the name Abinaeus is of Syrian origin, and the likelihood that the archive is not from Dionysias, where Abinaeus commanded the fort (now excavated), but from Philadelphia, where he presumably spent his retirement.

Of the text themselves the first two are in Latin. 1, a petition from Abinaeus to the emperors, is an important document and receives a thorough discussion. The official and private letters that follow (2-38) form the core of the archive. Senders include the duus Valacius (2), a procurator (3), an exactor (13-15), officials both military (e.g. 10-12, 16, 26) and non-military (e.g. 9, 18), and members of Abinaeus' household (21-25); the majority of the correspondents are clearly Abinaeus' inferiors. The next section (44-57) contains petitions sent to Abinaeus, among them several from civilians concerned with purely civil offences (e.g. theft) or requesting his support against powerful neighbours (cf. 59, 53, 55); they give the impression that Abinaeus had extensive jurisdiction de facto in the district surrounding his camp. The volume is completed by several contracts (58-65) and miscellaneous accounts and lists (66-82); they include at least one important document, a report of a trial before the iuridicus in which the defendant is Nonna, Abinaeus' wife (62).

The archive has something of value to contribute to many different fields (though, surprisingly, virtually nothing for the history of Christianity). Economic and social historians will find it a mine of information, as will those interested in the development of the Greek language. The papyri abound in peculiarities of orthography and syntax (see especially 31 and 36), some of them being forms (like έσουθ for έςουθ) which point towards later Greek. The importance of the archive for the military historian is considerable; it is particularly illuminating on the relations, often strained, between civil and military authorities and occasionally gives a glimpse of the terrorism that troops could exercise over a subject population (e.g. 18 and 28).

As the bulk of the papyri have long been available to scholars and those newly published are of little value, it may be wondered whether publication of this work was worth while. The answer is that it was, for two reasons in particular: first, the texts are much easier to comprehend now that they have all, wherever possible, been translated; it seems a pity, however, that in a volume which will be of such value to non-papyrologists, it was not found possible to add more notes, particularly to facilitate the understanding of the Greek (strange looking forms are often merely 'corrected' in the critical apparatus without further explanation). The second advantage is that the texts are now greatly superior to those in earlier editions; comparison of the two shows that almost all have some improvements and these are often substantial. The editors have not of course solved all the difficulties. A combination of a difficult reading with eccentric Greek has occasionally left them baffled, but a study of the available facsimiles gives some idea of the problems they have faced. At 7, 5-6 I should read πάντα γίνεται (I. γίνεται) καλά. 'all things turn out well', possibly followed by ἐν γορία for ὑν γορία; the name in 18, 17 seems to be quite clearly Αλκάμους; after όι άδυτοι in 48, 25 ἦκε ἐνόπλος may be tentatively suggested; the facsimile leaves no doubt that the text as printed here, at any rate the marking of uncertain letters, cannot represent the editors' reading. Unhappily it must be stated that errors and misprints of this and other kinds are much commoner throughout than would be expected in a work of this nature. I mention here only 2, 4 monusque for nunc usque and 59, 15 ἀδύτε κ.κ.λ., where translation and critical note are at variance. Fortunately most of the errors are so trivial or obvious as to cause no difficulty, though those in the indices are rather more serious.

A few detailed points may be mentioned. Aetius, sender of 4 and 5, may well be a military, not a civil official. The suggestion in 4, 8n. that τί is sometimes found for δι (as distinct from δι alone) seems questionable. The preamble to 19 says that it is 'not unlikely' that it is from Mios, but the introduction, 30 ff., treats ascription to him as certain, and the index wrongly says that Mios occurs at line 28. The description in the introduction to 58 of CTH. xii 6.22 as 'the unique argument in favour of the profitable character of the exactoria' is perhaps unjustified, cf. CTH. xi 1.3; 7.1; Nov. Maj. vii 14-15. These are of course minor points; they do not detract from the fact that the editors have produced, at a comparatively cheap price, a very useful collection of papyrus texts.

J. D. Thomas.

University College, Aberystwyth.


This volume in the famous Munich series of papyrological and juristic monographs was completed in essentials as long ago as 1941, when its author presented it as a doctoral thesis. Unfortunately, a nervous breakdown and the international situation prevented its immediate publication, and the sole indication of its existence was a summary of contents which appeared in Z. Sav. St. 62. The editors of the series, W. Kunkel and H. Bengtson, are to be congratulated on their decision, supported by the recommendation of H. J. Wolff, to produce it more or less in its original form; they have been helped by J. Hermann and D. Nör, who have added references to post-1941 publications, and D. Simon, who has undertaken responsibility for corrections and compiled an index of sources. The finished work is a tribute to the devotion of these scholars, as well as to the ability of the author, tragically prevented from
reaching the full maturity of a career whose promise was amply attested by his earlier monograph on _perduelio_.

The volume falls into four parts: an introduction, outlining the main problems connected with clauses of liability in _vanductio_, and three chapters. The first chapter is devoted to an examination of the papyrological evidence in the hope of answering the two main questions engaging the attention of those who have investigated the Roman _receptum nautarum_ since Mittertis: (i) whether clauses of liability in _vanductio_ represent necessary and formal assumption of liability by the _nauta_, or whether a similar liability was involved even without such clauses; (ii) the extent of the liability expressed in the clauses in the different historical periods through which they can be traced. The first section seeks to establish that the clauses of liability in _vanductio_ are a _naturale negotii_ and raises a doubt, based on the existence of a clause of liability even in the Ptolemaic period, as to the justification for the assumption that the later, post-Ptolemaic documents imitated Roman prototypes. The second section compares the clauses with the Roman _receptum-formula_ and other clauses of liability in papyri and native Greek sources. The third examines in this connexion the Latin document P. Grenf. II, 108, suggesting three possible interpretations but rejecting the idea that it is a _receptum and_ therefore, a connecting-link between the Roman legal sources and the practice in the papyri.

After a detailed analysis (in the second chapter) of the Roman legal sources for the liability of the _nauta_ and related problems, Brecht finally compares conflicting views and states his own conclusions in chapter 3. He sees powerful objections both to the view generally adopted since Mittertis—that clauses of liability in _vanductio derive from the receptum nautarum—and to the suggestion of Partsch recently adopted by Luzatto—that the Hellenistic law of transportation supplied the pattern for Roman _recepta_. His own view is that a form of agreement containing an explicit clause of liability is found as early as the middle of the last century of Ptolemaic rule, quite independently of Greek or Roman legal concepts, that this _vanductio_ exhibits no essential change in structure in the Roman period, and that it is without doubt a kind of _μετοκος_, even if adapted to the special conditions of shipping transportation, and whether or not it contains an explicit clause of liability. An examination of the possible effects of the _Constitutio Antoniniana_, in which the editors were unable to include a reference to C. B. Welles’s review article in _Et. Pap._ IX, shows no grounds for revising this view, nor is there any perceptible change in the form of the _vanductio_ as a result of the deliberate bias towards the Roman legal system under Diocletian; the traditional system of shipping transportation on the Nile prevented the Roman formula of the _receptum_ from exercising any influence on the _vanductio_ and continued to do so until Justinian.

The possibility of finding a parallel between the _vanductio_ and the _locatio-conductio nautae_ is then examined and leads to a consideration of divergent views as to the nature of the legal concepts of _custodia_, _diligentia_ and _επιμελησι_, and, in this connexion, of P. Lond. II, 301 in particular. The legal subtleties of this final discussion, as indeed of the whole volume, merit the close attention of classical jurists, and papyrologists will need to note the suggested corrections briefly listed on p. 155.

_B. R. Rees._

University College, Cardiff.


Dr Ijszewijn’s book becomes the standard work on this arid but indispensable topic. He lists the priests year by year; examines them prosopographically as individuals and as a class; and collects the evidence about the creation and constitution of the priesthoods themselves.

The list brings up to date that in _Prosopographia Ptolemaica III_ (1956): new evidence (almost all Demotic) fills gaps in the fasti for 266/5, 200/199, 195/4 and 180/77, and supplements the documentation of seven other years. It falls short of the ideal in three respects. (1) I wish prosopographic conjectures had been altogether excluded: e.g. 272/1 _Καλλικράτης_ (Boscov?) (2) Some untidiness could have been avoided by recalling old papyri: e.g. 273/2 Neomedes or Nearchos? (3) There is clearly much uncertainty in the reconstructing of Greek names from Demotic transcriptions: but we are not offered any criteria of judgement, even the general criteria of _Prosopographia Ptolemaica III_: it would be something to have a statement that no such criteria are possible (contrast Lexa, _Grammaire Démotique_ I §26). Nonetheless, this list is extremely valuable: it is well set out; the references, where I have checked them, are uniformly accurate; and there are no very serious misprints.²

The prosopography which follows provides a useful collection of material, though some at least of the identifications seem over-optimistic (especially the filiation of 273/2 Neomedes—_φυλή_—_ακλίνος_ in _P.Hib._ 110 gives a false division of syllables). Various details might be added: p. 34, n. 2 on the provenance of SB 3996, see Fraser, _JEA_ 41 (1955) 135—a later Dioeteles _Berytus_ xiii 142 ff.; p. 64 a Boiscus under Philopator, _Berytus_ xii 112; p. 68 Mitford’s discussion ¹

1 Evidence, which Wallbuck lacked, for the 6th year of Epiphanes ( _JEA_ 22 (1936) 32)—Ijszewijn, p. 38.

2 No. 20 can refer to _Acta Orientalia_ 25-252. No. 23 can write _ηυ_ [ ]_. No. 62 can. the Demotic omits the father’s name Khritn. No. 75 n. 2 write SB 3996. No. 102 write _Προκεχαμικον_. No. 128 write _κοκ_ .... 04. No. 150 add _PAhm_. 44.18-22.
of Ptolemy son of Pelops is JEA 46 (1960) 109–11; p. 70 Glaucos’s Olympic victory might be mentioned (Paus. vi 16.9); p. 76 SB 7266 should be BGU 1211—attributed to Philopator only by conjecture (cf. Zuntz, CQ, 44:70); p. 81 f. on Chrysarmus ἐν τῶν ἱερῶν, Chronique d’Egypte 32 (1957) 332.

In discussing the priesthoods, Ijjewijn makes a new and important observation—each pair of deified Ptolemies makes its first appearance in a year in which the Ptolemaia were celebrated, the deification therefore most probably took place at this festival. The only exception before the death of Euergetes II (Eupator in 153/2) is a sign of crisis. All the other eponymous offices do or may originate in a festival year; so does the cult in Ptolemais. New papyri are brought under consideration: P.Hib. 199 dates the first appearance of the θεὶς ἄδελφος to 272/3 (Ijjewijn reinforces the supplement from Welles, Royal Correspondence, no. 36, 10–17), and P. Oxy. 2445 gives new details about the canophoros. p. 134 Callixenus need not be referring to the Ptolemaia; p. 136 f. ‘Athlophoros’ surely means ‘one who carries the prize’ (in a procession)—Demotic ‘she who bears the prize of victory of (sometimes in the presence of) Berenice’. Mr D. M. Lewis remarks that ἄδελφα were carried in the procession described by Callixenus (Ath. 198 c).

Ijjewijn goes on to discuss the nationality, family and careers of the priests—useful tabulations, though often (necessarily) inconclusive. He decides, reasonably, that only one qualification for office was indispensable: royal favour.

Hence the interest of the fasti for political history. Ijjewijn’s study throws up much of importance. The priest of 247/6 was still in office in 245/4—a reflex of the Syrian crisis? The fasti for 204/3 reflect (once the identifications are accepted) the power and the fall of Agathocles—his ally Aristomenes in 204/3, his countryman Satyrus in 203/2, but in 202/1 ‘Telephorus’ creature Adaesus. Soter II served as priest for at least eight years between 116 and 107—why? Ijjewijn’s accurate and careful book makes possible a study of these wider aspects.

P. J. Parsons.

Christ Church, Oxford.


A firm disbeliever in the autonomy of ideas and deeply influenced by E. Meyerson’s dogma that man is the product of his history D. has made a notable attempt in this short work to define the debt which he believes Pythagoras owed to Homer and Hesiod. He bases his thesis on Porphyry’s statement, which derives ultimately from Neanthes, that Pythagoras himself was a disciple of the Homeridae, and concludes that it was in Samos that Poetry and Philosophy originally met. The possibility of an earlier meeting in Ionia is ignored. D. is right, of course, in maintaining that Homer and Hesiod represented a kind of collective unconscious for the sixth century B.C. and it seems possible from Aristotle’s statement that the Pythagoreans undertook an exegesis of Hesiod, as well as from glosses deriving from Androclydes and similar sources that the Pythagoreans differed from the Orphics in drawing upon epic for allegorical material to illustrate their own doctrines. The kind of ‘anthologies’ which the Pythagoreans may have used is then assessed by D. The heroes at their best behave like true philosophers and even Achilles when not blood-letting possesses many traits of which Pythagoras could have approved. He takes, for example, a cathartic joy in music, raises the Apolline paean, purifies the libation cup before sending Patroclus to fight, and is destined, like the Pythagoreans themselves, to find a home among the blessed. Both Ulysses and Pythagoras shared anonymity on occasion, and the former’s adventures, and in particular those concerned with Circe and the Sirens, provided ready material for sermons. Pythagoras indeed and not Theagenes of Rhegium was the true inventor of allegory.

‘Nulle représentation religieuse est aussi complexe que celle des héros.’ In the Iliad a hero is primarily a warrior, but Hesiod’s reference to ἄδελφοι ἄριστος, and the legend of the Choice of Hercules, paved the way for the heroisation of Pythagoras himself.

The thesis is well argued and documented, but depends by its very nature on hypotheses that are incapable of proof. On the other hand the author’s confessed awareness of the strain which he is obliged to place on slight evidence recommends him to the reader, and philologists and philosophers alike should find much that is novel to interest them here. There is a useful appendix of passages referred to in the text or notes and an index of ancient authors quoted.

J. R. T. POLLARD.

University College of North Wales, Bangor.


There are few new features in this portrait of Anaximander, though some details and the over-all synthesis are Dr Selman’s own. His brief account of the sources takes the fashionable view that Theophrastus is an authority independent of Aristotle: this fashion, in so far as it presupposes that occasional correction of master by disciple guarantees the disciple’s general independence, disturbs the reviewer. S. analyses some traditional interpretations of Anaximander, particularly Aristotle’s. Aristotle is blamed
for over-materialising the ἀπείρων and under-stating its divinity; but we know of its divinity from Aristotle, and S. perhaps underrates its materiality. That Aristotle’s conception of matter is irrelevant to Anaximander is true and cannot be over-stressed. S. rejects Cornford’s account of the Aristotelian mixture-interpretation on good grounds, Vlastos’ on grounds mostly shaky, except that the γόνυθον has little philosophical point if the ἀπείρων is a mixture. The point is well taken that the resemblance of Anaximander’s ἀρχή to Aristotle’s in being ‘primary and underived’ may have encouraged their assimilation in other respects.

The core of the book deals with the fragment in its context. The sentence ἐξ ὧν ἐδ η γένεσις ἄστι τοις οὖσιν, καὶ τὴν φθορὰν εἰς ταῦτα γίνεσθαι of DK 12 B1 is defended as genuine, but less convincingly than by Kahn; the dative τοῖς οὖσι could be designed to avoid the obscurity of τῶν ἄνωτων after ὧν, and Seligman exaggerates both the genetic aspect of Theophrastus’ account of the ἀπείρων and the analytic aspect of similar remarks in Aristotle. The plurals ὧν and ταῦτα are explained as of ‘generalised statement’, but this seems difficult to reconcile with the further interpretation of that into which things are destroyed as the ἀπείρων. S. appears to understand τὰ ὀντά as things in totality or ‘the world’, but worlds do not pay recompense to each other. Anaximander’s κόσμος are for Seligman innumerable successive worlds, but he mentions nothing in nature to induce this belief except for his unwarranted assumption that the drought mentioned by Theophrastus and the corresponding flood were world-destructive events. S. believes the penalty for injustice to be re-absorption into the aperion, but that seems difficult to reconcile with ἀλληλος in the fragment and with the normal operation of the ἰσος ἀλληλος.

S. seeks the source for the strife of the opposites not in the observation of natural changes but in the quarrels of mythical deities. In this there could be some truth, but cycles of birth and death and of crime and punishment are not explicit in Hesiod’s Theogony. Analysis of the ἀπείρων’s function leads S. to relate it interestingly to the mythical Oceanos: it need emphasising that in Archaic Greece Oceanos is not a ruling power, is the origin and goal of sun, not of world, and surrounds earth, not world. Oceanos may have been at the back of Anaximander’s mind, but Hesiod’s Chaos seems to the reviewer at least as important.

These, however, are controversial topics, and others may find here more to agree with than this reviewer. In some respects the book might mislead the non-Hellenist—for instance Aétius’ citation of Anaximenes is underlined in toto as authentic without comment, and a distinction between Anaximander’s prosaic and poetic styles is based on inadequate evidence. But students of philosophy in general, to whom the book is primarily addressed, will find an interesting account of the origin and function of metaphysics, even if the final justification of metaphysical speculation is calculated to convince none but the already converted.

Michael C. Stokes.

University of Edinburgh.


Dr Sprague attacks a notorious and difficult problem. Admitting, as we all must, that there are fallacious arguments in Plato’s dialogues, she tries to show that Plato was conscious of ‘an important number’ of these fallacies. She does not consider fallacies which Plato may not have been conscious of, but concentrates on the fallacies she considers conscious in an attempt to show that Plato ‘used’ these deliberately to contribute indirectly to the elucidation of his views.

Dr Sprague begins with a welcome analysis of the Euthydemus, a dialogue which often receives lip-service rather than detailed attention. She regards it rightly as a satire designed to expose eristic tricks in argument; she analyses these tricks as mostly turning on the fallacies of equivocation and ‘a dictum secundum quid ad dictum simpliciter’, and combinations of these. Now Plato makes Socrates himself explain the equivocation on μαθητή (277d–278d), and shows him insisting warily on the ‘secundum quid’ of some of his ‘dicta’ (293 b sq., 296 a sq.). Dr Sprague infers that Plato possessed ‘a complete mastery’ of these kinds of fallacy; she assumes that Plato when writing the Euthydemus fully understood the precise nature of the fallacies contained in all the arguments he satirised. On this assumption she argues that any reference to or use of these, or similar, arguments by Plato in other dialogues was accomplished there also by full understanding of their fallaciously.

Dr Sprague therefore proceeds to examine passages from the Theaetetus, Cratylus, and Hippias Minor where she detects various different uses of fallacious arguments similar to ones found in the Euthydemus.

Her discussion of the three arguments at Theaetetus 163 a sq. seems to me confused. Even if there was available to Theaetetus a legitimate sense of ἐννοεῖν = ‘perceive’ (which I doubt; the Euthydemus plays on ἐννοεῖν as (1) ‘knowing’, (2) ‘understanding’, but nowhere as ‘perceiving’), Socrates commits no fallacy in pointing out that not all ‘knowing’ is ‘perceiving’. By calling these arguments ‘antilogical’, (164 c), Socrates perhaps means that they are merely narrowly destructive of Theaetetus’ position in its present form, unconstructive rather than invalid.

On Cratylus 429 b sq. Dr Sprague draws attention to the similarity between Cratylus’ argument against false names and Dionysodorus’ argument against false speaking at Euthydemus 286 a sq. Dr Sprague analyses these arguments as fallacious because of equivocation between εἰρήν as copula and as ‘exists’. 
This, of course, raises huge problems. Plato himself analysed the 'false speaking' argument and some of the senses of ἑλοι in the Sophist; but many scholars would doubt whether he had mastered these enormous difficulties in his earlier works. In the Cratylus Socrates produces other arguments for false names, so presumably thinks Cratylus' argument against them fallacious; but he does not directly refute or analyse the fallacy, any more than he does in fact in the Euthydemus. The possibility remains that Plato when writing the Euthydemus and Cratylus was (justifiably) sure that these arguments must be fallacious without then being able to explain the fallacies in detail. It is a weakness of Dr Sprague's case that while Plato's explanation of the equivocation on μάθαινει at the beginning of the Euthydemus probably proves that he was aware of equivocation in general as a possible source of fallacy, it does not in fact prove that he was already able to explain all equivocations involving ἑλοι, or other words of similar philosophical difficulty.

On the Hippias Minor Dr Sprague seems to argue that the notoriously paradoxical conclusion justifies us in thinking Plato must have been conscious of the fallacies she detects earlier in the dialogue; but she herself quotes and adopts Taylor's plausible explanation of the final paradox, which would seem to restore the probability that most of the dialogue was meant perfectly seriously.

Dr Sprague also discusses points from the Laches and Meno, and in an appendix defends Plato's logic from some of the severer comments of Fr Bochenski. She is probably right to interpret Gorgias 507 α 5 as a statement of equivalence, not class-inclusion; this may well explain others of Plato's 'false conversions'.

These unavoidably curt criticisms must not deter anyone from assessing Dr Sprague's arguments for himself. Her book makes a systematic onslaught on important but neglected problems; her attempted use of the Euthydemus is especially suggestive.

David B. Robinson.

University of Edinburgh.


The time was more than ripe for a new edition of the Meno. E. Seymour Thompson's edition of 1901 has served scholars and students well, but Platonic scholarship has achieved much in this century, and Dr Bluck has done well to give to the interpretation of the Meno the benefit of recent progress. His work will be used and valued by readers of the dialogue for very many years to come.

The first duty of an edition of this kind and on this scale is to help the scholar or student with the detailed interpretation of the text, since this is the foundation on which the evaluation of wider issues must be built. It is here that Bluck is most successful. The very fullness of his commentary, which makes it a somewhat tiring work to read from beginning to end, is its great virtue as a work of reference. Bluck is not one of those editors who write about everything except what is important and difficult: he writes about everything. This means that some of his space is spent on what is easy or unimportant, and that he uses the phrase 'of course' more often than it should be used in this or any published work. But he also deals fully, cautiously and reliably with the crucial passages and the thorny problems of interpretation. There are typically helpful notes on 74d7 καὶ ταῖτα καὶ ἐνετά ἕτα ἄλληλος, where the reference to the Philebus is illuminating; on 80a5 (the φάραγος); on 82b8–9 (the role of the diagram in the discussion with the slave-boy); and on 97d5 (Daedalus).

One also looks to an edition on this scale for an account of recent work on its author and dialogue, and of work in related fields. Here again Bluck has been conscientious and judicious. His valuable Select Bibliography lists numerous works that have appeared since Thompson's day, and which he has used to good effect in his introduction and commentary.

Finally, one looks to an edition like this for enlightenment on the philosophical doctrine of its text, and for an account of the relation between the present dialogue and other works of Plato. Under these two headings Bluck is less successful. He rightly warns us against reading later Platonic metaphysics into the early part of the Meno, but when he comes to deal with ἀναγνωρεῖες, with the distinction between knowledge and opinion, and with the hypothetical method, he is dangerously anxious to square what Plato says in the Meno with what he says in the middle and later dialogues in general. In the Republic, knowledge and opinion have different objects: this is not so in the Meno, and Bluck does not give good enough reasons for his claim that 'the present account of ὅρη δόξα is quite consistent . . . with its treatment in other dialogues, where it plays an important part'.

He is too ready to see the developed theory of forms behind the arras of the earlier Platonic discussion of definition, hypothesis and virtue, and he is an adherent of that upside-down Burnet-Taylorism which sees the theory of forms persisting almost unaltered in every dialogue, however late, in which Plato talks of universals.

But it is where questions of Platonic interpretation involve substantive philosophical points that Bluck's judgments are most controversial. He does not do justice to all the sources and motives of Plato's attachment to mathematics as a paradigm of knowledge. The certainty of formal logic and mathematics has exercised a powerful gravitational attraction on the work of rationalist metaphysicists at all times and in all places, but classical commentators too often seek special and local reasons why Plato was so much affected by it.

There are signs that Bluck is himself a believer in some of Plato's own doctrines, and especially in the theory of forms, but he seldom, if ever, engages explicitly in the strictly philosophical controversy in
which this allegiance involves him. In his note on 86d5 he gives the impression that Plato could have produced an accurate definition of ἀπερτό if he had wished to do so, but neither here nor elsewhere does he support this claim, on which a good deal of recent philosophy casts grave doubt. This edition is an excellent representative of the classical tradition of sympathetic historical interpretation of Plato, but it is also typical of that tradition’s habit of crossing from scholarship into substantive philosophy without notice and therefore without due argument for its implicit philosophical theses.

Renford Bambrough.

St John’s College, Cambridge.


This attempt to determine the significance of ὁδὸς in Plato’s theory of knowledge faces formidable difficulties. In Plato ὁδὸς tends to be a chameleon type of concept, changing its colours to suit the surroundings of the particular philosophical problems under discussion. Precise and detailed interpretation of these problems is necessary for the elucidation of its meaning at any point. I do not think that Sprute’s study satisfies this condition. Many relevant passages are given only a cursory examination. One or two (e.g. Politicus 277d–279a) are neglected altogether. However, his analysis yields several general distinctions which will be found useful for an understanding of Plato’s handling of the concept.

After a review of the treatment of the problem of ὁδὸς in platonistic literature, Sprute first distinguishes (using primarily Theaet. 189e ff., Soph. 263d ff., Phil. 98b ff.) the constitutive elements of ὁδὸς. These are perception, memory and judgment. Then, with regard to these elements, he discusses three main problems, together with the further problem of the relation between these problems. They are (i) the relation between ὁδὸς and ἀληθὴς; (ii) the distinction between true and false ὁδὸς; (iii) the relation between true ὁδὸς and knowledge. His discussion leads him towards a distinction between two senses of ὁδὸς. There is the sense of ὁδὸς as empirical knowledge. This sense is most prominent in the Republic and the Timaeus; it assumes a distinction between ὁδὸς and knowledge of a higher kind which has different objects (the Forms), and it excludes the possibility of converting ὁδὸς to knowledge proper.

But Sprute rightly notes that Plato sometimes uses ὁδὸς in a sense which assumes that ὁδὸς and knowledge have the same objects. He refers principally to the Meno and the Sophist, emphasising that in the Sophist at least the assumption implies that ὁδὸς is directed to Forms. Sprute finds this assumption disconcerting, partly because he feels that consistency is to be expected in Plato’s usage, partly because he finds that there is now no ready answer available to problems (i)–(iii) above, as there appeared to him to be, in principle at least, when ὁδὸς was equivalent to empirical knowledge. He gives special emphasis to the problem of distinguishing true ὁδὸς from knowledge, since he considers that ὁδὸς now carries the predominant sense of judgment (Urteil), and that Plato’s analysis of the process of forming judgments (ὁδὸς/herm) makes it indistinguishable from the dialectical process whereby knowledge is gained. To save the distinction he appeals to Epistle vii for its idea of the final ‘flash’ of insight into reality which carries the mind beyond the level of ὁδὸς.

This use of Ep. vii is far too easy an escape from the complex problems of the relation between ὁδὸς and knowledge in the later dialogues. Moreover, the difficulties arising from Plato’s retention of the concept of ὁδὸς in its sense of empirical knowledge are nowhere properly recognised. But perhaps it is unfair to expect this short study to explore these difficulties.

University of Bristol.

Norman Gulley.


To the making of books about Plato there is indeed no end. Mr Crombie’s object in adding to their number—a task which he undertook in response to an invitation from Professor Ayer—is to bridge the gap between the writings of scholars and the general reader; but by ‘the general reader’ he means the contemporary student of philosophy, who all too often thinks of Platonism as ‘the practice of trying to solve logical problems by postulating metaphysical entities’. His title fairly represents his purpose: he believes that there are ‘doctrines’ expounded in the dialogues, and ‘examines’ them by explaining what they are, discussing their meaning, and—last and least—considering their validity.

The author of such a book faces an inevitable problem of arrangement. Should he, like Taylor, discuss Plato’s works dialogue by dialogue? Or should he follow Grube and others in moving from topic to topic, combining sections of various dialogues under each? Crombie’s solution is one of the least satisfactory aspects of his book. A chapter on Plato’s Life and Writings is followed by one deceptively entitled The Development of Plato’s Thought—an interesting discussion of the sources and meaning of the theory of Forms, not of the different phases in Plato’s intellectual history: ‘development’ in this latter sense plays little part in Crombie’s account. The third chapter is on the Republic, summarised here in order to present ‘the essential unity’ of Platonism. Thereafter, we turn to one subject after another: Politics (separate from the Republic!); Beauty, Art, Ideology, Rhetoric, Education (all covered in twenty pages); Ethics;
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Philosophy of Mind; and Theology and Religion. These are selected as ‘topics of more general interest’, while ‘more technical philosophical topics’ are kept for treatment in a second volume still to come.

One unhappy consequence of this pattern is a good deal of repetition. A more serious result is lack of unity. Those who think of Platonism as a synoptic vision will not find it here: in spite of his Republic chapter, and of many efforts to reconcile one dialogue with another, not only the planning of the book but the author’s attitude leads him to present us with fragments, not a unified whole. His approach is thoroughly sensible, down-to-earth, suspicious or even incredulous of those aspects of Plato’s thought which cannot be brought within these limits. In this volume, at any rate, diwvemevs gets little attention, and επος is practically ignored. The central books of the Republic, we are told, contain ‘ambitious speculations’ rather than ‘the essence of Plato’s thought’, so that ‘the discussion should be taken with a pinch of salt’; as for the treatment of sex relations in Book Five, ‘the most charitable comment to make on this passage is to suggest that Plato’s purpose is to pull the legs of those who attach undue value to family ties’. Crombie tends to regard the Republic as something exceptional and temporary in Plato’s development, and one cannot help feeling that he treats it in a separate chapter partly because he wants to get it out of the way. The last sentence of the book, primarily concerned with Plato’s theological position, might well be taken as Crombie’s general verdict on him: ‘It rather looks as if neither his intellect nor his imagination knew what the other was doing.’

When all this has been said, it remains true that there is a great deal of value here. The discussion of some aspects of Plato’s thought—his attitude to pleasure, for example, his use of the word προσφύζη, the tripartite division of the soul—is thorough, penetrating, and illuminating. One looks forward to further elaboration in Volume Two of the author’s view of the theory of Forms: he sees the essential clue to their status as ‘independent substantial things’ in the doctrine that ‘the organisation of the natural world is the work of a creative mind which is totally independent of that which it creates’—the Craftsman of the Timaeus. Most important of all, Crombie conducts his examination in a way which his intended reader, the contemporary student of philosophy, is likely to understand. His style, normally sober enough, makes occasional excursions into liveliness or colloquialism which will appeal to some and jar on others: the Spartans were ‘enormously tough’; the divine Craftsman deposits the immortal parts of human souls on the earth and other planets ‘to await their bodies, like so many smiles awaiting their Cheshire cats’. But tricks of style apart, this is a book written in a way which the modern student of philosophy will follow; and that cannot be said of every book on Plato produced in recent years.

University of Southampton.


Professor Düring has made an outstandingly valuable contribution to the progress of Aristotelian studies during the past decade. It was his vision and initiative which created the first Symposium Aristotelicum in 1957; and at that meeting, in Oxford, he worked out, with some colleagues, a plan for an edition of the five most important early works of Aristotle.

Düring’s Protrepticus has appeared long before the others, and the University of Göteborg and its official publishers are to be congratulated on a handsome, rapid and accurate piece of work.

The book consists of an introductory review of the problem; then A. Testimonia (i.e. passages from ancient authors which name the Protrepticus) without comment or translation; B. Fragmenta, with critical apparatus and English translation, followed by an index of words, list of sources, and various concordances; C. Related Texts, without translation or comment, followed by an Index; Appendix of passages (references only, in most cases) which others have ascribed to the Protrepticus; Commentary on the fragments; an essay on the philosophy of the Protrepticus as reconstructed by Düring; and a bibliography of over 100 items.

Düring arranges the ‘fragments’ in a new order, in an attempt to reconstruct the argument of Aristotle’s book, and he prints them without references (though of course the references are listed elsewhere). The editor had to choose, of course: he could either print the relevant passages as the work of Proclus, Stobaeus, Iamblichus, etc. (as in the ‘A. Leben und Lehre’ sections of Diels-Kranz Fr. der Vorsokratiker), or else decide that he had some real fragments—i.e. ipissima verba—to deal with, and print them in the most plausible order possible (as in the ‘B. Fragmenta’ sections of Diels-Kranz). Düring optimistically chose the latter.

It must have been a difficult decision. I expect the other style of arrangement would have brought its own disadvantages; but it is easier to see the disadvantages in the method chosen by Düring, and it is the critic’s duty to point them out.

The chief purpose of this series of editions must be (in Düring’s words) to ‘provide a new footing for further research’. The first need, therefore, was for texts of all the available evidence, presented in an untendentious form (so far as that is possible); for the texts so far available (Rose, Walzer and Ross) are in various ways inadequate. The peculiar problem of Aristotle’s Protrepticus is this: how much of it is preserved in Iamblichus’ Protrepticus, and to what extent and in what ways has the material been reworked by Iamblichus or his intermediate sources? Düring’s chosen method of arrangement has forced him to trust his judgment to the limit on this problem, and
thus perhaps to prejudice some issues for future researchers.

Perhaps the clearest instance where another arrangement would have been preferable is Iamblichus' chapter V, 34 5 to 36 26 (Düring's B 22–30). The attribution of this passage to the Protrepticus, Düring observes, will always remain doubtful; but it seems clear to him that it contains excerpts from an early work of Aristotle, drastically abridged. The text printed by Düring unfortunately obscures the issue by omitting 35 18–36 6 (between B 28 and 29). For Iamblichus' argument runs like this: when man is deprived of sense and mind he is like a plant, when deprived of mind alone he is like a beast, when deprived of reason (ἀλογία) but possessed of mind he is like a god (B 28): hence the emotions must as far as possible be purified of reason and practical morality must be directed towards mind and god (omitted by Düring). The Stoicism of the omitted passage is obvious (cf. also τὸν καθά μέρος καθήκοντο τὸ εὐθύγειον ἀναμετρητέον, 36 3). But that makes it likely that much of the whole section, including its 'Aristotelian' material, was taken by Iamblichus from a Stoic source; for the section is more consecutive than Düring allows. If so, this is an important bit of evidence on the question of how Aristotelian material got into Iamblichus.

Another case where Düring's arrangement seems to me unjustified is Iamblichus Protr. 6, 37 38–41 5 (Düring's 31–40, 53–7). This section is (as De Strycker and others have observed) tidily organised and articulated, and I think the reasons for attributing the articulation to Aristotle himself are as good as the reasons for attributing any part of Iamblichus' Protr. to Aristotle. I regret that it has been split up.

However, Düring acknowledges that he does not attach much importance to the arrangement of the fragments (p. 37). The main thing is to have all the texts available, along with the other material which helps towards an interpretation. Düring's own interpretations and assessments are always valuable and suggestive, but not, of course, final. The debate will continue; and I now list a few points which seem to me particularly debatable.

B 3 χριστις δὲ τῶν εἰρημένων ευμβιανὲς τοὺς μηδένους ἄξιους ἐδέχεται, ὅταν τοῦκεις ὄργανα, καὶ τῶν διὰ τὴν γνώσην ἀγαθῶν πλάθος ἄξια ἀτοικον εἶναι τὰ κτήματα, ἐν πάντων ἀλεξίτουν.

'Besides, when worthless men get abundant possessions, they come to value these even more than the goods of the soul; and this is the basest of all conditions.'

So Düring translates the first sentence, following Ross. To make this translation plausible, Düring alters the 'αὐτοῖς' of the papyrus (which is the only source for this sentence, though the rest is found in Stobaeus) to 'αὐτοῖς'. There can be no doubt that the proper translation of the text found in the papyrus is as follows: 'Worthless people, if they happen to acquire great wealth, are in such a position that their possessions are actually (καὶ) worth more than the qualities of their souls.' I take 'ἀγαθόν' to be morally almost neutral (like 'goods' in 'goods train'); alternatively it may be taken as 'such goods of the soul as they do possess'.

That this is the right translation is proved by the following sentence. For there is no comparison between a man who is inferior to his servants and a man who values his possessions more highly than his own character. There is a comparison between a man who is inferior to his servants and a man whose character is inferior to his possessions. Düring (again following Ross) translates 'οἱ πλάθον ἄξιαν τῶν κτήματα εἶναι εὐμβιανὲς τῆς ἡδονῆς φύσεως' as 'those for whom possessions are more important than their own nature'. It might mean that, if the dative 'οἷς' (like Düring's emendation 'αὐτοῖς' in the previous sentence) were construed with 'ἄξιον εἶναι'. But the dative is equally at home with 'εὐμβιανὲς'; and the accusative and infinitive clause means 'their possessions are (objectively, not in their eyes) worth more than their own nature'.

The idea is a development of a theory of the relation between soul and body which is well documented in Aristotle's ethical works. In a notorious passage of the Politics, A 3–5, Aristotle argues for the naturalness of slavery: a slave is a κτίμα ἐν χρήσεως, and there are certain people who naturally belong to others who are their natural superiors. Similarly the soul is naturally superior to the body and is its natural master. In Eudemian Ethics II, 1218b32 ff. (Düring C 2.2), Aristotle establishes the same dichotomy as here, between external goods and goods of the soul, and observes that the latter are preferable (ἀλητέα); and this distinction, he adds, is also made 'ἐν τοῖς ἐξωτερικοῖς λόγοις'.

These two passages contain all the elements of this argument in the Protrepticus.

B 9. Düring translates χριστις χαί καὶ ἀλητέας κατὰ φύσιν δυσάτας as 'can use all other kinds of knowledge and can prescribe according to (the principles of) nature'. This provides him with his strongest (or least weak) argument for maintaining that B 9 is part of the Protrepticus. For it looks reminiscent of the ὅταν ἀπὸ τῆς φύσεως αὐτῆς of B 47. But I think the phrase means 'is naturally able to make use of all the others and give commands to them' (cf. Iamblichus Protr. 38, 15 Pestelli ἀρχικότερον τῆς φύσεως ἐκτεινομένος). If so, the important Aristotelian parallel disappears.

B 55. We have to consider four related texts, assembled by Düring in his note:

A. Iamblichus Protr. 40, 15–20 (= B 55)
B. Iamblichus CMS 82, 17–22 (= C 54)
C. Proclus in Eucl. 28, 15–17 (C 52.2)
D. Iamblichus CMS 83, 13–16 (C 55.2)

All four of these say something about the rapidity of progress in philosophy or mathematics; it is a question whether they say (a) that individual students progress
rapidly, or (b) that philosophy or mathematics in general, having developed late, has made rapid progress.

Düring's view is that A, B and C assert proposition (a), which is taken from Aristotle's Protrepticus (though B and C are derived from A); whereas D asserts proposition (b), which comes perhaps from Aristotle's On Philosophy.

If we start from D and then compare the wording of the others, we get a different picture. D has this: τοιοῦτον δὲ νῦν προελθθέθεν ἐκ μικρῶν ἀφορμῶν ἐν ἐλεγχτικότι γράφοντος ἐπειδὰν οὐ τοις γενομέτων καὶ τοῖς λόγοις καὶ τὰς ἄλλας παιδείς...; and in the context it certainly means that academic studies, after a late start, made rapid progress compared with other sciences, in spite of the lack of reward for this kind of work. C has 'τὸ μηδὲν πιθανόν προετοιμασμένον τοῖς προφητείας ὑμῖν ἐν ὁλίγον γράφον τοιαύτην ἐπίδοσιν τῷ τῶν μαθημάτων θεωρεῖν λαβεῖν'. This must be the same point; no one, faced with these two texts alone, would assert that the second is about the progress of individual students. But if C asserts our proposition (b), then so do A and B, which both have ὁμώς εἰ ὁλίγον θέσισαν παρελθθέθεν (προελθθέθαν Düring) ταῖς ἁκρίβειας (σε. τοῖς φιλοσοφίαν).

This is a point of some importance. For C (Proclus) names Aristotle (as Iamblichus does not) as the authority for this assertion (I have already criticised Rabinowitz's denial of this in my review of his 'Aristotle's Protrepticus' in this Journal 89, 1959). We can claim the assertion for the Protrepticus since it is all of a piece with other Aristotelian protreptic arguments in Iamblichus; now we have a most valuable testimony that the assertion really does belong to Aristotle.

Moreover, Proclus speaks of mathematics having made great progress, and so does Iamblichus in text D. Düring takes the usual view, that 'mathematics' was substituted for 'philosophy' by the Neo-Platonists. This may be right. But there are two reasons for thinking that Aristotle may have said something about mathematics in the Protrepticus. First, he mentions mathematics as the prototype of a late-developing, non-utilitarian science in Met. A 1, 981b23. Secondly if the Protrepticus was an answer to Isocrates' Antidosis, as Düring now believes (p. 35), then the study of mathematics may well have been defended in it; for Isocrates particularly attacked the mathematicians (Antidosis 258 ff.).

B 74 note. Düring finds a reference to this passage in Eucadian Ethics 1244b29 διεισ τῷ λόγῳ γέγορατο. But this cannot be right, for the same λόγος is mentioned a little later (1245a27): ὁ μὲν τοῦν λόγον ἐκεῖνα φης διαστόρων. Aristotle could not refer to the Protrepticus as διαστόρων; Düring himself truly remarks (251), 'the Protrepticus is not an aporetic treatise'.

B 81. ὅταν οὖν λέγεται τι ταὐτόν ἐκάτερον διοίκον ὀνταν, ἢ δὲ ἄλλας λέγομεν ἢ τῷ ποιεῖν ἢ τῷ πράξειν... 'When, therefore, we use the same word in two different meanings, the one implying action, the other passivity...'

This should read '...one of the two being so-called by virtue of acting or being acted on' [as opposed to the other which is merely a potentiality]. The author has in mind a theory of sensation or knowledge according to which they are 'πράξεω τι', such as Aristotle describes in de Anima II—III.

B 102. The translation is inconsistent with the note, and the note is wrong, I think. The gods are not mentioned at all in the text.

In his summing-up of the philosophy of the Protrepticus I think Professor Düring has perhaps been too anxious to present it as distinctively Aristotelian, and this has led him to minimise some differences between it and the big ethical treatises. To put it briefly, some of the passages attributed to the Protrepticus seem to entail consequences which are incompatible with the big ethical treatises of Aristotle. What is needed is a penetrating and rigorous examination of this problem. Must we believe the Protrepticus contained such passages? Is it impossible that they were re-worded by Iamblichus or someone else? What precisely is the upshot of these arguments, and how do they compare with the Eudemian and Nicomachean Ethics? My feeling is that Professor Düring has not pressed these questions hard enough, and has left us with shadowy impressions of the philosophy of the Protrepticus.

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This substantial volume devotes some 200 pages to ancient logic (to which this review is confined) and a further 100 to medieval logic. A work on this scale will primarily interest specialists, but the first 67 pages will provide an interesting introduction without too many technicalities. Virtually all Greek used is translated, and the style is uniformly clear, with rare exceptions (e.g. this reader, anyway, could do with some further clarification of the argument at the top of page 133, and the account of reduction on p. 76 could be improved. Some examples would be helpful on p. 184). There is a selective bibliography and an index.

Logic, we are told, grew partly out of reflection on the systematisation of geometry, partly out of the use of reductio ad (logical or empirical) absurdum in Zeno and Plato, and partly out of an attempt to deal with logical puzzles arising in the law-courts, etc. It was early argued that mere verbal patterns could not be the bearers of either truth (Disioi Logoi) or validity (Euthydemus), and Plato, though perhaps averse to formal logic as such, treated the questions.
What is the bearer of truth and falsity? (Sentences but also judgments.) What, or between what, is necessary connexion? (Between Forms.) What, and of what, is definition? The 'Theaetetus sits' example is, however, attributed to a failure to distinguish between singular and general statements (20).

After clarifying the relation between Plato's μέγιστον γένη and Aristotle's categories (30) the authors attribute Aristotle's over-emphasis on subject-predicate propositions to his insistence on primary substance as the ultimate subject, and a confusion of singular and general propositions to a Platonistic insistence on secondary substance as substance. The trouble over the sea-battle is attributed to a failure to realise the timelessness of truth and the role of propositions as against sentences and statements.

Aristotle's introduction of variables is said to avoid linguistic ambiguity only by an awkward terminology which itself leads to philosophical obscurity (and which, one might suggest, hindered the growth of relational logic by embedding the copula in a formal apparatus transcending the content of any given proposition). Controversy is rare, but Lukasiewicz is criticized for exaggerating Aristotle's emphasis on implication and axiomatization and failing to see that Aristotle regarded conversion as independent of the syllogism (80–1; cf. 159). Aristotle's modal terminology is clearly disentangled on p. 84, and his modal syllogistic is criticized because either modal terms are internal to their propositions, in which case no special syllogistic is required, or they are external, in which case they are predicates of singular propositions whose subjects are clauses, and so do not require a syllogistic (91). Aristotle is in fact accused of failing to separate, at various times, both the apodeictic 'S is necessarily P' from the assertoric 'S is necessarily-P' and the necessity internal to an apodeictic conclusion from that of any syllogistic conclusion as such (90–1, 95; cf. 104–5 on Theophrastos).

The Megarians are credited with inventing paradoxes, re-examining modal notions, and discussing conditionals, and their and the Stoics' views on the two latter topics are discussed at some length. The τρόπος is attributed to a confusion of statements about the past with statements in the past tense. It is argued (195–8) that neither Philo's nor the 'necessary connexion' theory of conditionals is adequate by itself, though in any case we must not exaggerate the truth-functional outlook of the Megarians and Stoics (148, 175). The chapter continues with examinations of Stoic theories of meaning and truth, and of their inference-schemata, and ends with a short comparative assessment of them and Aristotle.

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Students of Greek mathematical terminology have hitherto had to rely on indices to individual authors. What has been lacking so far is a study of the whole of the Greek mathematical vocabulary and of its development from the time of the Presocratics down to that of the late commentators. Mugler has set himself the task to fill this need. In preparing this lexicon he has used the most important texts of Euclid, Aristarchus, Archimedes, Apollonius, Hero, Pappus, Proclus, Eutocius, and, for rarer expressions, a good number of others. Illustrative examples are given in chronological order, beginning with Euclid.

Where a usage is attested earlier (as e.g.in Empedocles, Archytas, the Eleatics, Democritus, Plato, Aristotle, Eudemos, Autolycus) the evidence, again arranged chronologically, is given in a separate paragraph.

The lexicon proper is preceded by a Répertoire géométrique and a very valuable and instructive Introduction in which the author offers a study of Greek geometrical language. Euclid and his successors to this day use a language that is perfect in its precision. But Euclid's work is not the beginning: his language is the result of a long history of mathematical thought and writing. Some pre-Euclidean authors already use a terminology practically identical with that of Euclid; others write very differently. The majority of geometrical terms were probably established by the time the Elements were written.

Mugler gives examples to illustrate two characteristic features of Greek geometrical diction: (a) economy in the means of expression; and (b) faithful adherence to tradition. The Elements, of course, have a particularly important place in the tradition. It is there that the various streams of earlier mathematical terminology converge; and mathematical writing thereafter could not but be determined, in its terminology as in so much else, by the usage of the Stòras. Because of the privileged position of the Elements in Greek geometrical literature it is clear that in studying the history of geometrical usage in vocabulary, syntax and formulae, we will have to go back beyond Euclid to examine the materials that made up his language.

Mugler isolates a number of essential features of Greek geometrical language: the oldest part of the vocabulary consists of nouns, adjectives and verbs borrowed from ordinary language. In most cases we do not know the authors who first used these words with their specifically geometrical meaning; but many of these usages are older than Euclid. This feature, the use of terms belonging to the ordinary stock of language, is characteristic also in the later history of geometry. This old fonds was progressively enriched by terms and usages especially created for geometrical purposes. An interesting fact: old expressions belonging to pre-Euclidean terminology sometimes re-appear towards the end of the history of Greek geometry, after a long period of
disuse. Sometimes the reason for that may be the platonising interests of writers like Proclus.

Some geometrical terms are borrowed from other sciences or from the language of particular philosophical systems; from astronomy or physics; from Anaxagoras, Plato, Aristotle. A characteristic feature of the evolution of Greek geometrical language is the tendency to designate geometrical entities in space, points, lines, surfaces, by abstract terms originally denoting the operations from which these geometrical elements result: thus τόπος denotes the result of an intersection (a point, for instance) as well as the intersection itself; similar examples: διαμέτρος, ἀσθένης. Mugler further notices a predilection for abstract nouns; the occasional coexistence of two terms (in spite of the quest for economy) due apparently to the limitations imposed by grammar which, in different contexts, made the use of one term or the other impossible. The syntax of the verb is, according to Mugler, influenced in geometrical contexts, by the idealist presuppositions of the writers (this is illustrated in particular by the use of the passive voice and the perfect tense). M. also offers a study of the use of prepositions, conjunctions, particles, and participles in geometrical diction.

The alphabetic entries in the lexicon are clearly arranged and well-illustrated (only occasionally one may wish for slightly more: thus γεωμετρία means not only 'area' but also 'figure'). The Greek word is always followed by translations into Latin, French, German and English. Whether all the modern language translations are really necessary may well be doubted. With the Latin the position is different. Here one would wish to know what sources M. has used. Were they Latin translations and commentaries of late antiquity? Medieval works? Renaissance writers? We are not told. In some cases the answer to this question would be quite interesting; to give only one example: for ἔντασις (inscribing a figure into another) expansio is given as the Latin equivalent. Where does this come from? Lewis and Short do not know this meaning; the Latin Thesaurus is equally ignorant. It would have been helpful had we been told what the source for this is.

It is a pity that the author has confined himself so rigorously to the geometrical vocabulary (or has tried to do so); this has resulted in the exclusion of all specifically arithmetical material. Arithmetic and geometry are so intimately linked in Greek mathematical thought that the separation of the two must seem to be not only arbitrary but harmful. Indeed, on occasion, arithmetic slips in: see, for example, the entry under τετράγωνος; but this comes in only because the Euclidean text makes that necessary.

Such criticisms must not obscure the fact that this work, even though not perfect, is a most valuable auxilium which has already proved its usefulness.

A. WÄSSERSTEIN.

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books. He selects instead the stories which have inspired poets down the ages from Shakespeare to Joyce.

The book closes with Cupid and Psyche and Hero and Leander, with an excursion on allegory. Some, not all, additional myths are briefly outlined in an appendix, and there is a full, non-specialist bibliography for further reading, as well as useful maps and genealogical tables. One of the book's most attractive features is the wealth of illustrative photographs of mythological scenes drawn both from ancient and modern art. The author's name is sufficient guarantee of the work's all-round merits, and they will be hard to please indeed who fail to be captivated by its sanity and charm. Few errors were noted, though in a work of this range an author can hardly be expected to be master of every field. The reference to Horatius Coccus in Callimachus is highly suspect, and was Aeneas really a son of Priam? Misprints were noted on pp. 106, 198 and 285.

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Professor Starr gives the period 1100-650 B.C. as that with which his book is concerned, but these dates do not adequately reflect its scope. The work is divided into three parts: the first of these covers the period from when man first inhabited Greece down to the end of the Mycenaean civilization in c. 1100; the second part is devoted to the Dark Ages and takes the reader to 750; the third part, entitled 'The Age of Revolution', spans the century 750-650, and occupies half the book.

S. is an historian, but he is not considering the history of early Greece as a plain narrative of events; he is principally concerned with a far wider conception, the basic elements on which Greek civilisation was founded, and the whole nature of this civilisation. He has no easy task, for he is forced to base his deductions mainly on the archaeological evidence alone, though the literature and other factors are of use in the later stages. Indeed, he has deliberately limited himself in one matter, since it is his opinion that the oral tradition is, except in its broadest outlines, worthless as evidence.

It will be evident to the reader that S. is eminently fitted to his task. He has clearly thought very deeply about the whole subject, he has studied that subject in Greece itself, he has attempted with much success to interpret the findings of the archaeologists, and his knowledge of the modern literature is exhaustive. Finally, he has the inestimable advantage of having studied, and written on, the course of history generally. All these attributes have combined to produce a book of considerable value.

S.'s work is beyond the scope of a normal review, and I intend to consider only that part which concerns the approach to the Dark Ages and the Dark Ages themselves. In any case, I doubt my ability to deal adequately with the second half of his book. S. states that 'the historian can approach the development of early Greek civilisation only through its pottery' (p. 101). Let us then see where this leads him to. Briefly, he first analyses the concluding phase of Mycenaean civilisation, and shows it to be one of artificiality, ridden by convention and monarchic bureaucracy, with all artistic inspiration stifled. Then comes invasion from the North, and the Mycenaean power is broken. These invaders are the Dorians, whose primary importance lay in the destruction they wrought, thereby liberating artistic and other impulses from the fetters of convention, but who were not responsible for the creation of the succeeding culture.

There follows an age of unrest, marked by severe depopulation. The focal point of unrest was in Mainland Greece, and S. suggests that the invaders wandered about until in course of time they amalgamated with the surviving Mycenaean population and established themselves in villages—it is important to realise that he considers the date of maximum wandering to be perhaps c. 1000 B.C. Life was of a primitive nature, the economy mainly based on agriculture. The basileis were insignificant.

As against this gloomy picture, S. believes the appearance in c. 1050 of the Protogeometric style of pottery to be of unique importance: 'Its implication, simply put, is that the pattern of civilisation which we call Greek emerged in basic outline during the 11th century B.C.' (p. 99). The style reveals 'proportion, balance, and sober solidity; one senses here a dynamic tension of opposing yet elegantly co-ordinated parts' (p. 90). In view of these statements it is only natural that S. should devote much attention to this style and to the succeeding Geometric style.

One of the major problems that S. discusses is that of the place of origin of the Protogeometric style, and he concludes that, although the most important manifestation of the style occurred in Athens, a similar type of pottery developed independently and almost simultaneously in other parts of Greece. He admits that 'the main source of development certainly lay on the Greek mainland, and more specifically in the south-eastern districts' (presumably he means Attica and the Argolid), but that even so 'the Greek mainland moved together essentially as a unit' (p. 98).

These seem to me to be the main findings. To what extent are they open to criticism? I would like to say, first, that I am in agreement with S.'s estimate of the significance of the Protogeometric style, and am relieved that the statement has been made by an historian.

S. displays, however, a curious reluctance to give the invading element any part of the credit for the new ideas introduced by the Protogeometric potters. He rightly assumes a fusion between the new arrivals and the Mycenaean survivors, and one would
naturally expect the newcomers to have made some contribution, but S. (unless I have misinterpreted him) does not allow for this. His conception of the Athenians as essentially non-Dorian may account for his reluctance; but of course not all the invaders were Dorian, and I believe that there is sufficient evidence to show an amalgamation of survivors and newcomers at an earlier date in Athens than almost anywhere in Greece. And even if the Athenians were autochthonous, it is S.’s hypothesis that the Proto-geometric style evolved in other parts of Greece independently of Athens. Did the newcomers contribute nothing of artistic significance?

The hypothesis of independent development is itself open to criticism. It will readily be admitted that the Proto-geometric style shows local variations according to the district in which it is found, but it seems to me that an entirely separate evolution is aesthetically unlikely. At present it still remains a possibility that the basic style emerged in Athens and was imitated in other districts, variations being accounted for by the type of pottery previously current. There may not have been such a general and spontaneous blossoming as S. advocates. He would have done better in this instance to have left the matter open.

Next, is S. necessarily correct in his picture of a primitive type of political organisation, largely nomadic, persisting until after 1000? It is no doubt true of some areas, but is it true of all? Also, are such conditions suitable for the emergence c. 1050 of the Proto-geometric style? The evidence would permit some areas to have achieved settled conditions even before 1100, and would suggest that some of the Proto-geometric settlements were rather larger than villages (e.g. Athens, Argos, Iolkos). S. is not perhaps going counter to the evidence, but I think he has gone beyond it—even as he has done in stating that the practice of cremation was introduced to Greece from Asia Minor.

The mention of Asia Minor may serve to introduce another topic on which S. expresses views of great interest, the trans-Aegean migrations. He accepts the existence of such movements at an early date—from the eleventh century onwards—but considers that the numbers involved must have been very small, since there was at this time ‘neither any serious surplus of population nor a strong enough organisation to launch out overseas’ (p. 111). This is important, and S. is surely correct in stressing the lack of surplus population, but the present evidence would not exclude the possibility of some kind of organisation based on relatively settled conditions (see above). S. goes further, however, and considers that the important thing that happened was the expansion of Greek civilisation; in other words, the migrants amalgamated with the existing inhabitants of the East Aegean, and their culture was already strong enough to be adopted by those inhabitants. This is a point well worth making, and there is evidence for it at Old Smyrna and probably in the Halicarnassus peninsula. On the other hand, we do not yet know what happened in Ionia—there is, so far as I know, no native ware in the reoccupation strata of Miletus.

In conclusion, it appears to me that the picture given of the Dark Ages is not entirely free from blemish, due to S. occasionally going beyond the evidence. But this detracts only very slightly from the value of an extremely thoughtful and stimulating book. S. has given us new ideas and a new approach, and all he says is worthy of attention.

V. R. d’A. DESBOROUGH

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Several notable attempts have been made in recent years, perhaps in reaction against the somewhat arid nature of an exclusively archaeological or historical approach, to show, as D. remarks in a less general context, that ‘unter der mythologische Einkleidung ist jene vorchristliche Kulturvorstellung erkennbar.’ The method is, of course, not new, and both D. and M. acknowledge their debt to Cornford and others.

The Olympic games arose, according to tradition, out of the funeral games of Pelops, but the latter differed radically from those of Patroklos in that they originated in ritual. Pelops was an Anatolian fertility power whose cult replaced that of the local wine deity Oinomaos at Olympia, while Hera, an Argive version of the Earth-mother, was a doublet of Hippodameia. The myth of their marriage preserved a γάμος γάμος equatable with that of Zeus and Hera. The story of Pelops’ dismemberment, and the eating of his shoulder by Demeter, preserves the memory of a sacrificial slaying and omophagia at the site of his so-called tomb. These were followed in turn by ritual games, including chariot racing, designed to delight and revive the dead deity. ‘Das Saatkorn muss sterben, damit er den Menschen neue Nahrung geben kann.’

After the collapse of the Mycenaean empire the games were reformed in honour of Zeus, and a moveable four-year cycle was intercalated into the fixed eight-year period of the Earth-mother, which was based on the phases of the moon. Married women were henceforth excluded, although the memory of ‘Mutterrecht’ survived in the girls’ race and the presence at the games of the priestess of Demeter Chamyne. The έκεχερια too represented a ‘Removatio imperii mycenici’, while the custom of the athletes performing naked had its origin in ‘Fruchtbarkheitsreligion’. The victors’ wild olive crown was
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the symbol of Herakles-Jasios ("eine Schöpfung der inneres Proletariats der minoisch-mykenische Gesellschaft"), Demeter's male counterpart.

Such is D.'s thesis in bold outline. The weakest points are perhaps the somewhat contrived equation between Hera and Hippodameia (via Poseidon), and Pelops and Zeus. On the other hand D. must receive credit for including all the relevant evidence, both archaeological and literary, and for presenting the views of other scholars fairly. The book is, in many ways, a brilliant tour de force tempered by remarkable learning. It is also a model of continental paperback production, and is furnished with full notes and indices. The ref. in n. 230 should be 1414 bis 1415.

M. like D. is a confirmed believer in 'Mutterrecht,' and revives in this short chapter the views advanced by Bachofen in the work of that name published a century ago. Only 'Mutterrecht,' according to M., can offer a satisfactory explanation of the early date for the Olympic Games preserved on the diskos of Asklepiades, the antiquity of the Heraion, the rigid cycle of the Heraea or the unique phenomenon of 'ekteheiria.' Even when 'Vaterrecht' supervened and excluded women it still retained Demeter's priestess, the girls' race and the crown of wild olive. But everything depends as M. admits 'wen Bachofen mit seinem Mutterrecht Recht hat.'

JOHN POLLARD.

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This important monograph is based on the works of Julius Jütner, editor of Philostratos' de Gymnastica, and constitutes not only an indispensable compendium of all the ancient literary and archaeological references to boxing in ancient times, but offers many valuable comparisons with the modern art.

The Ancients did not wear padded gloves, but either bound their hands with thongs similar in type to the modern bandage, or employed heavy armour of the cestus type. Some like Odysseus fought with bare fists. Boxing in one form or another was practised by the Babylonians, Minoans and Illyrians of the Hallstatt era, but only in classical Greece did it resemble the modern amateur sport. In Rome it was at worst a 'schauspiel,' which tended to deteriorate into a gladiatorial exhibition, at best a practice for war. It declined after the abolition of the Olympic Games, and was only revived by the British during the eighteenth century. There was, however, no equivalent of professional boxing in Ancient times.

Many details about ancient boxing may be gleaned from the extensive archaeological material. There was a profound change in attitude to Greek boxing between Homer and Apollonius or Theocritus, though fatalities occurred at every period. The brutal descriptions of Hellenistic boxing were probably drawn from life. Details of boxers' physique, equipment, instruction and training have been preserved, and it is interesting to find that such familiar exercises as shadow-boxing and bag-punching were practised in ancient times. There were, however, notable differences from modern boxing. There was no ring or rounds, though the contestants might take breathers. There was also no limit to the number of contests in which a boxer might engage, and tripping was lawful. The methods of attack, targets and knock-out blows are shown on vases, and contrary to general belief both footwork and defence were well developed. Despite the severity of the injuries inflicted a champion such as Theagenes of Thasos survived 1,300 bouts, and remained unbeaten for 22 years. The grimness of the theme is lightened by one gleam of humour. The champion boxer Pythagoras of Samos was not, M. reminds us, the philosopher of that name! For the rest it is perhaps sufficient to say that the scholarship and illustrations are worthy of Pauly-Wissowa. (Jöngling, 1333 should be jungling and should not be Ionía?)


Far too rarely do lifelong experts on a subject produce a synopsis of the field in which they have specialised. For this reason alone the new contribution of Matz to C.A.H. would be welcome. But there is far more than this to be grateful for in this instance. It would be difficult to imagine a more lucid and comprehensive survey of the Middle and Late Minoan I–II periods conveying both their essential features and the author's deep interest and enthusiasm for his subject.

For his presentation Matz has combined the system of Evans, with its division into stylistic phases, with that favoured by the Italian excavators of architectural phases. The first chapter covers the Early Palaces, MM I–II, and the second the Late Palaces, MM III, up to their final destruction assigned to c. 1400, LM IB/II. The combination is convincing and helpful towards an overall presentation.

To the author the first period is that in which the Aegean area became Crete's sphere of influence and its two great achievements were Palace architecture and naturalism in pictorial representation. The second period marked the zenith of this civilisation and the spread of its culture in the mainland and the Islands but it also saw the growth of Mycenaean trade rivalry and ultimately the establishment of a Mycenaean dynasty at Knossos. All the great centres are considered to have been destroyed at the same time.
with the suggestion as 'mere possibilities' that it might have been the result of internal revolt or dynastic dissension.

There is naturally room for disagreement, mainly on matters of historical interpretation, and it would perhaps have been preferable to indicate the existence of other views on important points as has been done in the case of the date of the Linear B tablets. Obviously contentious points are the emphasis on Minoan influence and thalassocracy in MM II, Mycenaean-Late Minoan trade rivalry in Egypt, the purpose of the LM settlements in the Aegean, the meaning of Kephal and the origin of the 'flying gallop'.

More important for the historical interpretation is the dating of the destruction of the various centres in the LM period. Matz, while recognising the difference in style between the vases and sealings of the destruction levels at Knossos and those elsewhere, shares Pendlebury's view that the destructions are contemporary and took place about 1400, so differing from the opinions of Evans and Furumark. He seems to consider that vases of LM IIIB style, which mark most of the catastrophes outside Knossos, were being made at the same time as the 'Palace Style'. This argument hinges on their place of manufacture. The reviewer, like many others, believes them to be Knossian. From the 'Warrior Graves' and more recent excavations at Knossos, we now know that LM IIIB is not only a stylistic phase but has chronological significance as well and that vases of true LM II style were not being made there late in LM II. But we are not solely dependent on vase styles; the same is true of pottery shapes, for example the kylix found only at Knossos (in a reoccupied house at Mallia). Nor need we limit consideration to pottery. Knossian seals and sealings, weapons and frescoes are all more advanced and finally the other sites have Linear A tablets while Knossos alone used Linear B. The cumulative effect of all this evidence surely indicates a difference in date, in fact the difference between LM IIIB and LM II (or preferably early LM IIIA). Indeed, so great is the stylistic difference that the reviewer is inclined to believe that several of the destroyed sites had been reoccupied before the palace at Knossos was sacked. Certainly their later pottery is a development from the Knossian LM II/IIIA style and not a continuation of LM IB.

There are bound to be differences of interpretation and they do nothing to detract from admiration for this new fascicle.

M. R. POPHAM.

British School, Athens.

This fascicle is divided into two sections; the archaeological background by Desborough and the literary tradition by Hammond. The first section clearly shows the greatly increased scope of our archaeological knowledge of the end of the Bronze Age since the first edition of C.A.H. It is a lucid, finely integrated survey of the evidence with a sober and interesting reconstruction of events firmly based on this evidence.

It begins with the disasters at the end of the LH IIIB, with a hesitant acceptance of a movement from the north as their cause, and then considers the partial recovery early in IIIC with the Aegean Islands playing an important part. This latter phase required recognition and even more might have been made of the continuity of trading in some areas reaching from Troy to Italy. Then follows the final disaster and the disintegration of Mycenaean civilisation when Desborough suggests the appearance of cist tombs and long dress pins as possibly indicative of the arrival of newcomers. In the last resort, however, we are little nearer to tracking the presence of Dorian archaeologically: 'it is principally in the disappearance of typically Mycenaean objects and customs that one recognises the presence of newcomers'. The Protogeometric period which follows is one which the author has made particularly his own and it is most valuable to have this recent summary of his views. New is the suggestion that Sub-Mycenaean in Athens is a local style contemporary with the final stages of LH IIIC in other areas where it passes straight into Protogeometric: this has much to commend it and helps to close an unconvincing gap outside Attica.

The final section on Crete has to contend with the scarcity of published material. Even so, there is perhaps enough to postulate some threat to the Island in LM IIIB, the desertion of the town site of Palaikastro, of Gournia and the reoccupied parts of the Palace at Knossos, all marked by the presence of IIIB pottery of Mycenaean manufacture or style (unpublished in the case of Knossos): the burning of Kephala Chondrou and House E at Mallia within the same period but possibly later might show that the threat materialised in some quarters.

This survey of the end of the Mycenaean period was greatly needed and we may be grateful that it has been well done.

The literary tradition for the migrations is a more complex subject for condensation; the sceptic is bound to ask for a fuller discussion of the value of sources and genealogies. Such scepticism is increased by the claims Hammond makes for archaeology. It has surely not, for instance, confirmed that 'the power of Mycenae finally overthrew that of Troy' nor that after the Trojan War there was a 'revolutionary period with stasis rampant in the Mycenaean states', to take but two of several such claims. At Troy the archaeologist can point to destruction levels and indications of a siege; he can ascribe destructions to natural or human agency. But there is nothing in the excavations to indicate that
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Mycenaean were responsible for any of the destructions. Only after accepting the basic truth of the Homeric story and its background can this be assumed and a particular destruction level assigned to the event. Similarly the widespread LH III B destructions are more convincingly explained, with Desborough, as the result of external attack rather than internal dissention.

Equally open to doubt is the attempt to harmonise the literary chronology with the archaeological evidence so far available for the Dorian settlement of Thera and Melos. The abandonment of Phylakopi is not enough and the earliest subsequent pottery is too late to fit the Thucydidean date.

This is then a more controversial and partisan section with a valuable synopsis of a complex field and a welcome restatement of the author's attractive theory of a Dorian invasion from Epirus.

A few minor corrections in the second part: p. 25 l.21 insert 'of' before 'Dorians', p. 30 l.3 Tlepolemus, p. 54, B 5, "1931–2" not "1934".

M. R. Popham.

British School, Athens.


It seems that only one of the many editions of this book was reviewed in this Journal; the reviewer was A. R. Burn. It was the seventh edition, and the first to be edited by Professor Günther Klaffenbach after Wilcken's death. Now, in the year of the 100th anniversary of Wilcken's birth, the ninth edition has been produced, beautifully printed and illustrated, and again sensibly and reverently edited by Klaffenbach. With a few changes and additions, the text has remained very much up to date. Compared with other one-volume Greek histories, it has two great advantages: that it gives an account of the Eastern world into which the Greeks entered; and that it provides a really full, though not too detailed story of the Greeks themselves. If the subtitle calls it 'in the framework of ancient history', it means East as well as West. In fact, this general outlook (universalthistorisch in the sense of Eduard Meyer rather than Toynbee) should be self-evident in every complete Greek history.

As usual the main work of the editor is in the notes which are very extensive (45 pages). Professor Klaffenbach is sceptical about the method of the decipherment of Linear B (after, as he admits, having first shared the general enthusiasm); he is equally sceptical about the Themistocles inscription, and he supports the dating-down of the Athenian coinage decree and the Panhellenic congress by H. B. Mattingly. Such statements, though we may not agree with them, should be noted, since the author, after all, is one of our leading epigraphists; they may be useful in any further discussion. It is only natural that scholars should disagree with some of Wilcken's or Klaffenbach's views, e.g. when it is still maintained that Pericles was Oberstrategos, or Alexander is described as the champion of Greek civilisation. However, taken as a whole, the combined work of author and editor provides sound, if sometimes a little pedestrian reading. Thirty-eight years after its first publication, the book is still a safe guide, and it deserves to be translated into English, as was already suggested by Mr Burn in 1953.

VICTOR EHRENBERG.

London.


This volume, planned originally as a sequel to his earlier collected essays, appears now as a memorial to Arnold Gomme. Of the twelve essays which it contains seven have been published previously (though that on Thucydides and Kleon was not very accessible, having appeared originally in Εἰκαστικά for 1954); five are here printed for the first time. These are 'Homer and Recent Criticism', a vigorous and polemical defense of the unitarian view against Professor Dodds and the Milman Parry school (especially Professor Page), delivered as the Presidential Address to the Classical Association of Scotland in 1957; three papers entitled 'Thucydides and Fourth-Century Political Thought', 'Concepts of Freedom' and 'International Politics and Civil War', which give some indication of having formed the Gray Lectures at Cambridge (though this is not stated); and finally one on 'Aristotle and the Tragic Character'. The reprinted papers all concern Thucydides or aspects of Athenian history.

These essays bear the clear stamp of their author. They are fresh and lively, and constantly come up with those convincing and perceptive observations which can only spring from prolonged original thinking; but they tend to meander from point to point, so that the thread is hard to follow—indeed there is often no real thread at all. The essay on 'Concepts of Freedom' is a good example. It professes to be discussing the important problem: had the Greeks any notion of 'individual freedom' (as Grote said, and Constant denied)? But having turned (naturally) to Socrates, Gomme is led to put the question: 'What would Plato have done with Socrates in the ideal state?' and from then on the thread is lost. The lecture turns into a discussion of the changes in Plato's attitude towards free enquiry and the unresolved contradiction in Plato's mind, and the ordinary citizen in the real polis is forgotten. The same is true in varying degrees of the other papers, and Gomme seems to be aware of it, for he admits that 'International Politics and Civil War' is 'a random discourse' (p. 171).

However, an author is to be judged by what he gives. For the reader who is content to get his
enjoyment as he goes along without demanding a cumulative argument, these essays will provide the stimulus that comes from contact with an original and indeed an exuberant mind, that felt passionately and did not shrink from battle. When he discussed Thucydides Gomme was always worth listening to, and Thucydides occupies many pages of this volume. As the last fruits of Gomme’s learning it is a welcome reminder of the ποιητικὰ (in its best sense) of his mind, and a sad indication of the loss that British scholarship and more especially the Hellenic Society sustained in his death.

The editing has been well done (apart from the consistent mis-spelling of Wilamowitz’s name); there is a general index, an index of passages discussed and a bibliography of Gomme’s publications from his first essay in 1911.

University of Liverpool.

F. W. WALBANK.


This book aims to present in 96 pages a narrative account of the history of Sparta from the Dorian Invasion to the Persian Wars and to keep controversy as far as possible to the 50 pages of notes. It would have been a miracle if such a method in such a subject had wholly succeeded, and despite the clarity and simplicity of Huxley’s manner it is no book for beginners. For others the book has two considerable merits, on the one hand the presentation of the evidence on many topics in a brief and useful form, on the other the charm of the ingenious—Huxley is never at a loss.

Inevitably the book suffers from its brevity. What, for instance, is the value of a page and a half on the subject of Spartan population? If, as Huxley believes, the decline in the number of Spartiates had begun by the sixth century, the subject is too important to receive such casual notice. Likewise the Great Rhetra is handled very economically, but the discussion is far too brief to have any effect on those who take a different view. Indeed it is not always clear what Huxley thinks about certain matters of central importance, e.g. the distribution of land and the system of tenure and, notably, the development of the Spartan army. Huxley rejects the theory that would make the Rhetra divide the state into five, but nowhere does he properly explain the fragment of Aristotle’s Spartan Constitution which speaks of five lochoi. On page 48 Huxley says that ‘in early Sparta the military unit formed from a phratry was called a lochos’, on which in note 310 he comments ‘The ancient hereditary lochoi formed from phratries were called πατριους λόχους. They were discussed by Aristotle (Fr. 541 Rose)—.’ I find this obscure, to put it mildly. Huxley has his views on the subject (cf. note 667 which says that ‘the losses in the 460s brought already the end of the army of tribes, obes and lochoi’), but they are not clear. It would have been very much better if there had been an appendix fully arguing out his position—better both for the reader and, one suspects, for Huxley himself.

In the first two chapters Huxley bases himself on the account of Sparta in the eighth century furnished by Pausanias in Book III (where Pausanias can be checked he shows himself to have drawn upon a strong local tradition of the Spartans themselves”—p. 19—presumably in reference principally to Asine). This involves him in supposing that there was still an Achaeae remnant in southern Laconia during the reign of Teleklos in the third quarter of the eighth century, that Achaeans still held the towns of Amyklai, Pharis and Geronthrai and that the capture of Helos belongs to the next reign. This is very hard to credit. By 730 the assault on Messenia had begun in full earnest and, if the pressure of population was such as to drive Sparta to that, surely the towns of southern Laconia would have felt the growth of Sparta long before. Ephorus at any rate appears to have thought so, and there is no good reason to prefer the account of Pausanias.

When he comes to the central question of the development of the constitution, Huxley is at his most challenging. In the orthodox manner he assigns the reform embodied in the Great Rhetra to the early seventh century shortly before the institution of the Karneia, in the arrangement of which (Ath. 4. 141 EF) he finds the reflection of the new organisation—not the division of the state into five new tribes, but the increase of the number of obes from five to nine and the incorporation of new citizens. (‘At the end of the first Messenian war there were three tribes and five obes, but the large number of lots distributed by Lykourgos and Polydoros suggests that the number of obes was increased, and new members of the tribes recruited . . . Citizens newly enfranchised by Polydoros were probably kept in their periöik obes and brought into the state with them.’ p. 47). In all this Lykourgos was closely associated with the populist Polýdoros. These are dizzy heights. Why were there five ephors? Huxley replies that the number merely reflects the number of obes when the Ephorate was instituted. ‘It is possible that the number of ephors was increased together with the obes from five to nine at the time of the Rhetra; a lexicographer states that there were once nine ephors, but the text may be corrupt; in any case if the number was increased to nine about 676 B.C. it was soon reduced to five again . . .’ (p. 51). He does not tell us how or why (nor indeed why there were the five Agathoe–goi or the five arbitrators for Salamis or what exactly were Aristotles five lochoi). One feels lost in inextricabilis error. Until he gives a proper account of how there came to be these five, Huxley will not have many disciples. Further, how good is the evidence that Polydoros was a populist king? The whole attack on the Ephorate, in which king Pausanias played perhaps a decisive part, may have elevated Polydoros, if he had opposed the operations of ephors, to the position of champion of the people. There is
no proof of it, but one is uneasy about Huxley’s
time this tradition. There is, of course, a great
deal more to this chapter than I have indicated, all
of it provoking and therefore worth attention, even
at its most obscure.

The fourth chapter on the Second Messenian War
and the Conquest of Pylos has an interesting analysis
of the varying traditions, and the last chapter on
Sparta in the sixth century is a reasonably full assem-
blage of the evidence (though the evolution of the
Peloponnesian League is barely attended to). But
here too there are many points on which one would
wish to hear Huxley more fully. For instance, he
appears to date the alliance of Sparta and Tegea in
about 560: this is completely counter to Herodotus
who says that very shortly before the fall of Sardis, i.e.
about 546, the Spartans became πολέμος καταπέδωμε
τῷ πολέμῳ, and one would like to know whether
Huxley rejects Herodotus or postulates a breach in the
alliance. Superficially, neither course is comfortable.
Again, a minor matter, it needs proof that it was
Kleomenes who proposed to restore Hippias: Huxley
may have good reasons for supposing that P.Oxy 1012
frag. 9 line 55 is more than what is appears to be, a
misreading of Herodotus, and one wishes he had dis-
cussed the matter. Or again, from Hdt.5.49.8
Huxley draws support for his view that the alleged
revolt of 490 was part of an eleven-year war of which
we learn from Rhianos. Since Herodotus mentions
Argos in this very passage, it is a pity that Huxley did
not explain why he does not extend it to war against
Argos: and if Sparta was fighting in Messenia, Huxley
should have discussed how it affected, or why it did
not affect, the campaign of Sepia. Meanwhile,
those who believe in the Helot Revolt of 490 will
continue to attribute it to the opportunity presented
by the invasion of 490, or (much less credibly) the
activities of Kleomenes in Arcadia.

Brevity is not the only virtue, and when a book
contains so much that is provoking and new it is
essential that the author makes himself fully under-
stood. Huxley has a fertile mind and much can be
expected of him, but this book and this subject have
not shown him at his best.

G. L. Cawkwell.

University College, Oxford.

KIECHLE (F.) Lakonien und Sparta. Untersuch-
ungen zur ethnischen Struktur und zur
politischen Entwicklung Lakoniens und
Spartas bis zum Ende der archaischen Zeit.
(Vestigia, 5.) Munich: C. H. Beck. 1953.
Pp. xii + 276. DM 36.

To an admirer of Dr Kiechle’s Messenische Studien,
this complementary volume, which attempts to trace
the history of neighbouring Lakonia down to the sixth
century B.C., comes as a slight disappointment though
it is by no means without its interest or importance.
K.’s strong point is his ability to take a synoptic view
of evidence from every kind of source and to ask
himself new and profitable questions about it; his weak-
nesses that his answers are not always fully worked
out and that occasionally the new questions drive out
older ones which are still worth considering.

The chief question he asks here is about the nature
and the influence of the non-Dorian element in the
population of Lakonia, not only or even principally
among the helots but at every level of Lakonian and
even Spartan society, and where he concentrates on
the evidence for these non-Dorians he does well,
sometimes brilliantly: the state of Lakonia in and
around the period of the Dorian Invasion; the later
absorption by Sparta of the surviving ‘Achaian’ cities and
in primitis Amyklai (K. belongs to that small
enlightened group who respect Pausianis’ Lakonika,
but he should take note of Jacoby’s caution (PGH 595,
Komm. p. 637) before asserting Soisibios as the source
for anything except P.’s chronology); the earlier and
later emigrations of Dorians and, he would argue,
non-Dorians to Kretes, Kypros, Melos, Thera etc.
All this is exciting and illuminating stuff, however
much we may disagree on details; or feel that K. is
sometimes not quite as precise as he might be (If pots are
the criterion, 750–00, not ‘after 900’, is the date for
Thera and in any case I can see no need for a second,
‘Aigeid’, stage of colonization); or also feel that some
of the answers have come out of the hat, not out of the
evidence (I simply do not understand how he arrives
at his firm conclusion that the Catalogue reflects a
sub-Mykenaisa Lakonia).

But where he introduces his Achaians into problems
where they do not obviously belong he is less successful
and it is here that he ignores old questions to his cost.
He argues, for example, that the Rhetra is concerned
with the admission of Amyklaians to the Spartan state
(hence the concern with tribes (old Dorian tribes) and
obes (Old Dorian obes)). A priori there is nothing
improbable in this, but stripped of essentials his case
would run (I think): (a) the ancient evidence, properly
understood, is, with few exceptions, unanimous
in dating the Rhetra, directly or by implication, to
the eighth century (true, and it is a pity that K. has not
set out the demonstration of this); (b) Amyklai was
captured in the eighth century (true, probably); (c) the
Rhetra could be, probably is, concerned with the
admission of new members to the community, who
might be or might include Amyklaians (true again);
(d) therefore it is concerned with the admission of
Amyklaians (not by any means proven; firstly and
obviously because (d) does not follow from (c));
secondly because the unanimity, such as it is, is for
King Charillos, not for King Telklos, in whose reign
Amyklai was taken, and, although the two kings may
well have overlapped around 750, the point should be
argued; so more importantly should the basic issue
in this whole debate—is it conceivable that the Spar-
tan demos received Rhetra-type authority as early as
750? There is a case to be made that it is conceivable
but the best case I can make has not yet persuaded
me—K. does not even try. ’Zeigt allein schon die für
ein griechischen Staat des 8. Jh.auffällige Angabe,
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dass bei ihm (sc. the demos) das κράτος liege, welche wichtige Rolle der Apella zugedacht war”—indeed it would, if it had been shown that it was possible.

K.'s treatment of the Rhetra is by far the most striking example of this fault and even here there is much shrewd argument and interesting observation, but this and passages like it rob the book of the appeal it would have had as a coherent whole if limited more strictly to the question of non-Dorian Lakonia; not so restricted it is still, in the best sense of the word, provocative; it is neither definitive nor even usefully resumptive.

W. G. FORREST.

Wadham College, Oxford.


This dissertation, which is written by a pupil of Professor Berve, surveys the relations of Sparta, Delphi and the Amphictyonic league starting with the expulsion of the Peisistratidae and ending with the close of the Peloponnesian war. It is a rather well worn field in more general works, and one need not be surprised that in this monograph Zeilhofer has no new material to add, but he traverses the ground thoroughly with a fresh and lively discussion of the many points of controversy which it contains. For instance he devotes a section to the question of Delphian 'Medism and defeatism', and after examining the oracles attributed to the period of Xerxes' invasion decides on acquitting Delphi of any such charge. Here his comments on the individual responses are very intelligent, but surely he is rather dogmatic in refusing to consider oracles before 480 as evidence for a tendency? Also one notices a failure to distinguish between the many possible shades of policy covered by the terms 'Medism and defeatism'.

An interesting feature which recurs throughout the work is the distinction which Zeilhofer draws between Delphi and the oracle on the one hand and the Amphictyony on the other. He rightly recognises that if one is to suppose that the published responses of the Pythia were at times expressive of a deliberate policy (as he presumes), this policy was dictated by the Delphic priesthood themselves and did not necessarily agree with the policy of the Amphictyony, but might run counter to it. Sparta had no permanent seat on the Amphictyony and often little direct influence there, but managed usually to maintain close relations with the Delphic priesthood.

The pattern of Sparta's foreign relations in this period is hard to reduce to sense without a complete analysis of her internal politics—a subject of intense difficulty and uncertainty. It is understandable that in a treatise on this scale Zeilhofer could not be expected to go into this aspect in thorough detail, but also it is questionable how far progress can be made in such questions as Sparta's relations with Delphi in the period without trying to decide what different forces in Sparta were acting on her foreign relations. Zeilhofer, of course, recognises the personal importance of Cleomenes, but regards Pleistocomos' recall as a mere instance of the oracle's general support of Spartan kingship with no special significance in the context. Generally, he is rather too inclined to draw a simple antithesis between the kings and the ephors, without recognising that hostile kings attacked each other through the ephorate.

As a whole the thesis is well worth consultation by anyone working in the field. H. W. PARKE.

Trinity College, Dublin.


With this fourth volume, and after over thirty years, Burn reaches the end of his massive task, a history of Greece to the end of Herodotus' narrative. It has been a grand design. The late Bronze Age narrative of Minos, Philistines and Greeks, now sadly out of date, the largely anthropological pause of The World of Hesiod, have been succeeded by the linked narrative and cultural history of The Lyric Age of Greece and now by the firm and clear narrative of the crucial years 546-478 of this book. Since Grote, no one in this country has worked on this scale, and the evidence which Burn has had to work with has been immeasurably increased. By any standards, it is a work of which he can be proud.

He does not possess all the freshness and immediacy of Myres, to whom in essence, I think, he owes most; he has not the learning and insight of Meyer or the patience of Busolt. But he has in him the capacity to respond to various approaches, and a cool sense which enables him to accept and reject. In this book, he has given us a clear narrative without concealing his working, without shrinking a difficulty, and without pretending that his view is the only one possible, and all this without immoderate length and without sacrificing vitality and pace. To agree with his approach at all points is impossible, but he is never perverse and seldom unfair. As a complete guide to this difficult period, he is without a rival.

I could wish the book longer. There are too few points of repose, too few extended accounts of the stock-taking he must have done himself. To take two cases, he accepts Olmstead's view that Darius killed the true Smerdis, without developing the consequences for the character of Darius and his attitude to truth, and accounts for Cleomenes' actions, separately, as they arise, without ever examining his career as a whole to see how many keys are required for that lock of contorted design. (He does not in fact sufficiently try to get inside Sparta before Demaratus' opposition develops; the Spartan attack on Samos, of major importance for Spartan attitudes, is almost lost between this book and The Lyric Age, with the references in both hardly more than casual.)
But the heart of the book is the narrative, which stays alive even when the sources are at their baldest. A striking example is the account of the siege of Cleomenes on the Acropolis (pp. 180–2); has that episode ever been given its full value before? Sieges bring out the best in him; the siege of Paphos (pp. 203–5) is a splendid bit of archaeological history.

At any rate, to one who holds battle reconstructions in abhorrence and suspicion, Burn seems to do very well with his military history. He expounds his topography clearly and his maps help him; the caption to the map on p. 508 says that the heights are given in inches, but this is fortunately not true. His key for dealing with Herodotus is eminently practical, that Herodotus’ informants were not in high command and knew what happened, but not why it happened. I do not find myself able to swallow all the reconstructions; Miltiades’ plans for Marathon seem to me suicidal for a citizen-army without much training, and I cannot accept that the Greek high command at and before Salamis was quite as united as Burn makes them. The Themistocles-decree is of course crucial here; Burn is sceptical, but at least if he thinks that Aeschines made it up, he does not think he made it up out of his own head. He rightly thinks that the gap between sceptics and believers is less than they at present think.

Complaints. The most serious are on the oriental side. He is too dependent on Olmstead. If he had referred to Kent: Old Persian Grammar, Texts, Lexicon, systematically, instead of only sometimes, he would not have said that the Aria ramnes gold plate referred to Cyrus, king of Anshan, and would have learnt that its style was two hundred years later than Aria ramnes; he would have learnt that the revolt in Egypt in Darius’ first year is attested at Behistun, not in Susa, and that Darius’ first (eastern) Scythian expedition was dated at Behistun to his second or third year. Herodotus’ long Babylonian revolt has got lost altogether, rightly, but without warning. He does not know Wiseman: Chronicles of Chaldaean Kings, which would have filled out the picture for the late seventh century, which would have reminded him of the long-known evidence for Nebuchadrezzar in Egypt, and would have revealed the surprising Babylonian incursions into Cilicia in the 550’s. If he had taken advice, he would not have been so confident that the Nabonidus Chronicle refers to the fall of Sardis. Readers of this journal will find other mistakes for themselves. With so much detail involved, it is not surprising that there is a good crop to catch for a second edition. But Burn’s grasp of the essential evidence is enviable.

Christ Church, Oxford.

D. M. LEWIS.


Enmity between Aesop and the Delphians is referred to by Aristophanes and by Herodotus, who implies that he met death at their hands. Plutarch adds some details; but the first full account is given in the Vita Aesopi, a popular biography written, probably by an Egyptian, between about 30 B.C. and A.D. 100. The two versions of it that exist (G and W) are printed in Perry’s Aesopica. The story they tell is that Aesop mocked the Delphians because they had not suitably rewarded his ἔπειδεψες σοφίας, and in revenge they planted a gold cup from the temple in his baggage, charged him with sacrilege, and hurled him from a rock. A pestilence having come upon them in consequence, an oracle bade them atone for the killing, which was eventually avenged by strangers: ἀκούσαντες οἱ ἀπὸ τῆς Ἑλλάδος καὶ οἱ ἀπὸ Βαρβαρίας καὶ <οἴ> Σάμιοι, ἤξεδίκησαν τὸν τῶν Αἰασίων χάνων (Vita G). ἀκούσαντες οἱ τῆς Ἑλλάδος ἔξωκρηκτοι καὶ οἱ λοιποὶ διδάσκαλοι... , παραγενομένοι ἐν Δελφοῖς καὶ συνήτησαν ποιημένου ἤξεδίκησαν τὸν τῶν Αἰασίων μύρον (Vita W).

Wiechers, pointing out a number of not very striking parallels between this punitive expedition and the Sacred War of the early sixth century, advances the thesis that, in the popular tradition represented by the author of the Vita, the two were imagined to be one and the same. To the objection that it was the Delphians whom the other Greeks desired to punish for killing Aesop, whereas the Sacred War was undertaken by the Amphiictyones in defence of Delphi and against the people of Crisa or Cirrha, W. replies that since Delphi was situated in territory originally belonging to Cirrha, which claimed control over the oracle, and since in later centuries Cirrha disappeared from history, whereas Delphi became world-famous, it was natural that ordinary people should make no distinction between them, and should assume that an invader who carried war into the country of Delphi must have been operating against the Delphians. It is an ingenious and interesting hypothesis, which might conceivably be true, and the case for it is well and lucidly argued; but the actual evidence for it is extremely thin.

In the second part of the book W. notes similarities between the alleged circumstances of Aesop’s death and the φαρμακος-sacrifice connected with the Attic Thargelia. Harpocrate puts forward, as an αἴτιον for the φαρμακος-rite, the story of a man called Φαρμακος who was stoned for stealing some cups belonging to Apollo: and W. suggests that in the account of Aesop’s execution for the same offence we should see an αἴτιον for a similar rite at Delphi, discontinued at an early date and allowed by the Delphians to fall into oblivion.

In a short concluding section the Aesop legend is compared with the story of Neo tole mus’s death at Delphi—by stoning, according to Euripides, and after an attempt at temple robbery, according to Strabo and Pausanias—and it is suggested that both are αἴτια for a Delphic φαρμακεία-rite.

S. A. HANDFORD.

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Kirrha. Etude de préhistoire phocidienne.

Kirrha (I should prefer Krisa, but tastes differ), the prehistoric Phokian city on the coast near modern Itea, offered the French excavators in the late 1930's one of the best stratified sites in Greece; in three of the five sectors explored well-defined and undisturbed levels followed each other to a depth of some six metres, from EH III to LH III, and the evidence, taken together with that of the alternating inland site of 'Krisa' (BCH lxxi (1937) 299 ff., lxii (1938) 110 ff.), gives an admirably full and revealing picture of the prehistoric development of this part of Phokis.

The results are valuable, extremely so for the 4 metres of Middle Helladic deposit, but in general comforting rather than surprising: the usual break somewhere between EH and MH, followed however by a return of EH forms in 'MH Ib' (the comparison with Lerna is interesting); a decline, beginning in MH II, accelerated in MH IIIa (this while 'Krisa' flourished); a striking revival in MH IIIb following some measure of destruction which 's'accorderait bien avec l'hypothèse d'une invasion'; on the other hand a gradual transition from MH to LH I and then again decline.

A rich harvest of tombs (including one EH intramural pithos burial) shows a clear development leading to a fine collection of MH IIIb cist graves which have some affinities with the shaft graves of Mycenae. The pottery too is rich; plentiful enough and good enough to justify the careful analysis by series and level (quantitative and qualitative) which the excavators offer; it more than justifies their modest claim to have discovered only 'de dosage et de nuances'.

Indeed M. et Mme. Van Effenterre, who have been chiefly responsible for the work, are altogether too modest. Kirrha is not a startling site but it is an authoritative one; it has been well excavated and carefully studied. The lapse of time since the excavation may explain why it is sometimes difficult to relate the generalisations of the earlier chapters to the later catalogues and some of the pots to their exact stratigraphical context; it certainly seems to have blurred some of the details (particularly in the description of the buildings) as it may have blurred a few of the photographs; but, on the whole, the results have been described with restraint, precision, a sense of proportion and an elegance of style which matches the production of the book. It is a fine and business-like performance.

W. G. FORREST.


This volume concludes a work of which the first volume appeared ten years earlier (this Journal, lxxiv, 1954, 214). It has become slightly altered in the interval (there is no sign of the promised commentary), but is still intended 'to survey the accounts of Alexander' in our five main literary sources. Volume I established 58 'categories', under which the material was to be organised (e.g. 'Detachments sent and received by Alexander', 'Dedications by Alexander'); it went on to give an index, arranged by places on A.'s route, of all categories treated under each place by each author. (The volume was filled up, for no clear reason, with a translation of the fragments in Jacoby that deal with A.) The new volume consists of two main parts: I. 'The Categories', an index to the five narratives, arranged under the 58 categories; II. 'The Extant Historians', indexing ten new categories (subsuming the 58) under the five authors and the places along the route. Most of the volume is occupied by I. This is vastly complicated by the author's view (developed long ago in The Ephemerides of A.'s Expedition) that the extant accounts are based, ultimately, on Callisthenes (himself based on the royal journal) until his death in 327, then on no common source for over a year, then on Eumenes' royal journal from late 326. This view, built up into a fictional 'biography' of the royal journal, has found few adherents and no longer needs refutation. But it leads the author to subdivide each 'category' into three very unequal time 'divisions' (334–327; 327–326; 326–323). It is not certain what profit he expects anyone to derive from this, or from the comments it leads to. (E.g. 'The First Division has the largest number of entries; there is a sharp drop in the Second Division, and a rise in the Third'—words to this footling effect appear in 43 of the 58 'summaries', to no one's surprise or instruction.) The pointless multiplication of categories adds to the obscurity: it would have been much more helpful (as giving some real idea of the interests of our own sources and perhaps of theirs), if the author had confined himself throughout to something more meaningful, like the ten categories (such as 'military matters', 'envoys and speeches', 'dates, distances') of the final section—even though coherence and logic is not their strong point either.

It is to be hoped that the vast amount of patient effort that has gone into this work will turn out to be of some use to someone. But it is not easy to see quite what use, except for documenting such well-known facts as that most of our information comes from Arrian, but that Plutarch and Curtius tell us more about drinking and feasting. On the other hand, how not to use the work is seen in the author's example (I xii f.) of its use. He quotes a typically baseless statement by Tarn, to the effect that Ptolemy as an ultimate source can be distinguished from Aristobulus by his emphasis on A.'s divine guidance, as in the case of 'the two snakes on the way to Ammon'. We are invited to track Category XLII (omens, etc.) until we come to the visit to Ammon,
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where we shall find Aristobulus’ two ravens as well as Ptolemy’s two snakes. But if (per impossibile) any student of A. has forgotten this, he needs only find the visit to Ammon in Arrian—which will take him less than a minute.

The fact that only the five main accounts—familiar to every student of the subject—are covered practically nullifies the utility of these indices: the information one might really want (odd statements in unexpected places) is withheld. Fortunately, much of this, as well as what Robinson gives us, has long been available in Berve’s Alexandrienich. And C. B. Welles, in his Loeb edition of Diodorus XVII, has just given us a most helpful collection of parallel versions of incidents treated by Diodorus. Equally competent editions of the other accounts would be far more useful than the elaborate machinery—rather reminiscent of a Heath Robinson cartoon—set up by the present work.

E. Badian.

University of Durham.


No comprehensive study of the chronology of the prefects of Egypt from Diocletian to Theodosius has appeared since that of Cantarelli published fifty years ago. Vandersleyen has therefore performed a useful service in producing this very thorough re-examination of the subject.

There are two main divisions to the book: 284–327 A.D., when the evidence comes chiefly from the papyri, and 328–395 A.D., when it is almost entirely literary. In the first of these periods Vandersleyen is able to draw upon a vast amount of new material and so to make substantial improvements on the work of his predecessors. V. examines in great detail all the many chronological problems that arise. Among his more important conclusions may be mentioned the dating of Diocletian’s accession to November 20, 284 and the revolt of Domitianus to 297–8; it would appear that papyri from Panopolis to be published by Skeat will go far towards proving both these suggestions correct (see Proceedings IXth Congress of Papyrology, 193–8). Occasionally I find his treatment of the problems unsatisfactory: thus in Chap. xii V. is properly sceptical in theory of the value of the Coptic and Greek hagiographical sources but seems somewhat reluctant to apply this scepticism in practice; the discussion in Chap. xiii of the terms ἐγκατάστασις and ἐγκαταστάσεις as applied to prefects and praesides is valuable but probably relies too much on the use of correct terminology by Egyptian peasants; the possibility that Aurelius Mercurius (no. 3 in V.’s list) was not prefect but catholics might have been considered; the proposed emendation of P. Oxy. 2106, 1 (p. 114 n. 3) is certainly not justified by the arguments advanced; despite V., I believe that the prefecture of Sossianus Hierocles could well be 310. But places where I disagree with V.’s conclusions are comparatively rare and in general his use of the material seems to me to be sound and his judgment reliable.

The second part of V.’s study naturally contains less that is new. There is a careful reappraisal of the letters of Athanasius which tends to confirm the accepted views, and an ingenious attempt to get sense out of the garbled chronological data of the Excerpta Barbara. I am not at all convinced, however, by V.’s treatment of this farrago Alexandrina. V. makes a similar, but rather more satisfactory, attempt to explain the chronological confusions of P. Golosiches.

The list of prefects on pages 11–22 will prove most useful, but as it is likely to be used chiefly for reference one word of warning should be given: the dates in the list are rarely accompanied by any indication that they are doubtful, but it should not therefore be assumed that they are beyond dispute: frequently they depend on the arguments V. puts forward in the appropriate part of the commentary and this should always be referred to before a date is accepted. If this is borne in mind V.’s careful and painstaking study is sure to prove valuable to all who have occasion to use it.

J. D. Thomas.

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Manni here presents tables of the eponymous magistrates of Athens, Delos, Achaea, Aeetolia, Delphi and Miletus, together with the Roman consuls and lists of various Hellenistic dynasties. This is preceded (pp. 9–70) by a discussion of the calendar systems of Athens, Egypt, Syria and Rome (with an appendix on the foundation of Rome) and of the chronology of various literary sources, the Parian marble and the Chronicon Romana (FGH 252). A comprehensive reference book for Hellenistic chronology would be an enormous asset, but it would be dangerous to assume that this is such a book: M. has devoted a long series of articles (still continuing—see Ath. xl. (1962) 315–24) to problems of Hellenistic chronology, and this work represents a summary of his conclusions. But M.’s views are often unorthodox, and while a full discussion is out of place, it may be as well to draw attention to some of them.

A comparison of M.’s list of Athenian archons with that given by Merritt in his Athenian Year, pp. 231–8, shows that between 265–4 and 170–69 the two agree on the dating of only three archons. This is largely because Manni bases his system on a belief that with rare exceptions intercalary years were arranged according to the strict provisions of the Metonic cycle: the American investigators are agreed, however,
that this is a quite impossible basis for the reconstruction of the list. The results of this are well illustrated in the latter part of the third century. M. argued in RAL, ser. viii, 4 (1949), 53-85, that Antigonus Gonatas died in 241-0, not 240-9, the date given by Porphyry and generally accepted. Since the archon Lysias held office in an intercalary year at the beginning of Demetrius’ reign, he too is put in 241-0. But this disrupts the secretary cycle, and so M. moves Thrasylphon back from his certain position in 221-0 (Syll.3 557) to 222-1, and puts the institution of Ptolemais in 233, as against the usual date of 224-3. This last involves ignoring the crucial evidence of the ‘Great Archon List’ (Dow, Hosp. ii, (1933) 418-46, Merritt, Hosp. xxiii, (1954) 244) which seems to show that Ptolemais was instituted during its course and that this must lie in the 220s. M. lets the list start in 217-16, and this enables him to dispose of a break in the tribal cycle in 201-0, usually connected with the events at the outbreak of the Second Macedonian War.

One of the most important oriental documents for the chronology of the Seleucid dynasty is BM 33603, published by Sachs and Wiseman in Iraq, xvi, (1954), 202-12. M. believes that the Babylonian dates in this document, ostensibly based on the Chaldaic era beginning in April 311 should be regarded as misunderstandings of dates based on the Macedonian era beginning in October 312. He has three reasons for this. (a) The date of the death of Seleucus I given in the tablet ‘non è accettabile’. But this depends only on M.’s own reconstruction in Ath. xxvii, (1949) 108 ff.; there is no definite evidence for the date. (b) The length given for the reign of Antiochus III is too short according to the dates given for the beginning and end of his reign. But to judge by Sachs and Wiseman’s translation there is a difference between ‘reigned’ and ‘sat on the throne’: the former indicates the first regnal year, the latter the actual date of accession (cf. the dates for Seleucus IV). This will involve dating the death of Seleucus III in 222, not 223, the orthodox date argued for by Beloch GG2 iv. 2, 193-5. But despite Beloch’s arguments it does seem possible to fit the events of Polybius 5, 41-5 into the summer of 222, and the later dating enables us to accept the evidence of Pol. iv. 48. 6. (c) The death of Antiochus III is given as 25-III-125 but in another document it seems to be 11-III-125. But the king-list may mean that in month III it was heard that Antiochus had died on 25-II (thus Aymard, REA, lvii (1955), 108-9).

Some smaller points: p. 15: last line of third column of table: 247 should be 246. p. 50: M. thinks that the six-years gap between the censors of 241 and 235 and the double list of consuls for 220 may indicate that a year has been interpolated between 241 and 235, and one omitted between 220 and 219. But for gaps longer than five years see 230-24, 154-47: Laevisus and Scaevela were probably vitato creati in 220. p. 52: Darius I should be

Darius II. p. 77 n. 3: Eumenes I should be Eumenes II. p. 90: M. rightly accepts Dorimachus as Aetolian strategos for 211-10, implying the date of 212-11 for the Romano-Aetolian alliance. p. 107 n. 2: Trebonius was cos. suff. with Fabius, not with the one-day consul Caninius Rebilus.

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J. BRISCOE.


This is a delightful book, learned but simple, cautious but exciting. The strange figure of Aristeas has caught Mr Bolton’s fancy; he pursues him, his Arimaspians and their griffins, through the wildernesses of Aschylean geography, Pythagorean tradition, central Asian folklore and archaeology, to catch him in the end, no ‘shaman’ but a normal if far from ordinary seventh-century Greek, inspired by Apollo to search out the home of Apollo’s own Hyperboreans and searching faithfully until, among the Issedonians, somewhere beyond the headwaters of the Ishim and towards the Altai, he was turned back by tales of the horrors that lay beyond (πόθος τῆς πόλους made other Greeks πενθόμενοι a century later for far less reason). So he came home to tell his story in the Arimaspea, a story full enough of marvels, of ‘swan-maidens’, the home of Boreas, cannibals and falling feathers, not to mention the Arimaspians and their griffins, but marvels which represented genuine Issedonian lore as Aristeas understood it.

The work was eagerly read, by the Ionian geographers and Herodotus, by Pindar and Aeschylus, but, most important of all, by Pythagoras, who found Hyperborean beans much to his taste and saw in Aristeas a kindred Apolline spirit. Aristeas, therefore, ‘re-appeared’ to propaganda-conscious Pythagoreans in Metapontion and, when Herakleides Pontikos came to write up a Pythagorean myth, Aristeas was firmly added to the circle and became a ‘shaman’. The Arimaspea itself had probably disappeared but Herakleides knew how to produce a fragment when he needed one and probably did.

The evidence, as Bolton admits, is often of the thinnest and much remains debatable: for example (i) his thorough analysis of the language of the few surviving lines of the poem leaves the question of their authenticity completely open and one may well feel that it is simpler to accept all of them (or at least the Tzetzes collection) as genuine rather than to suppose, with B., a Herakleidean forgery based on Ionian tradition which somehow retains some genuine early (poetic) features; (ii) on the exact path of the Herodotean ‘Handelsweg’ B. follows, as I should, the eastern, Alataic, rather than the north-eastern, Urartian tradition, and we can now quote on our side the possible appearance of a bald Argippaean goddess in the Pazirik treasure (see E. L. Phillips, himself a
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Uralian, in *Arctibus Asiae* xxiii (1960) 124 ff.) but it is not easy to swallow Isedonians south-west of the Argippaei and we must admit that our Thyssagaei have to cover a multitude of steps.

On the other hand some points are established (and they are important): (i) that the author of the *Arimaspea* did obtain his information in Central Asia or from a reliable visitor to Central Asia, not from his imagination or from his wandering soul—B.'s collection of Asiatic and Chinese parallels puts this beyond doubt; (ii) that the wandering soul is a Pythagorean invention and that Herakleides is likely to have played no little part in its substantiation—again the way in which B. follows all his clues back to Herakleides is as convincing as it is ingenious; (iii) that at an early stage of Greek geography the Phasis was identified with the Tanais and that, granting this identification, Io's wanderings in the *P.V.* became such as would scarcely raise the eyebrows of any seasoned member of the Hellenic Travellers Club—among much else B.'s insertion of Aeschylus fr. 195 between lines 720 and 721 of the *P.V.* is totally convincing.

That all this should be gained is the more surprising since B. is sometimes shy of being as precise and definite as he might, particularly at the limits of his subject. On the Herodotean interval of 240 years he abandons hope, yet this is surely a clear case of reckoning in forty-year generations as at ii 145-4 and, I think, ii 53 (notwithstanding ii 142.2); in other words Herodotus means about 180 years and is wholly consistent with, even roughly confirms the upper limit for Aristes given by the Kimmerian invasion and the foundation of Prokennesos. Again Epimenides is, for B., a good parallel to Aristes as a victim of Pythagorean distortion. He would surely be a better one if B. were to argue with more conviction that he was a solid figure of c. 600, if, for example his prophecy of a Spartan defeat was attached, as it surely should be, to the sixth-century, not to the 'ninth-century' Arkadian Wars. Finally if Aristes was a real Greek of c. 650 B.C. what conditions did he find in the Black Sea? No Greek colonies perhaps—Histria, as well as Olbia, is c. 600 (*Dacia* ii (1958) 69 ff. against Eusebius and B. p. 196) but it is worth insisting on Eumelos and Istrokles (*ILN* February 27, 1933, p. 329; *BSA* 53-4 (1958-9) 16) to show that Aristes need not have been alone even as early as, say, 670. And, even more interesting, what conditions did he find in Central Asia? Russian archaeology can now produce a rough cultural map of the early-nomadic steppe and of the later cultural conflict between the nomadic and the more northerly Ananyino belt—some details of both would give substance to Aristes' journey and to the later Herodotean trade-route.

Bolton might object that his purpose is primarily literary, but one of the chief attractions of this book is the width of its range and one of its chief merits the confident and thorough treatment of much of the non-literary evidence (e.g. the account of the griffin-motif in Asiatic art). More of this, very little more, might support or add colour to Mr Bolton's story; it certainly would not alter any one of his wholly satisfying conclusions.

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W. G. FORREST.


Pendlebury's *Archaeology of Crete* has been out of print for a long time, which makes the appearance of a new book on prehistoric Crete by such an experienced Cretan archaeologist doubly welcome. It is, however, disappointing to find that it is intended to be 'not so much a revision of but rather as a supplement to' that of Pendlebury. Fortunately it is more than this but perhaps some of its defects are traceable to this original intention.

The basically new chapters are those on geology, flora and fauna, ethnology, language and scripts, Minoan trade and social and economic life; in many ways these are the most interesting and stimulating sections.

The book may be regarded from two viewpoints, from that of the reader of the Pelican series approaching the subject for the first time and from that of the student of Cretan archaeology. The uninformed reader is likely to be confused mainly as the result of the arrangement of the book which sandwiches the new sections at various odd points in a basically chronological treatment of Cretan prehistory. The result, to take but one instance, is that having considered the geology of the Island and the Neolithic period—and the reader will need great patience to understand the pottery jargon of which this section is full—he is plunged into a discussion of Cretan scripts and the decipherment of Linear B which basically concerns the Late Minoan II period. Nor, if he has understandably skipped over the bare chronological table on p. 17, is he likely to thread his way through the intricacies of Evans' chronological subdivisions which he will meet frequently but which are not explained until 120 pages later. The ordinary reader is likely to be confused and he may well be somewhat disappointed. Minoan art is a subject of which he may have heard enthusiastic accounts but if he turns to the chapter on this topic he will find a discussion of the origin of the spiral, of tortion as a decorative principle and of the eidetic theory of Minoan art but gain little idea of the heights and the range of the achievements of Minoan artists. There would be some compensation if the illustrations were good but on the whole they are poor and their selection rather wayward; sherd for the Neolithic period, murky frescoes, two vases of the Late Minoan period one of which is a somewhat unlovely oddity; no LM IB vases, surely the acme of Cretan pottery, no palace style amphorae and no seal stones, apart from a few line drawings. Nevertheless,
he is offered for a very reasonable sum a comprehensive and scholarly account which is by no means always hard reading and keeps well in mind that it is a story of people as well as things.

The student is far better served. While he might have preferred a revised Pendlebury, he has the next best thing, in part an addition to the older book and in part a summary of it generally well brought up to date and with an excellent bibliography. Indeed, much of the present criticism of the book would be invalid if the new sections had appeared as a series of essays on Minoan civilisation. As it is, the student is offered them and more besides at a price far below what such a book would have cost.


Some unusual views: p. 194 Phaestos palace partly restored only after MM IIIB destruction and not rebuilt as a palace, p. 190 Gournia palace destroyed in MM IIIB and reoccupied as ‘workmen’s flats’, pp. 291 and 299 LM II Vases found in E. Crete, p. 300 eruption of Thera dated 1400 B.C.

Some suggestions: p. 253 no evidence of use of armoured corsets in Crete but see the Linear B tablets and add to references the Dendra LH IIIA bronze corset and the Patras greaves, p. 267 add to imported stone vases in LM II/IIIA the royal Tomb at Telmessos, p. 281 simpler to consider the steatite vases at Ayia Triada contemporary with the destruction, i.e. LM IB, strengthened by comparison with Vaphio cups, p. 290. Kephala tholos LM II rather than LM IA as indicated by joining sherds from the fill, p. 300 too much emphasis placed on Evans’ comparison of reoccupation pottery with sherds from Tell el Amarna, often used inaccurately and in general sense in his time, p. 304 and p. 320, little evidence of ‘Achaean’ settlement in Crete during fourteenth century except perhaps in W. Crete cf. p. 308, and the ‘refugee’ hill settlements are surely not earlier than the end of IIIB, i.e. thirteenth century. The historical picture seems rather, LM IB destructions, LM II Mycenaean occupation of Knossos, LM IIIA reoccupation of LM IB sites, LM IIIA destruction of Knossos, peaceful continuation of occupation well into LM IIIB, LM IIIB destructions and desertions, introduction of Mycenaean pottery forms and almost certainly settlers, early LM IIIC foundation of ‘refugee’ hill sites, later IIIC another wave of Mainland settlers. p. 307, horror vacui more characteristic of LM IIIC than IIIA and then only in one style.

M. R. Popham.

MATZ (F.) Crete and early Greece: the prelude to Greek art. (Art of the world, no.) London: Methuen. 1962. Pp. 259. 2 maps. 64 plates (incl. 56 in colour). 30 text figures. £2 8s.

It is always a pleasure to review a book by Friedrich Matz because one can always see the wood through the trees. He is never bogged down by his knowledge but can always stress the significant features that characterise one culture or phase from all others, what Matz terms the individuation, or what Aristotle called to ti 'hj eivd.

This virtue is particularly important in dealing with Aegean art because in the late Bronze Age it is often hard to draw a line between what is Minoan and what is Mycenaean.

In his introduction the author uses the old Pelasgian wall of the Acropolis of Athens to point his moral of the continuity of Greek culture, how elements derived from the bronze age or even the neolithic period crop up every now and then in the art of classical times. Long ago the Swedish scholar Sam Wide had suggested that one of the most permanent features in Greek art was the ever recurring geometric motives of the ‘Peasant Art’ that never quite died out.

In the introduction Matz also emphases the influences of geography: ‘indented coastline’, ‘good visibility’, ‘alternation of sea and land’. ‘In early times the sea acted as a bridge rather than a barrier.’ This means that whereas Hellas was closed towards the west and north it was open towards the south. Its prehistory and early history were characterised by the penetration of cultural influences from south-east to north-west while corresponding counter-presures were exercised in the reverse direction by ‘under-developed peoples’. The author then briefly reviews Aegean researches from Schliemann up to the present day.

Chapter I—‘Incunabula’ discusses the various neolithic styles of Greece in a clear and competent manner, but was evidently written before the publication of the preliminary report on the British excavations at Nea Nikomedes, which showed that the pre-Sesklo culture extended also to western Macedonia. In the later Sesklo ware there are already hints of the torsion style which, as Matz first emphasised in his Frühkretische Siegel, was so prominent a feature of the bronze age art of Crete. It seems, however, that his dating for the Neolithic proper (fourth and fifth millennia B.C.) must be pushed back according to the latest radio-carbon dates.

Chapter II—‘The Age of Development’, discusses the early bronze age, and the author states that
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whereas regional differences have long been established it is more important to stress the similarities. The identification of dialects is only the first stage; what we have to attempt now is to describe the structure of the forms employed, "inner form" as it might be called. Throughout the whole area places of settlement now unmistakably tend to assume an urban character." He quotes 'circuit walls which are nobly proportioned and provided with gateways and towers', 'paved streets, squares, fountains, and cisterns for water'. 'The Princess Hall in Troy II was a massive megaron... The total length was about 45 m., the width about 15 m. The House of Tiles at Lerna was unique in the Aegean.' It was an enigmatic complex with façades on all four sides and no central court resembling rather Anatolian than Aegean plans, and was also distinguished by a large number of clay seal impressions from over seventy different seals. The building was destroyed in the Early Helladic II period.

Tombs often reproduced the forms of houses, on Mochlos and at Palaikastro those of the 'but and ben' type like the neolithic house at Magasa, in the Mesara 'tholos' tombs reproduced the forms of cave dwellings. At Troy Aegean and Anatolian cultures intermingled, but the Aegean contacts were with the mainland and the Cyclades rather than with Crete.

Matz notes the resemblances between late neolithic patterns and those of the patterned Urfinis ware, but leaves open the question whether this is fortuitous or due to some continuity of culture (Wid's Peasant Art again) but points out that the neolithic substrata of population on both sides of the Aegean were certainly related, as is borne out by the evidence of place names. But 'the differences between these various groupings were now becoming more clearly marked... Thus it may be said that the really characteristic feature of this entire phase is the tendency towards a greater accentuation of individuality. Although we cannot of course yet speak of the achievements of individuals, it nevertheless seems certain that corresponding to the greater degree of differentiation between local (and thus probably also tribal) groups, we have a breakdown and "individuation" of the single universal character of this age in comparison with the preceding one.'

In the Early Minoan period Matz in the main follows the orthodoxy of Evans rather than the heterodoxy of Professor Levi in his chronology, and also in his views on the Great Mother Goddess.

The author dates the beginning of the Metal Age in 2600 B.C. or shortly before, obviously relying on Allbright's date for Hammurabi. I am myself inclined to favor the slightly later date of 1750 put forward by Sidney Smith, but it should be remarked that some scholars such as Mellaart are in favor now of the early date for Hammurabi, which would push back the beginning of the Aegean Bronze Age to about 3000 B.C.

Chapter III — 'The Age of Maturity' is concerned with the Minoan art of the Palace period. Of the Palace of Minos the author says: 'One cannot really speak of a building as such—so open is the palace in every sense of the word. In lieu of a continuous line of wall we have on the exterior a mass of projections and recesses, apparently arranged in a quite arbitrary manner. The differences in the thickness of the walls allow us to draw conclusions as to the height of the various storeys, which again seem to lack any kind of order or symmetry'. He proceeds to say: 'As to the disposition of the rooms in the "Piano Nobile" only guesses are possible. Some information can be gleaned from the walls of the foundations which have been preserved. These together with the monumental staircase suggest that there were a number of formal reception rooms.' There is, however, a little more evidence than this; we know that pillar crypts in basements are regularly surmounted by pillar rooms with round wooden pillars directly above the masonry piers of the crypt, and bases or fragments of round gypsum bases for wooden columns were found in these crypts. On p. 92 Matz defines the differences between a Minoan palace and its Eastern neighbour. The central court occurs also in Mesopotamia and the Nile Valley. 'But the relationship between the buildings and the court there is different. In the ancient civilisations it is conjunctive; that is to say the court is left free from encroaching buildings. Of vital importance is the cube-shaped building which is complete in itself and can thus express a sense of timeless monumentality. At Knossos, on the other hand, the system of courts is conjunctive: the rooms are arranged around a court that is primarily rectangular in plan and is marked out in advance. The openings in the outside wall correspond to those leading to the interior of the palace. This makes for a sense of dynamism that is alien to the conjunctive type of architecture. The other element in Minoan palace architecture, as we have seen, existed in embryo in the agglutinative structure of sub-neolithic Knossos and Early Minoan Vasiliki. We are now in a position to understand why we have no further examples in the Aegean of the cubistic and conjunctive "House of Tiles" at Lerna. This was a foreign body, a reproduction of an Oriental type of building.'

There follows a good résumé of the Cretan palaces (including a summary of the recent excavations at Phaestos) and of the more important Minoan villas and their frescoes; the only surprising omission is the fresco from Pseira, fragmentary indeed but important as being the only relief fresco outside of Knossos.

In the ancient Orient says Matz 'the picture is an existing reality. Its revolutionary transformation into a representation, which is fundamental to Western art, is one of the achievements of the Hellenes. The Minoan pictures we are dealing with are still a long way distant from this goal; they are still encumbered with far too many uncontrolled primitive features for them to be called representational pictures in our sense of the word. But from a structural point of view they are striving spontaneously towards this
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objective. . . . There is still a predilection for the custom of distributing the figures over a two-dimensional surface, and not binding them by a base line. The human figures in particular show the use of the decorative element and the relation to a central point . . . pictures representing action show greater interest in general and typical situations than in the dramatic aspect or "individuation". This is probably connected with the fact that the animal pictures are more exquisite and occur more frequently than those featuring human beings. Of matchless beauty are the pictures of lions, bulls, ibexes, birds and animals locked in combat.

The chapter concludes with a brief account of Minoan religion in the course of which the author suggests the phrase 'sacred façade' to describe the type of small pillar shrine that existed in the central court at Knossos and was depicted on the miniature frescoes.

Chapter IV—"Transformation and Renaissance", deals with the Mycenaean style of the Greek mainland and its offshoots. 'But the decisive factor in determining the different character of these three centuries is not the shift in area, but the fact that a different people has now taken over the lead. . . . The most remarkable fact is that the un-Minoan features of Mycenaean works, i.e. those that exhibit signs of the degeneration of Minoan style should be the ones that determine the structure of Greek style.' Yes! remarkable perhaps, but not so very strange; this is Wide's Peasant Art again; as the Mycenaean art gradually discards its Minoan borrowings so one would expect the old Geometric art of Middle Helladic Matt-painted ware to reassert itself and exert more influence on the proto-geometric style of the early iron age. Even in Shaft Grave IV at Mycenae where Minoan influences were very strong the gold vase illustrated on Matz' Plate 43 is a purely Helladic shape. I agree with the author, however, in regarding the Vapheio cups as certainly imports from Crete.

The translation from the original German is for the most part good and clear, and only occasionally marred by an awkward phrase such as 'catastrophe horizon' on p. 162 or an ambiguity such as the 'they' in the first line of p. 163.

The coloured plates are splendid and reproduce the original colours with great fidelity, but the words 'Shaft Grave F' in the description of Figure 37 should be amended to Shaft Grave V to agree with the classification used elsewhere in this book. Hagios Elias is employed throughout for the mountain near Mycenae normally known as Prophet Elias.

R. W. HUTCHINSON.


The publication of this book was unfortunately timed, for it coincided with, and thus takes no account of, R. W. Hutchison's Prehistoric Crete and Professor Matz' C.A.H. fascicule: on the other hand, there was just time to incorporate some of the findings of L. R. Palmer's Mycenaeans and Minoans, with which Professor Graham finds himself in partial agreement. The result of this is that Chapter I, a short general survey of the field, is a strange patchwork of incompatible views. Accepting tentatively Palmer's dating of the last palace at Knossos, G. concedes that 'the architectural stage presented by the ruins of the Palace of Minos is not exactly comparable to that presented by the other palaces, since these were destroyed some two centuries earlier' (p. 15)—a conclusion which is of radical importance to his own work, but is at no point reflected in the rest of the book. Other quite contradictory statements, as that Evans' 'new and aggressive dynasty' was composed of invaders from the mainland, are left unaltered; and since G. still holds that the palace was 'essentially Minoan (not Mycenaean) down to its last days' (pp. x, 14), it is surprising that he is susceptible to Palmer's arguments.

Chronology is indeed one of the blind spots in this book: the only table, given 'in round numbers, adequate for our purposes' (p. 6), starts Early Minoan at 3000 B.C.: while the MM III B destruction of Knossos is apparently dated to the early seventeenth century (p. 9). The book concentrates on the latest phase of the palaces, for G. believes that any attempt to trace the evolution of Minoan architecture would still be premature: yet far earlier elements, like the N.W. 'lustral basin' at Knossos (filled in after MM III A) are included without comment on the divergence of date.

Chapter II is a very short description of the three major palaces; Chapter III a useful summary of twenty-one selected minor palaces and villas on various sites. Thereafter the work proceeds by topics, illustrating each problem by reference to the sites described. This latter part of the book is much more impressive, and includes the meat of G.'s interesting series of articles in recent numbers of the AJA; but in this context, the topics discussed would seem too selective, and at times too abstruse, to appeal to the general reader visiting Crete, for whom the book is partly intended.

G. first of all restates his arguments for locating the 'bull games' in the central courts of the palaces. Here his key piece of evidence, the gem in Oxford (Kenn's no. 202), has to bear an excessive strain: in particular, the impression clearly shows that the bull's muzzle is hidden by the edge of the 'block', so that the latter must be a tank or other hollow structure, rather than a platform like that in the N.W. corner of the court at Phaistos (which indeed it in no way resembles).

G. then deals in general with the residential quarters of the palaces, and in particular with the 'lustral basins'. He argues strongly for the view that these last are merely sunken bathrooms, but has to accept a compromise in cases where the location or
the associated finds point unmistakably to a ritual, or at least a non-domestic, use. It remains true, however, that there is no positive evidence for domestic use; there is proof that a different type of room was being used for bathing; while of the three basins in the palace proper at Knosos, one, in the N.W. quarter, stands isolated in a large hall or court (G. hazards a guess that this is part of ‘a fine suite of guest chambers’), the second adjoins the ‘Throne Room’ (and is thus presumably some 500 years later in date (LM III) in G.’s view), while the third, in the S.E. quarter, is a bare ten yards from an acknowledged bath room (here (p. 113) ‘LM III’ is perhaps a slip for ‘MM III’). On any account, it seems that never more than two basins were in use at one time, and those very curiously sited for bath rooms. The explanation of the examples in the outlying buildings is another matter: but the Little Palace basin is very publicly placed, as G. admits (p. 107). It was perhaps only at Phaistos, Mallia and other provincial sites that cleanliness superseded godliness. Elsewhere, G. sees religious functions in places where not even Evans had detected them (for instance, the ‘seat of honour’ in the Royal Villa, pp. 53–4).

The chapters on public and other rooms, building materials and methods, windows and doors, stairs, decoration and furnishings are full of interesting suggestions: not everyone, though, will be happy to place the main banqueting room of Knosos in the remote Upper North Pillar Hall (p. 127). G. is bolder in his restoration of upper storeys than almost any of his predecessors. The work concludes with a stimulating chapter on ‘procedures and principles’. The identification of a Minoan unit of measure is very plausible, and G. speaks up stoutly for the originality of Minoan palace architecture against what he calls the ‘excessive claims’ of the excavators of Alalakh and Beycesultan.

Although Evans is hailed by G. as the ‘Master of Minoan Archaeology’, his work (not for the first time) here receives less than justice. It is hardly true to say (p. 118) that the stairway entrance to the upper storey from the West court was ‘conjured up out of a fragment of decorative stonework of a type sometimes used above doorways’. There was also the massive evidence of a row of gypsum slabs from a ballustrade, resting on a foundation of blocks that project from the palace façade (P. of M. II 590, 592), and this perhaps vitiates G.’s notion of a huge ‘North West Hall’ above. Again, Evans’ explanation of the locking devices was not as obtuse as G. suggests (p. 175): it was applied to the cellar door in the South House, where there was a locking pin on the outside of the door as well as the inside, so that the bolt could only be withdrawn after it had been released on both sides. Nor, finally, can it seriously be maintained (p. 230) that Evans’ hypothesis of insulae in the first palace at Knosos is today supported by ‘few if any students of Minoan architecture’.

A great deal of trouble has been taken over the numerous illustrations. The photographs are excellent, and the restored drawings, many of them new, should provoke much discussion. The plans are less good: the cross-hatching on G.’s own plans has not reproduced well, and some are on a quite inadequate scale (the discussion of the ‘recesses’ in the palace façades is illustrated by drawings so small that they are scarcely visible in them); the plan of Phaistos (f. 4) breaks off short; and that of House E at Mallia (f. 23) is perhaps so simplified as to be misleading.

The book should have considerable popular appeal, for it is pleasantly written; and students of Minoan architecture would benefit from a close reading of its later chapters.

University of Edinburgh.

A. M. Snodgrass.


This volume completes the MAMA series, of which Sir William Calder was the chief architect, and it is greatly to be regretted that he did not live to see it published. The book falls into two parts. The first contains the Lycaonian material collected by Calder in 1934, with a contribution from Mr Gough on the Pisido-Phrygian borderland (section 9, inscriptions 330–404), three sketch-maps and a topographical introduction by Mr Ballance; the second part (section 10), for which Professor Cormack is responsible, is devoted to Aphrodisias in Caria. There are 40 photographic plates, a number of line drawings of the monuments described in sections 1–9, and an index for each part.

The names on the title page of this volume guarantee the standard of the work it contains. The transcriptions seem to be models of accuracy, the restorations of sense and moderation. Individually, few of the inscriptions yield new material of value to the historian: no. 12 reveals quæstor and pontifex at Lystra, no. 3 the tribe 'Ἰάπος Ῥωμαῖος (cf. Trib(us) Romana on an unpublished inscription from Pisidian Antioch), no. 66 adds the name Mourisa to Anatolian topography, and no. 211, if the reading is correct, prolongs the Galatian governorship of Calvisius Ruso until A.D. 110–111. Collectively, they help to build up a picture of Anatolian social life in the Roman and early Christian periods. For example, there is a striking contrast between the brief, sometimes uncouth, inscriptions that come from the countryside of the central plateau, and the more articulate utterances of the citizens of Aphrodisias, whose hellenised institutions this volume shows in action.

Some lack of unity in the book is inevitable, and we cannot complain that sections 1–9 and section 10
employ different epigraphic conventions; but the two sets of indexes (which are good but not perfect) should have been compiled on the same principles, and the Turkish letter Ĩ treated consistently throughout.

This volume is as handsome as its predecessors; but in spite of a higher price it was impossible to provide complete photographic documentation. The plates are clearly, if dully, printed, but the photographs are not always sensibly arranged (part of no. 514 appears on Plate 24, part on Plate 34), and some of the reproductions (e.g. nos. 553, 568) are too small for the inscription to be read. Students will also feel that in this, as in other volumes of MAMA, there are too few explanatory notes. This is the more to be regretted in a publication which, for most of the inscriptions, will be definitive.

It has always been MAMA policy to include all the inscriptions found during an expedition, unpublished or otherwise. In sections 1-9 nearly one-third of the monuments had already been published, in section 10 nearly two-thirds—in all 288 out of 627. Republishation has not always added much; in a few cases (nos. 421, 440, 513, 577) the editor has had to depend on old copies to restore the fragment he found. If some of these inscriptions had been omitted or merely listed, there could have been more and larger photographs, or at least more explanatory notes. There would have been no sacrifice of completeness; even now, only section 10, with its list of monuments previously published but not found in 1934, can claim to be an exhaustive catalogue.

As far as the areas it deals with are concerned, MAMA has performed two functions with distinction; but much remains to be done. The first task, that of recording and publishing, while they are preserved, the surface monuments of Anatolia, remains as urgent as ever, especially in remoter regions; and the second, the compilation of a general inventory of Anatolian inscriptions, is one that grows in magnitude and complexity every year—and every year becomes more pressing.


BARBARA LEVICK.

St Hilda's College, Oxford.


Though the nineteenth-century Austrian excavators of Samothrace uncovered a corner of this small, stoa-like hall, they did not recognise it for what it was, so that it was left to the painstaking and meticulous work of the late Karl Lehmann fully to reveal it. The elucidation of the original structural form from the evidence of the surviving rubble, as presented in this volume of the Samothrace series, is brilliant and convincing. Even allowing for uncertainties which Lehmann openly admits, this study represents a considerable contribution to our knowledge of Greek building techniques in the archaic period, and the ways by which they evolved into those of classical times.

The techniques used in this building are curious. Since it seems from the evidence of excavation that it belongs to the mid sixth century b.c., they were undoubtedly old-fashioned at the time of construction. This can be explained best by the time-lag for new techniques to reach relatively remote places like Samothrace. This enhances rather than detracts from the interest of the hall. The Samothracian architect was clearly afraid of using large stone. His walls are built up from an outer and inner face of small coursed blocks, with vertical joints that run obliquely to the face, being wedged into each other in what Lehmann appropriately terms a 'horizontal polygonal' technique. These courses are not regular, but are approximately graded, the larger being at the bottom exterior, the smaller at the top. In addition the courses are tied into place by a frame of thin beams running horizontally and vertically, recalling the technique used in the Mycenean palaces, though at Samothrace the tie-beams are applied to the surfaces, rather than being integrally built into the wall. The Doric columns, which have the extreme bottle-shaped entasis typical of archaic Greek architecture, are made up of small segmental blocks rather than circular drums. For this technique Lehmann draws a comparison with columns constructed from segments of baked brick in a curious, palace-like structure at Nippur in Mesopotamia. This comparison is of dubious validity. Though Lehmann quotes with approval Gjerstad's endorsement of the original dating of this structure as 'Mycenean', there can be no real doubt that Marquand's devastating criticism is correct, and that the palace is actually Hellenistic, the peculiarieties of design and technique being attributable to the translation of Greek stone architectural form into the local medium, baked brick; this is exactly the combination one would expect in this area in Hellenistic times.

Another curiosity of technique in the hall is the combination of stone and wood in the entablature, which in addition seems to have had an Ionic rather than a Doric form, reflecting a time when the
distinctions between the orders were not hard and fast, so betraying their presumably common origin. The question remains, what does the technique of the Hall of Votive Gifts represent? Lehmann clearly is thinking in terms of an eventual Mycenaean ancestry. This may be so. But the wooden frames of Mycenaean walls are more substantial than the Samothracian strips, and I cannot accept the Nippur columns as either Mycenaean or proto-Doric. That the technique derives from that of the 7th century, and tells us much about the beginnings of classical architecture, is reasonable, but on the ultimate origins we can still do little more than speculate.

R. A. Tomlinson.

University of Birmingham.


Of the city of Side little is known from ancient literature; and there may even be classical scholars who are not quite sure where it is. This little book puts an end to such ignorance. Thanks to the massive investigations that Istanbul University has carried out in Pamphylia under the leadership of Professor Manssel, Side is now one of the best known cities of the ancient world, and we may hope that its neighbour Perga will soon be hardly less famous.

The early phases of occupation at Side remain obscure and no support has been found for the claim that it was a colony of Aeolic Cyme. But though stigmatised in an epicene obiter dictum of Stratonicus as the most miserable people on earth, the Sidetes must have been quite well established by the time of Alexander the Great. Subsequently they co-operated with the Cilician pirates and ran an important slave market. Side was one of the leading cities of Roman Asia Minor, and it enjoyed a return of prosperity in early Byzantine times. With the Arab invasions the place seems to have languished, but a hoard of over 800 mediaeval European coins found on the spot provides evidence of its existence at the time of the Second Crusade. With the Turkish conquest the site was finally deserted.

The city as it now appears is mainly of the Roman era; and it is a shining example of what the Greeks eventually achieved by ‘staking everything on the polis-community’. Once it was safely launched into what the political historians call full decline, the Greek city developed an astonishing vigour and dignity. At Side the lay-out on the peninsula is irregular but none the less interesting on that account. The main avenues were thirty feet and more wide, with cambered roadways and Corinthian colonnades on either flank; but they were not continuous axial boulevards; and by way of contrast there are many cross streets only 5-8 feet wide, some with shallow steps leading up to them. The great avenues linked up the complexes of public buildings and monuments, and divided the city into quarters whose names are now known to us from inscriptions.

The Hellenistic walls are in part well preserved, with casemates and firing apertures at different levels. The Great Gate had an ingeniously contrived deathtrap court which was converted into a hall of fame in imperial times. Of the installations of the harbour, which (as we know) cost a great deal of trouble to keep dredged, there is not much to be seen now. But the aqueduct has been traced twenty miles from the tunnels by its source to the castellum divisorium and the clay pipes in the city, and the three-storied nymphaeum was unusually grandiose. This water supply system was repaired in late imperial times. It would be interesting to know how long it continued thereafter, for in these cities of later antiquity water was perhaps the biggest single factor in the maintenance of public morale. Of the other pillar of civilisation, education, less evidence has been exhumed as yet.

The hundred-columned square Agora was perhaps rather unhygienic in appearance, being architecturally of a piece with the colonnaded avenues whose effect naturally depended on a long vista; the most interesting building in this complex is the public lavatory. Various temples have been uncovered and skillfully identified with the help of coins, inscriptions and literary sources. Two fine ones on the cape belonged to Athena and Apollo, others in the town to Phrygian Men, Dionysus and the cosmic Tyche (the last-named being apparently a pyramidal-roofed building in the Agora square); and the Egyptian deities, the imperial cult and Jewish synagogue also had their place to judge by inscriptions. The huge theatre was largely above-ground, with vomitoria opening off the diazoma; seven Christians were martyred here for destroying public property. Houses, mausolea and a wide range of Byzantine buildings have been uncovered. Some of these buildings are extremely interesting in themselves, and they are all of first-rate importance for the study of Roman and Byzantine architecture. But space does not permit any description of them in this review.

This little book is not the final report. But it is a landmark because it will have far more readers than the definitive publication. Professor Mansel’s text is clear, authoritative and to the point; and it is excellently illustrated. Side has unfortunately told us less than we should have wished about the hellenisation of Asia Minor. But it tells us a great deal about the later Greek city; and Professor Mansel’s work in Pamphylia will rank as one of the most far-reaching contributions made in our generation to the study of ancient history.

J. M. Cook.

University of Bristol.

In a sea of sugar-cane . . . a great sandy eminence like a battleship at anchor." With this telling phrase, Sir Mortimer Wheeler opens his description of his excavation-site, the Bālā Ḥišār (High Fort) at Chārsāda in the Peshawar Valley (Western Pakistan). The site represents the citadel of ancient Pushkalavati, once besieged by Hephaestion, the general of Alexander the Great. The simile is indeed an apposite one. The fortresses of Western Asia—Gurgan, Herat, Bust, Kandahar, Gardez, Balkh, and Chārsāda itself, to name a few—dominate their surrounding plains like capital ships. Their massive armour of sun-dried brick protects them from the archaeologist's pick no less than it did in the past against the assaults of armies. Such sites are likely to represent the garrison centres of the Achaemenian Empire, and the staging posts of Alexander the Great. Yet their remains have so far offered little evidence of their historical role, their very construction presenting a major technical problem to excavators lacking the labour resources of the original builders.

Such mighty mounds present many intriguing problems. Is there preserved within their vast mass the remains of elaborate structures—superimposed decks or galleries for the garrisons, basements, dungeons and attics? Or have we to think of each generation living all on a single level, at the summit of an inert mound representing the flattened habitations of their predecessors? If there is justification for the reconstructions of fortified sites in Chorasmia recently published by Tolstov, the first picture must reflect some truth. Sir Mortimer poses the problem on p. 10, but his answer, on p. 18, shows that he inclines to the second alternative: 'The successive layers are the gradual accumulation resulting from normal occupation . . . and do not represent the deliberate and unitary construction of a high podium on the lines of the Harappa or Mohenjo-daro citadels.' At the same time, he admits the reservation that 'Within the restricted width of the cutting . . . no complete or significant building plan was recoverable'.

The tackling of so formidable an assignment by one of our leading archaeological virtuosi was bound to give rise to a report of the greatest interest. The time available for this excavation was only seven weeks, and the objectives set therefore limited and exploratory. Sir Mortimer's principal result is the publication and illustration of the stratified pottery sequence from the Bālā Ḥišār. Some of the pottery shapes, as the author points out, are new in the Indian context, but three in particular will be of interest to the Greek archaeologist. His 'Carinated Bowls' correspond closely in form with the well-known Greco-Achaemenian phiale shapes of the fifth century B.C. (it is a pity that the examples are too fragmentary to make it certain whether at Chārsāda these were, or were not, mesomphalos). The numerous 'Tulip Bowls' reproduce the profile of a specialised phiale shape known in the Mediterranean world during the fourth century B.C., e.g. at Deve Huyuk, and from Egyptian hoards. Everywhere this shape seems diagnostic of the eve of Alexander's conquests. Finally, the 'Lotus Bowls', of a strictly hemispherical shape unfamiliar on Indian sites, correspond in the Mediterranean world with the celebrated 'Megarian Bowls'—the most characteristic pottery shape under the Hellenistic kingdoms of the late third and second centuries B.C.

If Sir Mortimer's own dating arguments for these pottery 'key-types' required any further confirmation, such analogies would supply it. The main periods of historical activity at the Bālā Ḥišār are shown to be the late Achaemenian phase, culminating in the capture of the fortress by the armies of Alexander; and its occupation in the second century B.C. under the Indo-Greek kingdom of Menander and his dynasty. Besides thus defining the principal occupation periods and their wares, Sir Mortimer's studies at nearby mounds led to another important advance; this was the taking by the Pakistan Air Force of a magnificent air photograph of the neighbouring mound at Shaikhān. This photograph is reproduced on Plates XV and XVI. It reveals a Hellenistic city built on a rectangular grid plan, and closely comparable with the site of Sirkap, near Taxila.

The small finds are instructive if not spectacular: a terra-cotta mask of Alexander, from a jug-handle; a seal impression of Athena; and stamped pottery probably attributable to the Hunas in the fourth and fifth centuries A.D. There is evidence in support of the late dating (c. 200 B.C.) of the so-called 'Baroque Ladies'—the primitive female figurines in terra-cotta ascribed by some authorities to a remote antiquity. It is a pity that the only coin recorded, an Indo-Bactrian tetradrachm of Menander, could not have been illustrated, since it must be almost the only Indo-Greek coin of attested provenance. However, it is fully described.

The book has been beautifully produced by the Oxford University Press, with excellent photographs and drawings. The only misprint that is obvious is 'Albārūnī' for 'al-Bīrūnī', on pp. 6, 7, 14, and Index. Sir Mortimer is to be heartily congratulated for enabling us for the first time to discuss the site of Pushkalavati in meaningful terms, and on indicating the nature of its problems. His work is likely to bear further fruit in the results of the more extensive area-excavations which are currently in preparation.

A. D. H. Bivar.


The material published in this volume falls into two roughly equal parts. We begin with a selection of some 250 whole or nearly whole pots, illustrating the progress of the standard Attic shapes from Late Geometric to the end of Protoattic. This is followed by a copious array of painted fragments, valuable for the new light thrown on the first five generations of Attic figure painters. Special techniques (light on dark; stamped and incised coarse ware), test pieces and imports are left until the end of the catalogue. Finally, there are notes on the context of the pottery, and a description of the Agora area before it became the centre of public life at the end of the seventh century.

The catalogue is preceded by a humane and sympathetic monograph where the individual painters have for the author, no less than for her predecessors, ‘the familiarity of friends’. To this introductory essay, as well as to the catalogue, the photographic plates are excellently geared; each one is carefully composed so as to tell its own story. The quality of the photographs is well up to the superb standard that we have come to expect from Alison Frantz. Some profiles and some designs are also drawn; there are occasions when further drawings would have been welcome (e.g. profiles of the chronologically important kotylai, pl. 9; design of the fr. 439), but these can often be found in the original *Hesperia* publication.

Faced with a mountain of material, Dr Brann has selected wisely. Most of her pieces had already received detailed treatment in *Hesperia*, but we still badly needed a general prospectus of the Agora pottery; this was particularly true in the case of the plainer domestic pots, which only tell their full story when arranged in a series. For their dating, the wells of the Agora have yielded evidence on a scale that no future Attic excavation is ever likely to match; from this great abundance the author has chosen just enough to clarify the development of each shape; where possible, she has included something for every quarter century. The only significant evidence omitted here is statistical; readers wishing to know the relative frequency of each shape must turn to the author’s detailed publications in *Hesperia* xxx.

In spite of its fragmentary state, the figured pottery forms a useful supplement to the more remarkable grave offerings from the Kerameikos and the Attic countryside. The contribution of the Agora pieces is twofold: not only do they help to fill out the careers of the painters that we already know, but they also have much to tell us about the moments of uncertainty that followed the retirement of the dominant hands, who created and passed on a classical tradition in Attic vase-painting. The first of these greater personalities is the Dipylon Painter; the hands of the Subdipylon Workshop now emerge more clearly than ever as his immediate heirs, and the predecessors of the Philadelphia Painter. Olpai and kotylai from the Agora (pl. 21) show us minor work from the latest Geometric workshops, those of Athens 894 and 897.

In the next generation new pieces are ascribed to the Analatos painter at every stage of that prolific artist’s career. Further fragments (418–424) help the author to add the Vlasto sphinx fr. (*BSA* 35, p. 51c) to the works of his lesser contemporary, the Mesogeia Painter; this is a vitally important attribution, bringing the invention of the Black-and-White technique well within the limits of Early Protoattic. In the ensuing period of ferment, Dr Brann shows how some of the Mesogeia ornament survives into the repertory of the Polyphemus painter; not enough, perhaps, to prove a definite master–pupil relationship; but nevertheless a significant link. The mantle of the Analatos Painter, as many scholars have acknowledged, descends eventually upon the shoulders of the Ram Jug Painter: pieces like 539 strengthen this link. Somewhere between them comes the Kerameikos Mug Group, where the author sees the *juvenilia* of the latter artist; for the time being it might be wiser to treat the Mug Painter as a separate hand, since we have nothing to bridge the gap between his somewhat finicky style and the broader, more assured drawing of mature Ram Jug work.

After the mid-century comes a period of eclecticism, when the Athenians combined lavish floral ornaments in the Cycladic manner with a tentative use of the Corinthian black-figure technique; the author has identified a new hand, the Pair Painter, as a typical child of this time. Of Beazley’s ‘Earliest Black Figure Painters’ only the Woman Painter is early enough to be admitted into this volume, where the latest pieces are dated to c. 620 B.C.

A few suggestions, and minor points of detail: Nos. 1 and 245: probably not by the Dipylon Painter himself, but a late product of his workshop. The one-wheel chariot and the floating sigmas are late features; the horse is best paralleled on the Baring Amphora. The prothesis fragment seems below standard; is it really from the same vase as the chariot frieze?

No. 311, the ‘Astyanax’ fr. Not Late Geometric, but conservative Early Protoattic, c. 690. Like the Passas Painter (cf. Hampe, *Grabfund*, fig. 25) this man exploits outline drawing to show drapery; the whirligigs and quadruple diamond clumps are also post-Geometric. If (as I believe) J. M. Cook is right in seeing the ‘censer’ of no. 336 as a child (*Gnomon* 1962), then the ‘Astyanax’ reading will be difficult to maintain.

Nos. 83, 84 = 359, 360, 361. This early group of figured olpai involves us deeply in the problem of Potter and Painter in eighth century Athens. The dogs of 83 and 84 = 359 are ascribed to the unimaginative workshop of Athens 897; the deer of 360 to the Statathou hand, a member of the more lively workshop of Athens 894 (p. 72). But the whole group was surely potted under the same roof; perhaps this consideration led the author to have second thoughts in her introductory essay, where the Statathou hand is transferred to the workshop of Athens 897. Some qualification is needed here, since his other works, including his masterpiece (Davidson, fig. 115) cannot be
detached from the circle of Athens 894. A way out of this impasse is suggested by the amphora Mannhein CV 4 I pl. 3, 2, where dogs and deer are combined. Here the deer point to the Stathatou hand; the dogs must therefore be his own lightweight imitations of the lumbering creatures on Athens 897, such as are found on other pieces associated with Athens 894 (e.g. Davison, Figs. 34–5; perhaps also no. 361 in this volume). The Stathatou hand may thus remain a regular member of the Athens 894 firm, who occasionally painted opolai made in the rival workshop; perhaps it was through him that the 897 dogs passed into the 894 repertoire.

No. 416. Cf. now Reading 54.8.1, Arch. Reports 1963, 56, Fig. 2, a more careless work by the same hand.

No. 534: surely by the Analatos Painter in his old age; cf. the sphinxes and crones, Hampe Grabfund, pls. 13 and 21. Such precise drawing makes it difficult to accept wild pieces like Kühler Alt. Mal. pl. 31–4 as late Analatos work.

Nos. 592–3. The marine ornament is seen as a local revival of a Mycenaean idea; yet white octopods could have been borrowed from Crete in the early seventh century (see Alexiou, K.Ch. 4, 1950, 294 ff., pl. 14–15). Conversely the ovoid krater no. 95, though by the author to be modelled on the Cretan cremation pithos, now has a low-footed prototype in Attic Middle Geometric (Arch. Reports 1961, 4, Fig. 2).

Finally, a word about Geometric chronology, and chronological subdivisions. It is not always clear what the author means by Late Geometric, which sometimes includes (p. 12; Hesp. xxx 95) but more often excludes (p. 8; under no. 343) the period of the Dipylon grave monuments. Precision is needed here; perhaps we should subdivide Late Geometric into L.G. I (= 'Dipylon' = Kahane's Ripe) and L.G. II (= Kahane's Late, beginning with Subdipylon and ending with the workshop of Athens 894). L.G. II is dated by imports and imitations of the Corinthian shallow kotyle: if we can believe, with Dr Brann, that imitations always keep pace with the originals (pp. 6, 50), then we must also believe Kühler's higher chronology, which launches Protoattic well before c. 725; for at this time the shallow Corinthian prototype was being supplanted by a deeper variety, and the latest of the Attic shallow kotylai already have Protoattic horses (Kühler, Alt. Mal. Figs. 2, 3). But here the evidence from the Agora Wells cannot help us much, since the average time-span is estimated by the author at about thirty years (Hesp. xxx, 98). More relevant to the problem is an Attic kotyle like Young C 28 (Hesp. Suppl. II, Fig. 109), showing clear signs of a time-lag; here the shape is still on the shallow side, yet the frieze of wirebirds betrays knowledge of the deep Corinthian version. Further evidence of Attic conservatism is furnished by the Lion Painter's shallow kotyle, which was found with a deepish Corinthian original (Cook, BSA 42, 143 ff., Figs. 4a, 6a); and since the Lion Painter preceded the prolific workshop of Athens, 897, Attic L.G. II must come well down into the last quarter of the century.

However, Dr Brann does well to remind us that 'people, not periods, produce pots'; taking this as her text, she has produced a charming and readable book, full of fresh observation, and rich in delightful character sketches of the leading painters. Author and material are ideally matched; only a scholar with her lively interest in personalities could do justice to the one Orientalising school that never lost sight of the human figure. Let us hope that Dr Brann has not said her last word on Protoattic.

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There is nothing surprising in a volume on Islamic coins figuring in the well-known series of excavation reports on the Agora. Long years of τουρκοκρατία passed also over Athens, and it is but natural that they should have left their numismatic mementoes in the form of coins. As was to be expected, almost all the coins found in the Agora (all but nine out of 6449) belong to the Ottoman period; of the others, the three coins by the ninth–tenth century Arab Amirs of Crete are of great interest. Although these coins are, from the point of view of the classical scholar, merely by-products of the excavation, the School has very commendably given them the benefit of full scholarly treatment and assigned the task of describing them to one of the foremost authorities on Islamic numismatics; and thus laid the student of Islamic—and more particularly Ottoman—coins under deep obligation. The author eloquently describes the difficulties encountered during the classification of these unartistic Turkish coins, crude and clumsy as they are"—"in fact the most intractable task of numismatic classification' he has ever undertaken. The earliest positively datable Ottoman coin from the Agora is a silver aşche of Mehmed I, 1413–21. The next earliest definitely attributable coins are of Bāyazīd II, 1428–1451—the author tentatively attributes to the intervening period some of the very crude coppers.

There are numerous coins from the sixteenth century—most of them (like those of the fifteenth century) copper (mangys), but a few silver (aşches). In the seventeenth century the volume of coins unearthed in the Agora decreases; but the author points out that this phenomenon does not seem to be restricted to Athens, but is characteristic for the whole Ottoman empire. There are numerous specimens of the new-style emergency issue of 1688, which is known to have been issued in great quantities; 'the quantity in circulation in Athens was perhaps not relatively any greater than in other parts of the Ottoman realm,
but there may well be some connexion between this quantity and the reoccupation of Athens by Turkish troops in 1688 and 1689 after the withdrawal of the Venetians. With the eighteenth century the volume increases; the coins are almost exclusively silver. Turkish coins seem to have circulated freely for some time even after the introduction of the new Greek coinage in 1829 and the prohibition of all use of Turkish coins in 1833.

There is a full discussion of the mints, among which preponderate, after Istanbul and Cairo, those of Macedonia, Serbia, and Bosnia. Though the name of Athens cannot be read on any coin, the author thinks that many of the crude coppers of the fifteenth-sixteenth century were locally struck. There are interesting notes on the various mint towns, among them a long argument in which one of the mints is tentatively identified as Tripolitza in the Peloponnesus.

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The Classical Association’s tribute to Athena Parthenos was published on approximately the twenty-four hundredth anniversary of the dedication of her gold and ivory statue within her new temple in Periclean Athens. The essays are devoted to the goddess and her temple, and although they are not designed to be descriptive they offer a good deal of basic information about the Parthenon as well as original comments on various matters. The Royal Ontario Museum in Toronto has recently equipped an ‘Athens gallery’ with models of the Acropolis, restored and painted details of architecture and relief sculpture, and a one-tenth model of the cult statue within her cells; and of these there are new and valuable pictures. If, in the Frontispiece, the goddess looks rather doll-like, we should remember to make allowance for scale and for the far less intense and direct light in which the original was displayed.

The essays vary considerably in length, value and direction. Outstanding are Meiggs’ on the Political Implications of the Parthenon, in which the Oath of Plataia, or something like it, is taken to have operated against the re-building of temples until after peace with Persia; and Robertson’s account of the sculptures of the Parthenon, including a new interpretation of the centrepiece of the frieze as the folding of the old peplos in preparation for the new, just as the main part of the frieze shows the preparations for the procession. Hopper gives a long, detailed and (in view of the nature of the evidence) courageous account of Athena’s history (even her prehistory) and the early Acropolis. Miss Burford writes about what is known of the builders of the Parthenon and the stages of building, from literary and inscriptive sources. This is an important matter of which it has not been easy to find a succinct account. Herington’s subject is the Athenian’s own attitude to the Parthenos as the symbol and protectress of his city, wielding an authority which derives immediately from father Zeus. This concept is traced from Solon to the tragedians, in the popularity of the Athena-birth story in art, and in the developing ‘national’ aspect of the Panathenaic Games. So much else in sixth-century Athenian cult and art seems to reflect a renewed interest in Homer that the reviewer at least is sorry to find the ‘Pisistratean recension’ so unfashionable today—and so, I fancy, is Herington. As bonnecouches there are rolling hexameters from a nineteenth-century Prize Poem on Parthenonis Ruinae, a modern poem, and a Profile of Athena as the Good Fairy who looks after little boys who may be king one day.

With Periclean Athens and Athena’s new home we are at the heart of classical studies. It is salutary to recall how much more there is still to be learnt and understood about these subjects and about the Acropolis itself, also how rapidly work on the very matters discussed in this book goes forward. The latest detailed study of the Mycenaean Acropolis (Jacovides, Η Μυκηναϊκή Ακρόπολις, 1963) offers a different picture to that given here (cf. Arch. Reports for 1962–3, 4); the ‘Mycenaean palace column base’ has been finally debunked (p. 7); and Desborough’s Latest Mycenaeans (1964) must now be considered in any account of the importance of early Athens and her citadel. Fewer now will cheerfully accept a connexion between Minoan snake goddesses and even a Mycenaean a-ta-na po-ti-na-ja. Did Phidias work at Olympia before (as p. 25) or after (as p. 46) the Parthenos? German excavations of his workshop at Olympia have answered for the latter. We still cannot name most of the figures on the Parthenon pediments and significant new fragments are still being identified and added to the composition. We may expect some, but not all of the answers to be found in time. Meanwhile those who think they know Athens and the Parthenon may find much in this modest book to learn and reflect upon; strangers to Athens will be led with humane scholarship into the presence; and the goddess herself will surely smile upon such a dedication.

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In Khirikilia (1953) Dr Dikaios published an account of the earliest occupied settlement site in Cyprus, revealing a community (now classified as Neolithic I) to whom the use of pottery was unknown. Carbon–14 subsequently suggested a date early in the sixth millennium B.C. for the founding of the site.
Nearly twenty years before, he had described another type site, Erimi, whose culture is typified by round houses, handsome painted pottery in abundance, and a minimal use of copper artifacts. The florisit of Erimi (Chalcolithic I) has been dated c. 2700 B.C. by C.-14 tests. With Sotira D. reports on the third and last of his major excavations at these very early Cypriot settlements, in which he describes a culture (Neolithic II) to be set between Khirikotia and Erimi in the Cypriot sequence. The excavation was made on behalf of the Pennsylvania University Museum as part of their long-standing programme of work at Curium and its environs.

D. discovered Sotira in the course of his field-survey of Cyprus in search of Neolithic settlements; it was found on the same day as Khirikotia. Like Erimi and Khirikotia, Sotira is in southern Cyprus. The settlement occupies the level summit and slopes of a hill, Teppes, in broken country north of Curium, five miles north-west of Erimi, twenty-five miles west of Khirikotia. D. cleared the hill-top and made soundings on the slopes. A few graves were found at the hill-foot but, unlike Khirikotia and Erimi, no infra-mural burials came to light. Within the settlement over forty houses were cleared, belonging to four distinct structural phases, of which the first was destroyed by fire, the third (and best represented) by an earthquake. The last phase was apparently short-lived and consisted merely of impoverished structures raised on old foundations after the survivors had cleared away much of the earthquake debris, piling it into a massive ‘retaining-wall’ on the north side of the hill-top. The final abandonment came without violence from man or nature.

D. describes the architecture and stratigraphy area by area in full detail and with admirable lucidity, supported by very clearly drawn plans and sections. Houses were built of crude brick, or pisé on stone socles. The commonest type was rectangular in plan, with rounded corners. Post sockets in many floors show that roofs—sometimes with a span of over five metres—required intermediary supports. The houses were equipped with built hearths and a variety of other permanent domestic installations.

The Sotira culture is characterised by a very distinctive pottery. The reddish biscuit is covered with a polished red slip and the vases decorated in bands of different types by combing away the slip before it had dried. Shapes include large shallow bowls, often with spouts, and rather baggy narrow-necked jars. D. also describes and illustrates tools of stone, bone and antler, stone ornaments and vessels, and a limestone idol. Appendices report on the human crania (Angel), on animal bones, which include domesticated dog and sheep (Zeuner and Ellis), on shells (Dance) and on the flint industry (Stekelis).

At Khirikotia, the pre-pottery sequence was succeeded by a final occupation typified by Sotira-type combed ware. Not unnaturally, D. was originally led to believe that Neolithic II, wherever it came from, followed more or less immediately after Neolithic I, though he was careful to demonstrate the many differences between the two cultures. But Carbon-14 has fixed Sotira level I at 3,500 ± 130 B.C., level III at 3,190 ± 130 B.C. with a consequent gap of well over a thousand years between Khirikotia and Sotira. It is now hardly surprising to find that the skull types, house plans, burial rites, and flint industries at the two sites are so divergent, that the Sotira stone vessels are so poor by contrast with Khirikotia, to mention only some of the more obvious differences.

For the time being this gap in the Neolithic sequence for South Cyprus remains open. D. suggests that, for the north coast at any rate, the sequence at Troulli, where he has found a distinctive pottery fabric with reserved ornament stratified over a pre-pottery level, may go some way towards completing the sequence in general. Unfortunately, however, Sotira-stage material is so far almost wholly confined to the south side of Cyprus, and thus Sotira and ceramic Troulli could be contemporary local cultures. How eager one is to know what brought the pre-pottery cultures of Cyprus to their end, whether the island was thereafter uninhabited, from what source did it receive its next settlers, and after how long an interval.

There can be nothing but admiration and gratitude to Dr Dikaios for his lifetime’s work on the Neolithic cultures of Cyprus, and for the exemplary manner in which he has presented the results of his field work, of his excavations, and of his mature reflections on their implications.

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H. W. Catling.


In his Introduction the author explains the reasons which have unavoidably retarded this, his definitive study of the reliefs in the city-gates of ancient Thasos (modern Limenas). Even those inclined to be restive at such delays must nevertheless offer their homage to the scholar who first reconnitred the site with A. J. Reinach in 1910, realised its great potential, and led, with Ch. Avezou, the excavations there until 1914; and the great range of his knowledge over the fields of classical sculpture and ancient cult-history makes it doubly suitable that this part especially of the Études thasiennes should be his work.

The subject itself is unique; as he reminds us, no other Greek city’s walls survive with gates so elaborately ornamented. Indeed, he thinks it likely that this sculptural decoration, though frequent in the kingdoms east and south of the Aegean, was never normal in archaic Greece: so that Thasos, and perhaps a few northern neighbours, might have got the idea
from the east. (I am not competent to assess the case of those scholars who think it possible that these Thasian reliefs were not made originally for the gates, but were reset there from elsewhere in the city.)

The non-Greek examples, actual and conjectural, are gathered and fully discussed in Ch. 1. The next four chapters describe the five Thasian gates in chronological order. Ch. 2 deals with the large N.W. gate, whose surviving relief (Herakles the archer) is the most clearly apotropaic of the lot, backed as it is by its original inscription—the only one surviving at any of the gates—which labels the deities Herakles and Dionysos as ‘guards of the city’ (IG xi2. 8,356). It is also the most clearly datable of the sculptures, the style of the relief and letter-forms alike pointing towards the close of the sixth century. Of the reliefs at the other gates, the Silen (ch. 3) is here dated also to the years before 494. Those of the goddess driving a chariot (Hera Henioche or, as others prefer, Artemis Polo?) and the Anodos of Semele (or Hermes and the Charites?) are ascribed to the period c. 494–2; i.e., all these were spared in the dismantling of the fortifications in 492/1 and 464/3, though they may have been removed and then reset later. The latest relief, that of the ‘Gate of Zeus(?),’ Professor Picard ascribes to the revolution of 411, and holds to be archaizing. Some may still prefer to set the Silen c. 10–20 years later, and the ‘Zeus’ figures in the second quarter of the fifth century; but, as the author observes, the battered state of all the reliefs in the smaller gates forbids any confident ascription.

Sometimes text and illustrations do not quite harmonise. Plan A, set among the text-figures, is referred to as ‘Plan I, Plate I,’ and, unfortunately, has had to be so much reduced that its legends are barely legible. Several text-figures recur in different places, an unusual luxury (Figs. 3 and 51 are the same; 7 shows an original drawing, and 29 a copy of it; 11 reappears, at nearly the same scale, in two halves, 4 and 17). The same kind of repetition appears occasionally in the text, e.g. on p. 53 the lost relief of Dionysos and the Maenads is introduced as though for the first time, although it has been introduced already on pp. 47 f. The plans and elevations are mostly those of the Danish architect Sven Risson, made between 1913 and 1915; now and then the average reader needs a newer one, or even a sketch, to illustrate the author’s description of the present state of some part of the wall, especially when he is proposing a restoration: e.g. the reconstruction of the group inside the first, largest gate (a cult-niche containing a (conjectured) Zeus, flanked to R. by the surviving Herakles, to L. by a (conjectured) Dionysos which was replaced in antiquity by a (transferred) inscription IG xii2. 8,356). Some of the early photographs used in the Plates could carry more detailed captions (Plate 41 has none at all): e.g. the same average reader, referred (p. 59) to Plate 10, may well be unable to identify there, unaided, the inscription cited.

But such matters, though occasionally hampering
to the student, are details of presentation only. There remains a vast store-house of information and references covering innumerable topics of ancient art, literature, religion, and folklore, garnered here and supplied with an index that shows its encyclopaedic variety, from the Abantes’ hairstyle to the early pharaoh Zoser.

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L. H. Jeffery.


This admirable book is divided into two parts, the first being a critical study of the monuments themselves, the second a comparative discussion of the characteristics of Argolid and Peloponnesian architecture during the fourth and third centuries B.C. Not surprisingly, it is dominated by the buildings in the sanctuary of Asklepios at Epidaurus, the development of which is essentially the work of these two centuries, buildings from elsewhere in the Argolid being confined to the Temple of Hera in the Argive Heraion, a monumental triglyph altar in the same sanctuary, and four minor structures (none well preserved) at the Argive sanctuary of Pythian Apollo.

The first part is prefaced by an account of the Temple of Apollo at Bassae, which Roux sees as the archetype of a fourth century Peloponnesian style, in contradistinction to the Athenian influence discernible at the Argive Heraion. He follows Dinsmoor in attributing two main building periods to this temple, the first at the middle of the fifth century, or a little earlier. The second, involving an alteration of plan for the interior of the cella, includes the Ionic and Corinthian columns and entablature, together with the roof; this Roux would date to the early part of the fourth century (400–370), and so, chronologically, bring it closer to the buildings at Epidaurus. Pausanias’ attribution of the temple to Bassae to the Athenian architect Ictinus is naturally rejected (another similar casualty is ‘Polycleitus the younger’), supposed architect of the tholos and theatre at Epidaurus, who becomes a mere muddle-headed and erroneous attribution, by Pausanias, to the fifth century sculptor. I am not convinced by Roux’s arguments about the date of Bassae. The difference between the two phases seems to me to be less than the difference between the Ionic and Corinthian of Bassae and Epidaurus. I should prefer, with Miss Shoe, to put the second phase at the end of the fifth century.

The second part includes a study of the three orders, from which it emerges quite clearly that both in the Argolid and in Mainland Greece in general, Doric remained the principal external order, even in Hellenistic times (there is one anomalous third century Ionic pseudo-peripteral temple, I., at Epidaurus, which I omitted from my discussion of
this topic in *JHS* 1963), while Ionic and particularly Corinthian were developed as internal orders, often being merely decorative, and not at all essential structurally.

Roux's basic thesis, that there is a recognisable Argolid style of architecture, which is not without influence elsewhere in Hellenistic times, seems to me entirely sound. Its development of the Corinthian order is of fundamental importance. It also seems, failing other evidence, to have originated, or at least propagated, the four-volute Ionic capital, the basic idea of which is similar to Corinthian. It uses, both for Corinthian and Ionic, a shaft with only twenty flutes. Roux's list of examples (annexe IV, p. 417) proves conclusively that this was normal in mainland Greece, rather than the Attic-Ionic standard of twenty-four. My only hesitation about all this is that the Argolid type seems so widespread in mainland Greece that I wonder to what extent it was developed specifically in the Argolid. If Apollo Bassae is to be the prime cause of all this, the influence could well have spread to several different places. Roux himself puts Bassae at the head of a Peloponnesian, rather than an Argolid tradition. Inevitably, the argument is affected by the relative wealth of material from Epidaurus. The truth must be that there were several local variants of a general Peloponnesian tradition, with perhaps slight differences between them. All the same, on the admittedly partial evidence available, Epidaurus seems to have been the most progressive.

I can discuss only a few of the numerous points of interest. There is much in this book to praise. Roux's ideas on the purpose of the tholos at Epidaurus, that it represents a cenotaph of the hero-become-god Asklepios, are by far the most convincing of the many interpretations of this enigmatic structure. He stresses the funereal associations of the lavish ornament on this building. His sceptical approach to the (unfinished!) 'architect's model' of the Corinthian capital of the tholos found buried near by is salutary; it becomes merely an abandoned Roman copy. His explanation of the elongated proportions of the external Doric order of this building is convincing; viewed at a distance circular buildings often appear abnormally wide, and so require additional column height to counteract this. I am not so convinced by the suggestion that the Doric orders of the tholoi resulted in the generally slender Doric of the later fourth century and Hellenistic times, which I think is caused more by a desire to equate the orders, particularly when they were being used in one and the same building.

The few other criticisms I have are all minor. I am not happy about Roux's restoration of the roof timbers over the cela at Bassae, which become the prototype for other roofs at Epidaurus. He suggests rafter beams only, rising to the ridge without horizontal cross beams to give central support. Admittedly, Hodge has already tentatively suggested a roof without a horizontal ceiling, but Roux's version seems unstable, with the weight of the tiles forcing the ridge down and the ends of the rafters out. The temple of the Athenians at Delos, quoted by Roux as a parallel, has one of Hodge's 'Gaggera roofs'; the Python at Delos, Roux's other parallel, has a narrower span with support nearer the centre. There is a discrepancy between Roux's description of the façade to the two-aisled stoa at the sanctuary of Apollo Argos (p. 73, five columns in antis) and the plan (p. 66, fig. 9, three columns in antis). The plan of the workshop at Epidaurus and the adjacent buildings A and B (p. 87, fig. 15) differs from that in the general plan, pl. 26. There are some curious exceptions in the sequences of proportions given in annexe I (e.g. table 5, where a declining sequence is suddenly reversed for the tholos at Delphi, the opisthodomus of Bassae, and the pronaoi of Bassae).

The book is excellently produced. I noticed an occasional misplaced letter (dus écle) but nothing more, though the recent Director of the British School at Athens twice appears as 'M. Wood'. Is it mere insular prejudice to wish that the French could bind at least their more expensive books? This one is quoted in the lists of an English bookseller at £7 12s. The plates are contained in an admirable cloth-bound folder. Yet the book itself has a miserable paper cover, which had come off by the time I had finished reading.

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For more than ten years Ginouvé has studied and published articles on the bathing establishments of Greece. The studies that he contributed to *BCH* led up to the full publication in 1959 of the complex bath system at Gortys in Arcadia. Now, in *Balaneutikè*, he presents the first full scale treatment of the bathing habits of the Greeks, revealing what a significant place water held in the everyday and religious life of the ancient Greek communities. The material considered is various: the architecture of the bath houses, the objects used in bathing, the paintings, mainly on vases, showing the buildings and objects in use, the literary evidence, including papyri and inscriptions, and the knowledge gained from excavations. The net is cast wide, and Ginouvé is expert in organisation. There is naturally much pageturning, back to previous discussions and references, forward to plates of photographs and plans, but this is a necessary evil, common to books of this size and scope which contain illustrative material.

The first part of the book is concerned with the objects used for bathing: the fountain, the long bath, the round bath, the footbath, the bowl on fluted stand. Most useful here are the discussions of the Greek words, and of many thoughtful suggestions
about representations, one may be singled out: Ginouves’s attractive interpretation of the scene common in red-figure of ‘a boy washing’ as a grape-treader, and his tub not a wash-tub, but a vat (p. 52).

The second part, on cleanliness and everyday life, is divided into three chapters, dealing with bathing in the open air, bathing at home and bathing in public baths. Ginouves’s statement (p. 123) on Aphrodite in art that ‘tous ces documents nous renseignent bien plus sur les goûts d’une société, que ses habitudes’ is a point well made and has wide application; but later (p. 174) Ginouves comes to the conclusion that the attention which artists pay to the theme of the bath, reflects the increasing importance cleanliness was having in real life. The connections of public baths and gymnasia are well traced, and Ginouves’s general conclusion is that household baths did not afford people a sufficiently good bath, and they were willing to pay a small sum to visit the public baths for a good wash and also, one presumes, for a good talk.

The final and longest section concerns the use of water in religious rites. All are in a sense acts of purification, both actual and spiritual cleansing. Valuable here is Ginouves’s discussion of the objects used for cleansing in household ceremonies—ardanion, louteron, loutrophoros, hydria, lebes gamikos, and later a similar treatment of the objects used in cult—the perirrhinon and the chernobio. Ginouves has much to say on the puissance inspiratrice of water, the active rôle it plays in recharging the body, and at one point (p. 404) he writes of ‘une technique destinée à mettre le fidèle en contact avec l’eau, puissance divine, dont il devait tirer quelque bienfait, révélation, guérison, ou accession à un plan religieux supérieur’. In conclusion, all the ritual meanings of water are given: water as purifier of body and soul, water as remover of spiritual stains, of the past, a washing away of guilt, water as death and rebirth, water as fecundity, water as a virile and potent force. Ginouves’s remarks on the connection between the Greek use of water in religion and the Christian (‘the mystical washing away of sin’), embracing a consideration of ‘cures’ and of the efficacy of such grottoes as Lourdes, make one hope that he will treat this subject more fully in a later book.

The following notes are small corrections or additions of detail. P. 27. The object held by the woman bather on fig. 4 is surely a double comb, as Ginouves suggests; see the representation on a fragment in Rome: Zschietzschmann Hellas and Rome (1959) p. 279, top. P. 33, note 2. Add now my list of bakers and discussions of related topics in JHS xxxii, 1962. P. 37. May not the higher seat of the classical bath tub be intended to save the bather from sitting in dirty water? P. 40. Ginouves description of the Bologna cup, fig. 23, is adequate, but incorrect in certain details. ‘Pithos’ is surely wrong, it is more likely a lebes, and the stand is better explained as made of metal, not wood, the whole being heating equipment for the bath.

For fig. 150, read fig. 151. P. 52. Add to the previous discussions of ‘the boy washing’, Smith in CVA San Francisco 1 (10) text to pl. 22 (482) 1. P. 56. The word amis should not be used with reference to women, it is exclusively a man’s article; skaphion is better. Pp. 77-79. The louteron has received a barrage of scholastic treatment; see most recently Shefton in Hesperia xxxi, 1962, pp. 390-968. A detail Ginouves seems not to have noticed but which is clear in many extant specimens is the roughening of the upper surface of the bowl, sometimes embellished with elaborate patterns of dots. This roughening is of use in the preparation of solids by hand-grinding or kneading, but of no obvious practical use for water. Ginouves overstresses the louteron’s connection with water. Pp. 112-113. The Laconian cup from Samos, which seems closely connected with the problem of women bathing, merited a picture, if only a reproduction of Boehlau. P. 175. For fig. 149, read fig. 150. The references to the plans are incorrectly numbered in the text; correct from the list of illustrations on p. 508, which tells the truth. Pp. 275-276. The Golycho hydria is now in the National Museum, Warsaw inv. 142290, and add Beazley’s discussion in VPol pp. 40-44. P. 277. Much speculation on the use of the lebes gamikos as a receptacle for heating water would have been saved, had it been realised that no fine ware vase—black-figured, red-figured, black or plain—can be used as a heater, because the pot will crack almost immediately. P. 301. For fig. 135, add Beazley’s publication in VPol pp. 25-26 and pl. 8, 2. P. 387, note 3. On the position of the sanctuary of Dionysos en Limnais, see now JHS lxxx, 1960, pp. 112-117. The above notes are insignificant, and I mention only one major omission: Amyx’s commentary on the text of the Attic Stelai in Hesperia xxvii, 1958, pp. 163-310, where many of the words treated by Ginouves have already been studied in detail.

The book is supplied with six indexes: sites, museums, Greek words, papyrological texts, literary texts and epigraphical texts. The absence of a general index is no real loss, as the casual reader will find what he wants in one index or another, or by way of the plates. The last would however have benefited from the addition of page numbers to the discussion in the text, but they are to be found in the valuable table of illustrations on p. 508. The reproduction of the photographs is clear, and the photographs themselves are large. The plan, fig. 137, is out of register, but otherwise the plans are crisp, figs. 157 and 158 of the Piraeus baths being particularly fine.

As motto for the book, Ginouves quotes Pindar’s ‘water’s best; for English readers, perhaps The Ancient Mariner comes as readily to mind, for all that Ginouves’s bathers omit to do is drink. His readers will not fail to do so, and always with pleasure and gratitude for a task fastidiously performed.

B. A. Sparkses.

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NOTICES OF BOOKS


Many years ago a misguided man wrote of ‘die von die Französische in Delphi beliehte methode unbequeme steine zu ignorieren’. This was false when written, although it must be admitted that in the enthusiasm of the first campaigns quite a few stones were overlooked; now it has become ludicrous and this volume of studies is just one more proof of the extraordinary command of the material of all sorts, bequeme and unbequeme, the feeling for the site in all its aspects which Delphi and the French School between them have bred in succeeding generations of ‘Delphiens’.

The essays, five in all, have no common theme, though several of them point the same moral, that Pausanias was regularly right and E. Bourget sometimes wrong, and all but one (no. 4) are similarly concerned with a minute study of some random stones and fragments which, independently or together, Messrs Roux and Pouilloux believe that they can reduce to some sort of order.

In Ch. I M. Roux re-examines the whole question of the disposition of the monuments in the first 35 yards or so of the Sacred Way. The core of his argument is that the niche on the northern side of the way must be later than the fourth century Arkadian base which fronts it and cannot therefore retain its by now almost traditional title ‘L’ex-voto d’Aigos Potamoi’. The latter belongs, where Pausanias put it, immediately inside the entrance on the left. But for M. Roux a core is there to carry flesh, and, while ferrying Lysander across the street, he contrives to raise and settle a dozen other issues of no less interest—most strikingly the Argive dedications (in particular the Horse and the semicircle of the Kings) are given a very new look. Eyebrows may be raised at some of his gayer suggestions but no one, I think, will deny that on several major points Delphic guides of the future will have to change their patter.

Roux’s second chapter is less exciting, an attempt to settle the non-existent superstructure of the treasury of Brasidas and the Akanthians on the miserable remains of Foundation XVI, inside Gate C on the east side of the sanctuary. Here he may be right or he may be wrong, the pitiful evidence rules out decision, but again there is the same acute observation, the same meticulous attention to detail (e.g. in his rerouting of the path from Gate C towards the Sacred Way).

In ch. III M. Pouilloux takes over and the pace slackens for a leisurely (a little too leisurely?) but none the less fascinating search for the site and source of the fountain Kassotis, which is finally discovered due north of the NE corner of the temple and, again where Pausanias put it, directly below the Knidian Lesche. An incidental examination of the source for the fountain on the terrace below the temple leads into the foundations of the temple itself and as with everything else there, into darkness.

Ch. IV is not archaeological; a persuasive reinterpretation by Pouilloux of Euripides’ Andromache vv. 1085–1157 which reconciles Euripides with the tradition and with the architecture though it saddles Neoptolemos with a complicated itinerary and a mighty leap (from Chian altar to temple peristyle—(a quibble: need he go quite so far? If indeed he leapt ‘au-dessus de la masse de ses assaillants’ they would flee not into the temple but away from it. Can he not rather land in the middle?)

In Ch. V Pouilloux and Roux come together to conjure up from two miserable limestone fragments a complete base for the Akanthos-column to be sandwiched between the bases of Daochos and the Corecyreans and the temenos of Neoptolemos. They then try their hands at the modern game of refuting excavators from their own dig notebooks and, it seems to me successfully, argue that the traditional date for the column can easily and comfortably be lowered to the second half of the fourth century.

Hard covers, spacious pages, an invaluable revision of the plan of the sanctuary which appeared in M. de la Coste-Messelière’s ‘Au Musée de Delphes’, and 26 plates of the standard we expect from E. de Boccard, all these give the book a sumptuous air, almost too sumptuous for these studies of detail. On the other hand, the authors themselves are far too modest. ‘Nous aurions attenir notre but, si nous avions réussi à apporter plus de clarté dans ces questions délicates’. They have done very much more than that.

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W. G. FORREST.


Brommer’s Vasenlisten zur griechischen Heldensagen has facilitated and encouraged work on the iconography of Greek legend, and the book under review is an interesting contribution. Beckel begins with a discussion of the rôle of the gods in their direct relations with heroes in the Iliad and Odyssey, and proceeds to the illustrations of these, noting the differences imposed by and arising out of the transposition from words to visual image. Thence he moves to the illustrations of other Trojan episodes whose original epic sources are lost to us: Troilus, Achilles and Memnon, Achilles and Ajax at play, the quarrel over Achilles’ armour, and certain episodes from the sack. The choice is selective and he does not touch the non-Trojan cyclic epics, but devotes the bulk of the book to certain great individual heroes who were helped by deities in their adventures or labours: Perseus, Herakles (naturally the longest section, further increased by appendices on two of the Olympia metopes), Theseus, Bellerophon,
Kadmos and Jason. Then there are valuable sections on the pediments at Aegina and Olympia, and finally (after the notes which give very full documentation) a useful and blessedly selective list of monuments which serves also as an index and is supplemented by other indices.

Beckel takes pains to distinguish between the true divine helper, with whom alone he is really concerned, and the divine onlooker whom it is easy to confuse with the other and who was certainly sometimes so confused by the ancient artists. He analyses recurrent but sometimes changing, types of grouping and gesture between deity and hero, indicating the imparting of μένος or help by counsel or by teaching him a trick or lending him magic accoutrements. Athena appears far more often than any other deity in all these aspects of the Fairy Godmother (Beckel's theme brings out strikingly the basic likeness of these to fairy-stories).

There are many excellent observations aside from the main theme: for instance (p. 32) that the Athenai on the protoattic amphora in Elexis with Perseus and the gorgons is the oldest certain representation of a deity in Greek art; or (p. 40) that the widespread popularity and essential homogeneity of Perseus-pictures in early archaic art suggests the existence of an epic. A few small points. P. 49. Alkyoneus and Antaios cannot always be distinguished, but some further confusion seems to have got in here; I think at some point their names have been exchanged. P. 65: I find it hard to believe that the youth with spear on the Salonica vase No. 66 is really Hermes. P. 134, No. V: the dating of these three black vases with impressed (hardly relief) decoration to the second half of the fourth century ('4th century again' on p. 35) is surely a full hundred years too late.

In discussing the Aegina pediments (p. 80 f.) Beckel rightly emphasises the extremely slight grounds on which Furtwängler's generally accepted interpretations are based. Nevertheless I feel doubtful whether they can really be meant only generically as 'heroic battles' and not as illustrating particular stories.

MARTIN ROBERTSON.

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Corpus vasorum antiquorum. France, fasc. 19.

Musée de Laon, fasc. 1.


The Laon vases are the collection of Paul Marguerite de La Charlonie, bequeathed to the city in 1921. The present fascicule contains most of the Attic vases; the rest of the collection will be published later. The reproductions are very good. Madame de la Genière has studied the vases with care and understanding, and has provided an admirable commentary.

Black-figure. Pl. 4,3 and pl. 5 is by the Bucci

Paintier. In pl. 9,3-4 the white parts are repainted. The subject is no doubt Herakles and Antaios as Bothmer said. Pl. 10,3 and 6: Class, as well as Painter, of Vatican G.49. Pl. 11,6-7: Group of Vatican G.50; compare especially the Pitt Rivers oinochoe published by van Hoorn (Choea fig. 169).

Beckel (13,1-2 and pl. 13,3-5 both belong, for shape and ornament, to Phanyllis Class A. In pl. 13,3-5 are not both onlookers male? Pl. 16,6-7, Little Lion Class; compare Agora P24328. Pl. 17,6-8: the upright object is the stone lid of the pithos. Pl. 21,3-4 is well compared in the text with Louvre CA3083 and the minor works of Elbows Out; pl. 21,2 and 5 with Villa Giulia M.591; pl. 24,1-2, with Thebes R.5,18. Pl. 25,2-5: tail-end of the Haimon Group, near the Lancut Group. For the shape, the reference should be to ABV, p. 567, iii, rather than to ABV, p. 565. The graffito still requires explanation. Pl. 25,6-7: a replica by the same hand, from Al Mina, Oxford 1951-559 (ABV, p. 566 no. 612) makes it clear that the subject is Poseidon overwhelming the giant.

Red-figure. Pl. 25-26, already assigned to the Pan Painter in VA. p. 117 no. 2, although one of his slighter pieces, is the finest of the red-figure vases in Laon. It is not a 'Nolan amphora' in the strict sense; it goes with Naples Stg. 225 (Jb. 76 p. 68). The herm on the left is unfinished. Pl. 31,1-2 and 5 is in the manner of the Kleophon Painter (ARV² p. 1148 no. 4). Pl. 31,3-4 and 6 is also in his manner, but less close to him; the bell-krater Petit Palais 317 (CV. pl. 21,3-4 and pl. 22,1-6 and 8) may be by the same hand. Pl. 36,1 and pl. 37,1: compare the pelikai by the Painter of London E356. Pl. 36,2 and pl. 37,2: the rougher Altenburg 296 is well compared. Pl. 36,3 and pl. 38,1: by the Curti Painter (ARV² p. 1042 no. 3). The object between the two right-hand figures on B is an aryballos, Pl. 36,4 and pl. 38,2: by the Painter of Bologna 322 (ARV² p. 1170 no. 8). Pl. 39,2: related to the F.B. Group (ARV² p. 1493 no. 6). Pl. 39,7 and 11 by the Painter of Brussels R330: for the lid compare his askos Oxford 328 (CV. pl. 45,5). Pl. 39,6-8: for the various uses of 'oinochoe VIII', V. Pol. 60. Pl. 41,6: not this, but pl. 43,5 is the lekythos which I assigned to the Klügmann Painter. Pl. 42,7 is by the Providence Painter himself. Pl. 43,4: the shape (except perhaps the mouth) is that favoured by the Carlsruhe Painter, but the drawing is not his. Pl. 44,1 and pl. 44,2 are both by the Dessypri Painter (ARV² p. 1198 nos. 11 and 10, and p. 1686). Pl. 46 and pl. 47,1: as Florence 3965 (CV. pl. 82). Modern, head and forehand of the horse on the left in pl. 46,1. Pl. 47,2 and 4: the right foot is modern. Pl. 48,2,5-6, and 8: connected with the Curtius Painter in ARV² p. 935, foot, where a Ferrara fragment is put with it. Pl. 48,1,3-4, and 7: by a follower of Makron (ARV² p. 812 no. 59 bis). The thing on the plate is a loaf. Pl. 49,5: the satyr holds torches rather than flutes. Pl. 52: Group of Ferrara T.981 (ARV² p. 1676 no. 10 bis). The Genoa pelike
NOTICES OF BOOKS

quoted in the test is not by the Berlin Painter but by the Syriskos Painter.
There is no date on the title-page.

J. D. BEAZLEY.

Oxford.


These two parts deal with most of the Attic red-figured cups, stemless cups, skyphoi, pyxides, oinochoai in the Berlin Museum, and the fine photographs—general views, and many details of each piece—with the careful and understanding descriptions, form a splendid record. All the vases have been cleaned before photographing, and the restorations removed. Three cups have been augmented by the fragments, from Italian museums, recognised in Campana Fragments as belonging. Some of the cup-exteriors, to avoid foreshortening the heads, have been photographed from what seems too high a point, so that the lower parts of the figures are diminished (compare AJA 1954 p. 170); and the brown inner markings are not always brought out (contrast pl. 51,5 with pl. 51,6): otherwise the photography deserves high praise. So does the whole publication.

Pl. 56,1–3: for the name Philokomos see Eph. 1955 pp. 202–3. Pl. 59,4: is the girl really sleeping? Pl. 63,1–2: for satyrs assaulting sleeping maenads, CB, ii pp. 96–98. Pl. 70,3: for the subject, compare Athens Acr. 197 (Langlotz pl. 9). Pl. 72: on the Foundry cup, AJA 1962 p. 236. Remembering some Greek friends, one asks whether the man on pl. 76,4 is really non-Greek. Pl. 77: see also AJA 1948 pp. 337–B. Pl. 83: the Louvre replica of the Theseus and Sinis is now published, with the restorations nearly all removed, in Ginnouis Bulanatuké pl. 11,37. Pl. 87: the things strung on the branches cannot be heavy, and may be either honeycombs or wafer-like cakes. Pl. 94,3 is figured in Klein Lieblingsinschriften p. 169. Pl. 140: Eos and the body of Memnon is also the subject of the cup-fragments Acropolis 206 (Langlotz pl. 9).

Pl. 54,1–3, 65,1, and pl. 123, 1 and 5: unworthy of Oltos, yet by him. Pl. 64,3–4: the vases described in ARV² pp. 243–4 in the early manner of the Triptolemos Painter are in fact his. Pl. 69: in ARV² p. 1649 I consider whether this fine, very Bryan cup may not be by his follower the Dokimasia Painter. Pl. 92,3–4 is by the Bordeautパン Painter (ARV² p. 835 no. 1), pl. 93,1–2 and 6, by the Angular Painter (ARV¹ p. 612 no. 13).

In ARV² pp. 601–3 I drew up a list of vases 'in the manner of the Painter of Bologna 417' and added that nearly all of them were 'by the Mouse Painter', named after the pyxis with cats and mice, Berlin 2517 (pl. 137,6): I see now that the vases attributed to the Mouse Painter are in fact by the Painter of Bologna 417 himself.

The skyphos with two Persians (pl. 141,2 and 5–6) recalls both the early classic followers of Douris, and the Group of Ferrara T.981. The small skyphos 2318, published for the first time in pl. 140, must be by the same hand as the Anchises cup in the Vatican (Mus. Greg. ii pl. 85,2: Gymnastium 67 pl. 17,2).

Oxford.


It may have been noticed that among the fascicles of the Corpus Vasorum there continue to be good ones and less good. The first Swiss fascicle is one of the good. It contains the small collections of Dimini fragments, of Mycenaean, of Attic geometric, and the larger collections of Attic red-figure and of black vases. Black vases have often received poor treatment in the Corpus; Miss Bruckner has studied them with particular care and has added to our knowledge.

Pl. 7 is by the Painter of Berlin 2268 (ARV² p. 154 no. 7). The satyr inside carries not wineskins but baskets, probably containing fish (see CF p. 12). Pl. 12, 1–2: compare, within the Median circle, the Painter of Athens 1243 (ARV² pp. 1919–20). Pl. 12, 7–9 is by the Trieste Painter; his other works are Trieste 666.13, London E112, Altenburg 236 (CV pl. 71). Pl. 14 and pl. 16, 1–3: Miss Bruckner calls the drawing maladroite: rather, grotesque. Pl. 15: within the Group of Polygnotos, not far from the Peleus and Hector Painters. Pl. 17, 1–3 and pl. 18, 1–2: Miss Bruckner compares the oinochoe with Brussels oinochoai A719 and A720: the shape is indeed very close to those by the Painter of the Brussels Oinochoai, and the potter might be the same (see also ARV² p. 636). Pl. 17, 9 and pl. 18, 4: by the Painter of London E395 (ARV² p. 1149 no. 17). Pl. 20, 1 and 3: certainly by the Pothis Painter (ARV² p. 1189 no. 6). Pl. 20, 2 and 4, is poorish work by the Phiale Painter (ARV² p. 1019 no. 79). Pl. 21, 3–4 is already in Passeri, pl. 257; Group G (ARV² p. 1462 no. 1). Pl. 22, 11: by the Straggly Painter. Pl. 22, 12 and 20: Group of London E614 (ARV² p. 1707). Pl. 22, 22: tradition of the Seirenskae Painter; Lecce 596 (ARV² p. 1667 no. 8) is a replica by the same hand. Pl. 23, 3: for these see Raccolta Guglielmi i p. 51.

Oxford.


The aim of this study is to arrive at a full stylistic classification of the Attic red-figure drinking-vessels
in the form of an animal's head. As the author himself points out, only a few of the lattermost of these (nos. 96, 99, 104, 112, 114, 118 in his list) are actually pierced to function as rhyta. The remainder simply represent various forms of kantharoid drinking-vessels, with or without a stem and usually possessing only a single handle; their origins are discussed in greater detail in K. Tuchelt's *Tiergefäße in Kopf- und Protomengestalt*, reviewed elsewhere in this issue. Not included, on the other hand, are the more ornate Attic red-figure rhyta not of animal-head form, such as the Sotadean sphinx in the British Museum (presumably as having already been adequately dealt with by Buschor), together with the animals' hoofs, knuckle-bones and other ceramic oddities, although useful light is none the less shed on certain of these vessels and on animal-head vessels of lekythoid, etc., form. Also, new attributions are offered for the painted decoration of several of the vessels dealt with, viz. nos. 35, 49, 62, 67, 76, 83, 88, 91–3, 98, 105, 109, 112.

Altogether 126 animal-head vessels are described and analysed stylistically and just over half of these are illustrated in the excellent half-tone plates. The list of these vessels does not claim to be exhaustive, but the complementary but less enlightened catalogue contained in Tuchelt's book seems to offer no really significant additions. All save six of the items considered are attributed to one or another of eleven different 'classes', representing the products of individual workshops. This classification is closely modelled on that evolved by Beazley for the Attic head-vases (*JHS* xlix (1929) 38 f., *ARV* 892 f.) and, indeed, certain of the workshop units are regarded as corresponding exactly. The early 5th-century beginnings of the red-figure animal-head vases in Athens are traced in the Early Ram Class and their first flowering in the donkeys, rams and hounds of the Brygan Class. The tradition is maintained by the Dourian Class and, in a debased form, by the Miniature Class. The range of types is greatly increased with the Sotadean Class, although at some points there is a sacrifice of quality to large-scale production. The workshops of the second half of the 5th century mainly concentrated on imitating Sotadean and earlier products (the so-called Epigonous Classes—the Dresden, New York–Paris, Penthesilean and Spetia Classes), but new vigour returns towards the end of the century with the Persian Class.

The general validity of the classification proposed seems assured, resting as it does partly on the evidence of the moulded parts, partly on relationships already established between the decorators of the vases. Further confirmation may come in due course with closer analysis of the more purely ceramic shapes of the potters and painters in question. If the reviewer feels obliged to disagree on points of detail, it must be stressed that his divergent views are based almost entirely on the clear and full statement provided by the author.

First of all, it is not always clear just what the relationship was between the modellers creating the prototypes for the plastic elements of such vases and the potters' workshops concerned with their subsequent mass-production and this is particularly the case in those workshops showing no consistent pattern of inventiveness in the field. The matter is further complicated by the widespread plagiarism existing in ancient terracotta-work, whereby new moulds were taken from the finished products of other workshops (cf. *BSA* xlvi 217 f.). It would thus appear that the classification by potters' workshops as given here may exist on a rather different plane from an arrangement of the material exclusively in terms of its moulded elements. On the whole the author seems to have realised this, but occasionally he treats the two systems of classification as identical, with curious results. Thus, in assigning nos. 59, 53 and 54 to the Sotadean Class, he presumably does not mean to imply that Sotades continued active until the end of the 5th century, but simply that these pieces were produced from moulds derived from originals of the Sotadean Class. The three vases in question were more probably actually made in the workshop of the Persian Class. Likewise, in placing no. 14 in the Brygan Class he doubtless does not intend to make Brygos coeval with the Eretria Painter but to indicate that the Toronto donkey-head has been copied mechanically from a Brygan example. The Toronto vase would itself seem more at home in his Spetia Class. Or again, as the Dourian Class does not seem above employing derivative moulds in other instances, it does not necessarily follow that the Dourian donkey-head of Brygan derivation (no. 120) should be excluded from that Class. In the predominantly derivative Epigonous Classes, also, one may hesitate, at least on the basis of the published photographs, to separate the two vases decorated by the Orleans Painter (nos. 68, 99 bis).

Given that the two systems of classification may not always be compatible, there is much to be said for also having a short table at the back of the book setting forth the mechanical relationships existing between the moulded parts of the different vases. Much of the evidence for this is already given by the author, but much more is left to be inferred—a procedure that is never safe where one does not work from a first-hand acquaintance with the object itself. Might one express the hope that such a table covering both the Attic and the Apulian examples will be provided in his forthcoming study of the Apulian red-figure rhyta—a study that promises much evidence of complicated derivative production?

But the important thing to observe is that the basis for a sound classification of the Attic red-figure animal-head vases seems to have been achieved in a book that is simply and clearly written and beautifully illustrated.

R. V. Nicholls.

Fitzwilliam Museum, Cambridge.
TUCHAR (K.) Tiergefäss in Kopf- und Proto-
mengestand. Untersuchungen zur Formen-
geschichte tierförmiger Giessgefäss [Is-
tanbuler Forschungen hrsg. von der Abteilung
Istanbul des Deutschen Archäologischen Insti-
P. 156, with 28 plates, 3 text figures and 5
maps. DM 58.

This book stems partly from the author’s earlier
thesis on Greek animal-protome rhyta presented in
1955, partly from his subsequent researches in
Turkey on the Middle Eastern ancestry of such
vessels. The field covered also includes both the
erlier rhyta in the shape of a complete animal
and the animal-head cups, but at the same time imposes
certain rather arbitrary limitations on the Greek
material dealt with. In part, at least, these have
become endemic to the subject as its treatment has
been developed from Panofka’s Trinkhœrner through
Buschor’s Krokokol des Sotades to Swoboda’s article in
Neue Denkmäler antiker Toreutik and Hoffmann’s in
Antike Kunst iv.

The animal-shaped vessels of the ancient Middle
East from the 4th to the 2nd millennia B.C. are broadly
and ably discussed with the aid of a series of dis-
tribution maps and their predominantly ritual
significance emerges. The author’s total neglect of
the lovely dynastic Egyptian bottles and containers
in animal form is understandable in terms of their
prophane function and the circumstance that they were
clearly not rhyta, but one may boggle at his proposal
to redact the Amlash zebu-vases, commonly assigned
to the 8th century B.C., in the late 3rd or early
2nd millennia B.C. Contact with Europe emerges
with the Cretan bull-rhyta of E.M. III–M.M. I date
which the author very persuasively links with their
Syrian counterparts and the main Middle Eastern
tradition, pointing out that they appear as a foreign-
inspired idea, the normal Aegean type being the
quadrupeled askos. The M.M. III–L.M. I
animal-head rhyta are related to the similar Imperial
Hittite vessels better known epigraphically than from
surviving examples, apart from their Kültepe
predecessors, whilst the Mycenaean bull’s-head
rhyta are shown to have evolved from these Minoan
ancestors.

From this point on, in narrowing his sights exclusively
to the animal-head rhyta and drinking vessels, the author loses contact with Greek material
until the 6th century B.C. But it is necessary to
realise what happened in Greece in the interval in
order to reassess certain of his conclusions. Although
not yet attested from the M.M. II–III periods, the
Minoan bull-rhyta in the form of a complete animal
reappear unchanged in L.M. I times and are there-
after represented down to the end of the L.M. III
B.1 phase. At this point they are completely re-
placed by the much more numerous wheelmade
votive bull-statuettes which are identical in style
and technique, but totally different in function.
Exactly the same change occurs in contemporary
Cyprus, and in Greece, where the whole-bull rhyta
seem to have been much rarer, the statuettes of this
kind are abundant from the beginning of the L.H.
III C.1 period. It is not known whether this change
was influenced from Anatolia, where one may point
to the strange ambivalence between animal-rhyton
and animal-statue in the Hittite records (cf. the
author’s discussion of biibûd, 49 f.). Although askoi
have a checkered survival, the animal-rhyton, with
a couple of Cypriot exceptions, seems to be dead in
this and the succeeding Protogeometric and Ge-
metric periods. In the centres where we now have
adequate evidence (e.g. Crete, Cyprus, Athens and
Samos) the wheelmade animal-statue holds sway
and it seems to be from these that ultimately stem,
apparently as profane novelties rather than ritual
vases, such things as the 7th-century wheelmade
ram-rhyta in Rhodes and Boeotia. The 6th-century
Boeotian animal-cups (94 f.) are to be seen more as
a late development from this tradition than as due
to any obscure connection with inner Anatolia.
(The 5th-century Pegasos-horn, ibid. no. 9, does
not belong here and probably does show East Greek
influence; on no. 1 cf. J. Boardman, Cretan Collec-
tion in Oxford 105 f.).

In his treatment of the Iron Age animal-head
vases the author gives a strangely negative view of
the archaic Greek contribution by excluding the
scent bottles in this form on the grounds that they
are not rhyta. In so doing he shirks one of the most
baffling problems of all, to be found in the back-
ground to the Rhodian bottles in the shape of the
heads of animals, both local and wildly exotic—a
problem whose solution, one suspects, may not be as
wholly irrelevant to his theme as he imagines.
A link is postulated between the Late Bronze Age
animal-head faience cups of the ‘Enkomii Class’
(55 f.) and the north-west Iranian and north Meso-
opotamian animal-head vessels of the later 8th–5th
centuries B.C. (57 f.). The catalogue of the latter
is somewhat vitiated by its failure to distinguish
between situlae and cups and by its incompleteness.
Thus, from the recent Paris exhibition, one might add
7,000 Ans d’Art en Iran pls. 17, 18, 34, 35, 59, 60.
Nos. 15 (now Cincinnati Art Museum)—17 are in
styles quite foreign to 5th-century Achaemenid art
and have been regarded as Sassanian. The truth
probably lies between the two extremes. The
relationship long claimed between the Achaemenid,
etc., animal-head cups and the Attic red-figure
drinking vessels of this form is incontestable, with a
6th-century intermediate East Greek stage repres-
ented by Maximova, Vases Plastiques pl. 27 no. 103.
The Phrygian animal-head rhyta from Amisos, etc.
(64 f.) have rightly been rescued from the Hellenistic
date to which they had been assigned in E. Akurgal’s
Phrygische Kunst; but it is doubtful if they can really
also be regarded as intermediaries in this evolution.
Their handles, etc., suggest the influence of the Attic
vases and more probably they do not begin until
the 5th century B.C. But the role of the Attic head-
vases, particularly the face-kantharoi, as a formative influence has, on the other hand, been rather overlooked. These probably evolved slightly earlier, since it is difficult if the black-figure donkey-kantharos in the British Museum can be placed much before the late 6th century (certainly not in the 340's B.C. as proposed by the author) and the red-figure animal-head vessels can hardly go back beyond the early 5th century (not to 520 B.C. as here claimed). The list of these Attic vessels (134 f.) is already completely superseded by that in H. Hoffmann's Attic Red-figured Vases, reviewed elsewhere in this issue.

The rest of the study is devoted to the rhyta terminating in the forepart of an animal. The Achaemenid vessels of this kind are listed (83 f—add 7,000 Ans d'Art en Iran pls. 57, 66) and their predecessors discussed. The distinction between these as rhyta and the related Scythian vessels as animal-protoyme drinking-horns (97 f.) is a little strained in that certain of the earlier Achaemenid 'rhyta' are without a lower outlet, whereas some of the horn-shaped vessels seem to have one. Certain of these last from Scythian graves seem purely Greek in style—possibly originating in East Greece or the Black Sea colonies. A terracotta animal-fronted drinking-horn from Old Smyrna and the scenes on a terracotta frieze from Buruncuk—'Larisa' (here dated too early) attest the presence of such vessels in the Acolis and Ionia in the 6th century B.C., in the period following the Persian conquest of 546 B.C. The horn-shaped animal-vessels from Amisos, etc. (90 f.) look to the reviewer as if they may begin in much the same period. Two Asia Minor monuments may, however, anticipate the actual arrival of the Persians—a crudely painted scene possibly showing a drinking-horn on a Late Phrygian sherd from Pazarli and a lion-fronted vessel of this kind in Berlin (pl. 20). But the latter seems a crude, rather provincial East Greek work, probably much later than it looks. Both the East Greek vessels just discussed and the Achaemenid rhyta seem to have had a faint, indirect impact on Greece proper in the 5th century B.C. as witness, e.g., the Boeotian Pegaso-horn mentioned above and the concept of the Sotadean sphinx-rython from the Brygos Tomb. The plain Scythian drinking-horn had, of course, long been associated with satyrs and Dionysos. But it was not till towards the end of the 5th century that the animal-protoyme rhyton of oriental inspiration was really adopted in Attica, thereafter to flourish as a ritual vessel, the peculiar adjunct of the 'heroized' dead and, later, of the Roman lares.

The Italiote vessels of this kind are dealt with separately (120 f.; cf. also 72). An Etruscan bucchero version of an East Greek animal-headed horn is attested as early as the 6th century B.C., but the bulk of the vessels under consideration are the uninspiring 4th-century-B.C. Apulian imitations of the Attic animal-head cups, with the neck of the vessel set at varying angles to the creature's head. The author follows Swoboda in proposing that those showing a markedly angled neck are non-functional imitations of rhyta, which seems a somewhat extreme conclusion. Both the 'cup-shape' and the 'rhyton-shape' may have either a kantharos lip or a profiled lip intended for pouring. Comparatively few have their tips pierced as rhyta. Presumably they were mostly used either as cups or as oinochoai. On the basis of a faint typological resemblance to the above, the author proposes to assign a number of late classical and early Hellenistic gold and silver rhyta to this area, probably to Taras (129 f.). That southern Italy bulked in the production of such vessels is suggested by certain other terracotta rhyta of distinctively metallic form. But the Panagyurishte vessels (nos. 5–8) are in a curious Hellenized style with Achaemenid survivals (e.g. in handle forms) that would be impossible in Magna Graecia (cf. Antike Kunst i 56). More likely centres are north-west Asia Minor and south-east Thrace, as suggested, e.g., by the animal-protoyme vessels shown on coins of Skepsi and Ainos. No. 4, also, probably did not have far to travel to reach Thrace. In addition to these areas and possibly Taras, Egypt clearly ranked as production-centre, as revealed by scenes in the Tomb of Petosiris.

The terms, κέρας, μυτών, and προτομή, are usefully distinguished (114 f.), but such vexed questions as the significance of ὃνος in this context are not ventured upon. Nor is any hypothesis advanced as to the exact ritual significance of the rhyton. Why should the hero have the bother of filling the animal-rython from a jug in order to use it to fill the phiale or cup from which he was to pour his libation? Was there some sacramental change from wine to victim's blood?

The half-tone plates of the volume are as diverse as its text. As is only to be expected in a work that ranges so far there is a certain unevenness of quality, but also much that is new and positive is contributed to a bewildering variety of fields.

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The nine gold vessels from Panagyurishté have been published several times since their discovery in 1949. It is so difficult to study ancient metal-work away from the originals that one can hardly have too many different views of an object, especially large pictures and details. The present publication is generous, giving us 39 plates (11 in colour) and 9 small pictures in the text which serve as aide-mémoire; in addition there are facsimile drawings of the inscriptions.

The text is sensible. The introduction sets out
many of the questions which spring to the mind: when and where were the vessels made; how were they accumulated; who owned them; when were they hidden, and why; what is their significance for the history of Thrace? As the treasure alone can provide the answers, a concise description of each piece is given. In this section an iconographical study might have helped the subsequent discussion of the date and origin; and a more detailed technical account would be welcome (were the reliefs really 'beaten out on a mould', and not worked in straightforward repoussé technique; and were all the pieces worked in the same way?).

In the general discussion, the author begins by stressing that we are dealing with a collection of rhyta. The forms are common in Greek pottery from the sixth century onward; four vessels ending in the head or forefront of an animal, with a high, decorated mouth, three with the body shaped like a human head; one amphora; one phiale. The phiale is a decorated shallow bowl; the amphora was intended from the start to have two perforations in the bottom, the other vessels, one each. The author regards the rhyta, at least, as drinking vessels, and points to Asiatic parallels, observing that P. Ambrani has shown that the form of the amphora has much in common with an Achaemenid example (Antike Kunst II (1959) 48). The argument is less convincing for the 7 other rhyta; the first Greek pottery protome-vides are indeed copied from the Near East, but by the fifth century (let alone the fourth) the form was firmly acclimatized in Greek lands; and although we have Greek rhyta with perforations at the snout of the protome, and some illustrations of people drinking from them, most of the vases are unperforated, and can only have been used by drinking or pouring from the upper part. Again, many Greek clay vases with the body shaped like a human head normally have a jug-mouth at the top and no perforation at the bottom. It looks, then, as if the Greek versions served three different purposes: for libations (perforated); for drinking (perforated and unperforated rhyta); and for serving (jugs). Yet to judge by the perforations 8 of the 9 vessels from Panagurishté were designed for the same purpose, despite differences of form. Is it not possible that the position is more complex than has been supposed? The subjects represented in relief are purely Greek, and the style either provincial Greek or Hellenizing; but such a collection of rhyta has an un-Greek look. Perhaps the head vases and protomes were made for a non-Greek owner in a format which, originating in the Near East, had been assimilated by the Greek world, and was now taken back again by the non-Greeks and modified for their own purposes.

The author goes on to argue that in the figured scenes the people are clothed, and that this preference for the clothed figure is Persian, not Greek. Certainly the mythological figures are draped in the Judgement of Paris, Dionysos with Eriope, and the assembly of Hera, Apollo, Artemis and Nike, but one need look no further than the East friezes of the Parthenon and Hephaisteion to see that there is no compulsive convention of nudity in Greek art for this kind of static divine group. Moreover, Herakles and Theseus are nude on one rhyton, and so are the heroes on the amphora; the cloaks which spread out to form a background or cover part of the body are also well at home in the Greek tradition from the fifth century onward; again, the metopes and the frieze of the Parthenon provide a parallel. Nor are the representation of decorated clothing and the interest in non-Greek types, or the study of animals, exclusively the product of that rebending of the Greek and Oriental world which we call Hellenistic (e.g., the François vase; works by the Medias painter; 'Kerch' vases; fourth century South Italian; Exekias, Caeretan hydriae, many Attic vases of the fifth century; head vases for over 150 years from the later sixth century onward).

The argument from the letter-forms used for the names of the figures is much more promising; but here again, one could have wished that much more comparative material had been produced. There is an ingenious attempt to wring the last drop out of the inscriptions which give the weights of the phiale and the amphora. The phiale weighs 845.7 grammes, and has two incised records of its weight both in acrophonic numerals (100), and alphabetic (196 drachmas 4 obols). The amphora has an alphabetic numeral inscription, which gives its weight as 200 plus an unidentified fraction; its weight today is 1695.25 grammes, so obviously the weight was expressed in Daries. The author rightly observes that the phiale was originally made on a Dicic standard (the round number, 100, cannot be coincidence) and then recorded in Attic drachmae, but he goes on to argue that the Dicic proper weighed ca. 8.40 grammes, and that only Lampiasii coins on a standard of ca. 8.44. It is very unfortunate that he did not see Miss Simon's excellent and provocative article in Antike Kunst III (which incidentally settles the problem of the subject represented on the amphora-rhyton) with its appendix by Dr H. A. Cahn, where the weight of the Dicic is given as 8.45 grammes; if this is correct (and it has not been challenged) the phiale can have been made, and the amphora weighed, in any place which used Daries as a standard; the field is wide.

The author then tries to establish the sequence in which the collection was acquired. The three rhyta shaped like women's heads form a group (but the fact that one is certainly an Amazon does not mean that the others must be; contrast is no less potent in art than uniformity); it is argued that they go closely with the amphora. The use of a purely alphabetic indication of weight on the amphora is taken as showing that it must be later than the phiale, whose weight was first recorded on the acrophonic system; and it is then stated that the phiale cannot have been the earliest piece in the set-
it must have been acquired to serve as a salver for the rhyta. There is a difficulty here; the English throughout is extremely good, except for a few Gallicisms, but here the meaning is important. Are we to take 'salver' literally, and suppose that the rhyta stood upon it? The suggestion is surely impossible, for the Negroses' heads stand up in high relief inside the phiale; it would take a good deal of skill and steadiness of hand to use it as a tray. If the idea is that it was not a tray, but merely used with the rhyta, it loses its force; the evidence of Attic vase-painting—to look no further afield—proves that a phiale could be used on its own, without a whole set of associated vessels. Worse still, although the inscriptions which give the weight may have been put on when the amphora and phiale were made, it is equally possible that they were not; one piece might have its weight written on it when made, and its acquisition could inspire the owner to weigh, and inscribe, an heirloom, and the earlier inscription might be on the later piece.

At the outset, the author points out that there are no substantial remains of any ancient building near the place where the treasure was found, and that it does not come from a tomb; so it presumably was brought from elsewhere. True enough; but his thinking, and that of other writers, seems dominated by the idea that it must have been accumulated by one person, or at least, by one family. There are, surely, other possible explanations. As has been observed, a group of eight rhyta and a phiale has a ritual look; it might be that the treasure represents not family thrift, but a commander's share of the loot from one of the rich shrines of Asia; the style makes the lower limit for the date seem certain—late fourth or early third century B.C.—but there were many men in Alexander's expedition, and in the armies of the successors, who had the power, and the opportunity, to acquire loot on this scale. With the author, we are forced back to a study of the treasure itself to find answers to the questions which it raises. They are still beyond our grasp, but a publication of this kind, with its good pictures, takes us several steps forward.

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The grave-statue of Aristodikos was exhumed in 1944 from a field in the district of Olympos in S.W. Attica. Mr Liapis of Keratea, then owner of the field, presented his discovery—an over-lifesize kouros of Parian marble, complete with its inscribed base—to the National Museum, where the Director, though unable himself to undertake a comprehensive publication of it in the difficult years immediately after the war, had it reassembled and placed on show as soon as was possible, and generously allowed other scholars to examine and describe it before its own preliminary publication in Atlantis 1955, 44 ff. Now comes his definitive study, which, as the sub-title shows, not only analyses the statue itself, but considers also the main lines of development which can be traced in Attic sculpture between c. 550 and 480 B.C., and the significance of its grave-statues against the general background of early Greek funeral οἰκομένες.

To base so extended a study upon a single statue has in this case been fully justified. Firstly, the Aristodikos kouros is, by common consent, a masterpiece. It well deserves full treatment by so understanding a scholar as Dr Karusos. Secondly, it marks the end of an epoch in Athenian burial customs. Thirdly, it is by far our best illustration as yet of the technical transition from the late archaic kouros-type to the wholly different conception embodied best in the Kritian boy. (The views in Plates 1–4 are chosen with excellent judgement to illustrate this: the unrealistic archaic stride has now achieved an illusion of reality, of a slow walk halted momentarily between one pace and the next.) The resultant book is a most perceptive and stimulating contribution to the study of ancient Greek art and thought—as, indeed, anyone familiar with Dr Karusos' earlier writings will expect.

In ch. 1 he discusses the statue itself, concluding that it stood originally higher, on a base of two or more steps. An appendix by Dr K. Rhomaios reconstructs convincingly how the sculptor plotted his work on the block, in minute detail, based on a foot measuring 16 daktyloi (= 28 cms).

Ch. 2 collects such prosopographic and other detail as may help to reconstruct the social and political background of Aristodikos' family.

In chs. 3–4 K. seeks to place the statue in its stylistic context, setting it c. 500 B.C., and backing his argument with a chronological catalogue of select items of Attic sculpture between c. 550 and 480. (The catalogue is divided into decades, but K. stresses that this is for convenience only, with no intention of rigidity.) The selection is chosen to illustrate not only the earlier sculptors' works from which the statue's stylistic forerunners may have come, but also certain other contemporary workshops from whose influence it could not be derived. K.'s conclusion is that, while the master of Aristodikos' kouros must remain nameless as yet, his work stems from a workshop connected with the pedimental group of the Gigantomachy; an earlier work of his may be found in the kore Akrop. 673, and a late one, possibly, in the Euthydikos kore. Among other sculptors considered, with their antecedents and descendants, are the anonymous masters of the Volomandra kouroi and the Rayet head, Aristokles, and Antenor. The wide knowledge and intensive research on which these conclusions rest are revealed in the densely-packed footnotes, which provide an invaluable bibliography and general commentary on the period under discussion.
Naturally, not all readers will accept all K.'s ascriptions. Technical details like the rendering of ears or feet are reasonably objective, even if they cannot securely establish more than the fact that two sculptures are roughly contemporary products of a single workshop; but over the far more subjective matter of the particular spirit informing a piece of sculpture no one expects universal agreement. Occasionally one may wish that K. had given in more detail the reasons for a connexion: e.g. for those made between the Volomandra kouros and the kore head Akrop. 696, or between the Rayet head and the New York head MMCat. Sculpt. no. 2. In his suggested chronology the boxer stele is first item in the decade c. 550-40, and the Kroisos kouros fourth in that c. 530-20: both a little later, that is, than some might accept. On the other hand, for the Athenian Treasury metopes he prefers the pre-Marathon date (though with an added note referring his reader to FD iv, 4, upon its publication), and for the Kritian boy the pre-480 date. His final list in this catalogue contains further interesting suggestions on the origins of certain archeic pieces in the Attic manner; the section is headed 'Dubia et spuria', but the more ominous adjective seems to go no further than the heading.

Ch. 5 contains, as well as a useful discussion of earlier views, his own carefully-reasoned conclusions as to the significance of the archaic Greek grave-statue: what its purpose was originally, and what it meant to the Athenians of the sixth century; this latter discussion includes the question of embryonic portraiture in funeral statues and reliefs, and here too his observations are both just and illuminating. Catalogue II gives a list of certain or probable Attic grave-statues, and the surviving bases (see also BSA 1962, 115 ff., and add now the kore head from Merenda, BCH 1962, p. 664, fig. 28, the kouros body ADelt 1960, pl. 26a, and the five new bases from the Kerameikos). An appendix discusses the treatment of the Schamhaar as an aid to dating.

Admirably few slips or misprints were noticed: the base C5 is for a kouros, not a lion; nn. 19, 31 and 33 misprint personal names; on p. 54 F2, '58' should read '56'.

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Among Anton Hekler's many important contributions to archaeology those on Greek portraiture are perhaps the most noteworthy. In 1912 was published his Bildniskunst der Griechen und Römer—still in constant use—and in 1940, just after his death, his Bildnisse berühmter Griechen. The latter consisted of only 51 pages (42 of text, the rest in notes) and about 50 illustrations—a seemingly slight work, but so seasoned in its judgements and with most of the recent discoveries so conveniently listed in the notes, that it was soon recognised as a fundamental work. It soon went into a second edition (actually a reprint), and now appears in a third edition, revised and enlarged by Helga von Heintze. Hekler's introduction (48 pages) is reprinted as it was, but the notes now supply not only recent discoveries, but lists—more or less complete—of the extant examples of each type treated by Hekler. Moreover, an up-to-date short bibliography is added, as well as new, excellent illustrations, somewhat larger than before, for the format of the book has been changed.

Much has happened in the field of Greek portraiture in the twenty-odd years since Hekler's death. There have been new discoveries of 'certified' portraits, for instance of Miltiades, of Xenophon, of Hippokrates, and above all of Themistokles; a few new reconstructions of portrait statues have been successfully made or attempted (e.g. of Demosthenes, Chrysippus, Plato, Sokrates); and our understanding of the evolution of Greek portraiture has become more precise. It was not easy to embody this new knowledge in notes, leaving the text unchanged; but H. v. Heintze has admirably acquitted herself of the task. Though Hekler's opinions regarding individual examples are cited also in the notes, they are often corrected in the light of recent discoveries; and the lists with up-to-date references will be welcomed by all archaeologists working in the field. They form a necessary adjunct to Bernoulli's and Scheffold's lists. Here and there are inserted personal opinions, sometimes regarding the antiquity of a piece, generally, however, with a question mark, that is, wisely put forward as mere suggestions.

The illustrations have been chosen with great care. First come, in chronological order, a series of known statue portraits, beginning with Harmodios and Aristogeiton, through Anakreon, Plato, Sokrates, Sophokles, Aischines, Demosthenes, Poseidippus, Metrodoros, Diogenes, and Chrysippus; then come, again in chronological order, the herms and busts and heads, beginning with Themistokles and Miltiades and the Homer of the Epimenides type, through Perikles, Lysias, Sokrates, Thucydides, Herodotus, Aristotles, Plato, and the three tragedians, down to Hellenistic times, with a few Hellenistic rulers included among the poets and philosophers, and with the blind Homer coming as a grand finale. In essentials the sequence is the same as Hekler's, but the fifth-century showing has been amplified, better or less well known views of extant examples have been given, and occasionally the whole herm or bust has been included (in cases where it is ancient), or the full profile instead of the three-quarter view has been added—all important improvements. The book, in other words has become a sine qua non for students of Greek portraiture.

Since Greek portraits are apt to be wanderers until they safely land in some Museum, I can add a few corrections to von Heintze's lists of locations. The
Menander (p. 66, no. 44), once in Boston as a loan of Mrs. Brandegee, is now in the Collection of Dumbarton Oaks, Washington, D.C. (cf. my catalogue of that collection, no. 4). The Menander supposedly in O. Deubner’s collection (p. 65, no. 31) was never actually acquired by him (so he has informed me), and its present location is not known. The portrait once in Washington (p. 58, no. 7) is now in Princeton, and I no longer think that it represents Sophokles, but only some individual resembling him. The Aristotle listed as being in Washington (p. 62, no. 12) has never been there and its present location is not known. The Demosthenes once on loan in Washington (p. 65, no. 44) is now in Princeton. The statuette of Epikouros, found in 1874 in the Esquiline (p. 68, no. 4), is now no longer in the Antiquario Comunale and its present location is not known. It is to be hoped that information regarding some of these lost pieces may be forthcoming.

Furthermore, the identity of some of the portraits included in H. v. Heintze’s various lists seems to me doubtful—for instance, the heads of Antisthenes in the Villa Albani, in Modena, in Mantua, and in Copenhagen (p. 55, nos. 10, 12, 4, 6); the Thucydides in the Louvre and in Corfu (p. 55, nos. 5, 8); the Aischylos from Fianello Sabine (p. 61, no. 9); the Crisippos in Athens (p. 70, no. 15); and of course I personally exclude the ‘Euripides’ of the Rieti type (cf. my Greek Portraits II, pp. 31 f.). Admittedly, however, the opinions on these various identifications are not yet unanimous.

H. von Heintze lists as of doubtful antiquity a number of portraits heretofore accepted as genuine; e.g. the Demosthenes in Geneva (p. 65), and the Demosthenes in Northwick Park (p. 64, no. 31). Perhaps they should be carefully re-examined.

Furthermore, the head of the little relief in the Cabinet des Médailles, supposedly a Sophokles of ‘type IV’, is thought to be alien or modern (p. 59). When I examined the piece, outside its case, in the summer of 1961 and again in the autumn of 1963, I found that the weathering on the back is the same on head and body, with similar, and what seemed to me to be ancient, rootmarks on both. So, in my opinion, the head was broken off and reattached, with a small piece missing, but is ancient and belongs.

It will be seen from these few remarks what a constantly changing scene the study of Greek portraits presents, and how beset it is with intricate problems. H. von Heintze’s contribution is both informative and challenging.

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GISELA M. A. RICHTER.


Much publicity has been given to the revelations of middle and late Byzantine art that have taken place in the Balkans in the last twenty or thirty years; but the discovery and excavation of earlier Byzantine churches in Macedonia dating from the period before the Slav invasions have been less advertised at least in western Europe. Mr Hoddinott’s lavish and remarkable book was originally planned as a general study of Macedonian and Serbian art and architecture in the middle ages, but the wealth of new material constantly coming to light (e.g. at Philippoi and Carinč Grad) persuaded him to narrow his sights to the formative era of the fourth to the seventh centuries.

The churches of Macedonia and Southern Serbia, the homeland of Constantine himself, lay reasonably close to Constantinople, but their architects and the faithful who worshipped in them were the heirs of many pre-Christian traditions and open to many foreign influences. Hoddinott conducts a thorough investigation of the problems that must have beset St Paul when he came to Macedonia, exploring every aspect of the religious background of the Philippians and Thessalonians and their neighbours. The scope of the investigation is wide, some may feel too wide, for the author takes samples for his soil analysis of the roots of Macedonian Christianity from the whole of the eastern Mediterranean and beyond. He ranges from the syncretism of the Hellenistic world to the influence of Buddhism on Christian spirituality and monasticism (which he seems to take for granted). (The Greek version of the Indian romance of Barlaam and Josaphat should surely now be ascribed to the hand of John of Damascus in the eighth century and not to an anonymous ‘John the Monk’, but its significance as a purveyor of oriental philosophy should not be overstated; while the supposed affinity between the figure of Christ in the apse mosaic of the church of Hosios David in Thessalonica (here miraculously reproduced in colour plates) and stock figures of the Buddha may seem rather to be in the eye of the beholder than to be a fact susceptible of proof.) St Demetrius of Thessalonica and St George of Cappadocia are thought, plausibly enough, to be metamorphoses of the Thracian Horseman seated crowned upon the grave thereof; and there is a lengthy digression on the Horseman’s ‘feminine hypostasis’ Bindis. The Lion Gate at Mycenae and other similar representations are adduced as evidence of a stubborn tendency among the Indo-European peoples to employ tripartite symbols to illustrate the facts of rebirth and eternal life, a tendency which finds its expression in Christian architecture in the tripartite sanctuary or the three-naved basilica. ‘The lions that had accompanied Lilith, Cybele and other Great Goddesses still flanked a fourteenth-century west European Madonna, and they still crouch fiercely at the feet of the nineteenth-century episcopal throne in the church of St Clement at Ohrid in Macedonia’ (p. 22). One may wonder whether in fact they are the same lions serving the same symbolic purpose in each case.
The theory of the persistence of ideas and of their expression in art is always enticing, but it may sometimes blind its proponents to the possibility of independent invention or mere coincidence. It is hard to believe, e.g., that the sun (or wheel?) motifs worked in brick on the walls of two of the churches in Kasia (also to be seen in the thirteenth-century church of St Theodora at Arta) are really sun-symbols eloquent of the lingering influence of Mithraism in Macedonia into the middle ages; or that ivy leaves decorating a fourth-century marble slab at Philippi indicate a local nostalgia for Bendis. One may also be cautious about accepting the admittedly tentative hypothesis that the palm branches flanking the cross on the north-east aper of the recently excavated Palace Octagon at Thessalonica are likely to be crypto-Mithraic fertility symbols, and the theory that the sixth-century scenes of the lion and bear hunts in the floor mosaics of the South Church at Caricin Grad represent the Christian triumph over Mithras and the Slav Lord of the Woods. It is perhaps wishful thinking to suppose on such meager and debatable evidence that the Slavs, had they been better appreciated by the Byzantine Emperors, would have graduated from paganism to Christianity and settled down peaceably. Many of these suggestions, however, give new food for thought about the influence of pagan beliefs on Christian art and ritual in this vital area of the early Church. For Macedonia's geographical position, 'always at or near the fulcrum of the balance' between East and West, gave it a unique importance in the formation of that synthesis between Greek, Roman and Oriental which characterises Byzantine art and civilisation. This point is worth making, and it is well if somewhat elaborately made by Hodkinott.

The description of the individual churches, which is the gist and raison d'etre of the book, is admirable. The numerous illustrations are for the most part excellent and the architectural plans and drawings, mainly reproduced from the existing publications of the various buildings scattered in many books and periodicals, are clear and plentiful. The author's main thesis is the development of early Byzantine church architecture as the expression of a form of ritual progressively more sophisticated; and he clearly demonstrates the evidence presented by the early Macedonian churches for a liturgical development from a simple and public service to a more exclusive and complicated ceremony enacted in the central portion of a tripartite sanctuary hidden from the congregation by a screen. The importance of Thessalonica first as the cultural centre of Illyricum, poised between Rome and Constantinople, and then as the sole repository of the Byzantine artistic tradition in Macedonia through the period of the Slav invasions and afterwards through the bleak years of Iconoclasm is rightly emphasised; and the full description of its pre-seventh-century churches is especially welcome since no account on such a comprehensive scale is otherwise available. Indeed much of the great value of this book lies in the fact that it renders accessible to readers who may have little Modern Greek and still less Serbo-Croat the published but not easily available results of the researches into their own monuments of eminent Greek and Slav experts. It is to be hoped that the author will put us further in his debt by undertaking to produce a similar study of the Macedonian and Serbian churches of the middle and late Byzantine era.

There are hardly any misprints (except in some of the Plates: the word 'archbishop', e.g., occurs three times on Pl. 49), still fewer obvious lapses. One of the inscriptions on p. 150, from the church of St Demetrius in Thessalonica, might have been printed in the iambic lines in which it reads; the Prefect Leonius of Thessalonica is relegated to the fourth century on p. 151; and it seems unfair that the author, who has evidently laboured to make a finished product of his work, should have allowed himself to state, on the very last page of his text, that the Byzantine Empire came to an end 'in the sixteenth century'. But on the whole both author and publisher are to be congratulated on the extremely high quality of the production.

D. M. NICOL.

University College, Dublin.


Miss Richter's catalogue of Greek and Roman material at Dumbarton Oaks appeared in 1956. The present volume, in the same generous and familiar format, is the first of a series of successors planned to cover the Byzantine. It contains 131 items arranged in five main groups, as indicated in the title. Nos. 1-27 are silver, 28-84 bronze, 85-90 iron, lead, and copper, 91-93 clay and terracotta, 94-103 glass, 104-109 glass paste, 110-123 cameos and intaglios, 124-125 mosaics, and 126-131 paintings. A sequel by the same author on jewellery and enamels is forthcoming, and further volumes will deal with the ivories, stone sculpture, textiles, coins, and (presumably) seals.

A third century enamelled bronze lamp (28) is the earliest, a seventeenth-century icon (191) the latest object in the book. The lamp, said to have been found at Kalat Seman, is probably of Gaulish manufacture, and another icon (190) is Russian. A silver phalera (18) with Byzantine control-stamps is Sassanian, and two small bronzes (36, 57) are perhaps Seljuk. Apart from these and a few Italian pieces the bulk of the collection comes from pre-Islamic Egypt and Syria, and from Constantinople. The array of Constantinopolitan work, more than 50 items, is impressive, beginning with fifth-century
NOTICES OF BOOKS

silver and bronze and culminating in the two
miniature mosaics (124, 125) of Palaeologan date.

While many of the attributions were briefly anticipated in the D.O.C. Handbook (1955), only a few
objects could there be illustrated. There is, moreover,
new material here, acquired between 1955 and 1960. Special mention may be made of an
exquisite cameo with the profile head of Constantine
the Great wearing a lion-skin (110), an inscribed
iron battle-axe of the fourth century (85), a fifth
century Syrian chalice (5) with an inscription of
Aradaburius (the Aradaburius who was Marcian's
magister militum per Orientem?), and a small but
distinguished terracotta eagle (93). This is an
exemplary publication of important material; the
apparatus is clear and comprehensive, the
discussions authoritative and illuminating, and the
plates uniformly excellent.

R. M. HARRISON.

University of Newcastle upon Tyne.

Sylloge nummorum Graecorum. Vol. v. Ash-
molean Museum, part i (A): Italy, Etruria—
Lucania (Thurium). London: Oxford
University Press for the British Academy. 1962.
PP. 40. 16 plates. £4.45.
The predecessor of the present fascicle, Sylloge
Nummorum Graecorum V, part i (see JHS 1952,
157-8), recorded the Greek coins of Italy bequeathed
by Sir Arthur Evans to the Ashmolean: it was the first
of a series of parts intended to catalogue the whole of
his collection. Among a number of rare coins were a
then unique 'Tarento-Campanian' didrachm inscribed
BPET and three gold issues of the fourth
century B.C. from Metapontum. The reviewer,
however, noted gaps in several series (e.g. Poseidonian
incuse coins and gold and silver issues of the Brettii),
and drew attention to inadequacies in the record of
legends and in descriptions.
The coins derive from four main sources—the
Bodleian, Ashmolean and College collections, and
gifts or bequests; in 1921 they were combined to form
a single Greek series but the mints were still unevenly
represented. Since then, however, there has been a
systematic attempt to build a more uniformly repre-
sentative collection. Through the generosity of
private collectors, and by purchases, no less than 750
silver coins relating to the area covered by the present
fascicle, have been added. These, and similarly
large accessions since the original publication, have
stimulated a change of plan. Vol. V, part i (A) is the
first of a projected series which will integrate, in
Sylloge form, all gold, electrum and silver coins from
the Ashmolean and a few bronze issues which are
hitherto unpublished varieties, or are not suitably
illustrated elsewhere. Coins, however, of the Roman
Imperial period in all metals are excluded. Some
overlap with part i is inevitable but the present plan
ensures that the original volume is not completely
superseded although it will mainly be of value for its
illustration of bronze issues.

Dr C. M. KRAAY is responsible for the text, which,
in a concise form, provides a wealth of information;
a particularly valuable feature is the detailed record
deries-identities wherever these are known. To the
list may be added 906, the reverse die of which is
shared by Schwabacher, Grekiska mynt ur Konung
Gustaf VI Adolfs samling, 41.
The area covered by this fascicle is from Etruria-
Lucania (Thurium). The number of coins recorded,
1,032, there were only 304 in the earlier Sylloge—
affords a good indication of the importance of the
decision to integrate all the coins. Not surprisingly
the best represented mints are Tarentum (404 coins)
and Metapontum (167); there are also good series
from Neapolis, Herakleia, Poseidonia and Thurium:
most of the gaps noted by the reviewer of part i are
now filled (e.g. 815-817 incuse coins of Poseidonia
and others). Many individual issues are especially
interesting. A rare didrachm from Fensis (56)
shares an obverse die of a facing head of Hera with an
issue from Hyria carrying an Oscar legend (74).
There is a Tarentine didrachm overstruck on a stater
of Corinth (223). The reverse type of a diobol from
Tarentum (520) illustrates Herakles wrestling with
Antaeus. A mutilated coin from Herakleia (635)
was, perhaps, clipped to make it conform to the
reduced standard or represents a transitional issue.

There are, inevitably in a work of this magnitude,
some errors in descriptions and in cross-references to
dies and types recorded in standard monographs, but
these will mainly be the concern of the specialist who
will easily correct them as necessary.
The new plates are of high quality and the whole
publication provides a welcome extension of the
scope of its predecessor.

BEDFORD COLLEGE, LONDON.

Sylloge nummorum Graecorum. [U.S.A.] The
Burton Y. Berry collection. Part 2. Megaris to
Egypt. New York: the American Numismatic
This is the second and final part of the Burton Y.
Berry collection (for part i see JHS 1963, 216-17)
recording issues from Megaris to Egypt (747-1506).
Miss Margaret Thompson and Dr Ross Holloway are
again responsible for the text.
The present volume contains a wealth of new
material and particularly well represented are issues
from Elis (800-850): these include a number of new
dies—obverse and reverse (899, 832, 849), obverses
(812, 814), reverses (813, 839) and, in addition, five
new die-combinations (818, 822, 824, 828-9)—all
unknown to Seltman, The Temple Coins of Olympia.

There are a large number of electrum coins. Many
of the Cyzicene staters and sixths are from the same
NOTICES OF BOOKS

dies as issues recorded by von Fritz in Die Elektron-prüfung von Kyzikos, and his provisional chronology for the series is followed. Among the electrum hektai from Mytilene (1004-09) the last obverse represented—a head of a youthful Zeus Ammon—is probably a complimentary ‘portrait’ of Alexander the Great (on this see Numismatic Chronicle, 1962, 65 ff.). Some further well-preserved hektai from Phocaea (1081-86), a stater from Lampsacus (952) and a group of uncertain issues from Asia Minor (1015-48) complete the electrum represented. The uncertain category presents some tantalising problems of attribution and the authors add a brief critical commentary wherever possible. The collection also includes three statera, a half stater and a third of Croesus (1137-41).

A single diobol (914), discovered at Benderigl, suggests that similar coins with an obverse type of a head of Herakles, were struck at Heraclea Pontica and not at Heraclea Sintica. The obol (1003) is almost certainly to be attributed to Lesbos but 1078-80, with less certainty, to Phocaea.

The series of Lycian dynasts is of particular interest: 1187-1189 indicate a possible sequence of rulers at the end of the dynastic period. The obverses of the triskeles staters (Nos. 1187-1188) are from the same die. A probable link between the two issues had been noted by G. K. Jenkins (Numismatic Chronicle, 1959, p. 37, n. 1) but no order of striking was suggested. Examination of the present obverse dies shows a greater degree of deterioration in 1881 which is clearly the later issue.

Six important staters from Phaselis (1201-06) not only add four new names to the list of magistrates at that city but a variety of symbols and decorative motifs: these issues are accordingly commented on in some detail.

Coins from Aspendus (1208-31), Nagidus (1276-84)—among them an unpublished stater (1281)—and issues of the Seleucid kings and Ptolemies (1459-1506) conclude the Sylloge.

The plates are again of good quality and part II admirably fulfils the promise of the earlier volume.

J. F. HEALY.

Bedford College, London.

BELLINGER (A. R.) and BELINCOURT (M. A.)

This study was based on a paper by Dr. Marjorie Alkins (Belincourt) which set out to discover the appearance of the Victory of the Roman Senate which played so conspicuous a part in the religious controversy of the fourth Christian century (1). The nature of the problem, however, and the extent of the material available, led to the present wider treatment covering the use of Victory as a type, or symbol, on coins from c. 510 B.C. to A.D. 578.

The Victory type at Olympia (pl. I, 1)—and possibly the archaic Cyzicen version on electrum (pl. I, 2)—provide the earliest evidence. Its character was influenced and established by artistic representations owing little or nothing to cult, mythology, or literature. Bellinger begins by discussing the appearance of Nike as a type, or part of one, at Greek mints during the fifth and fourth centuries B.C. Nike has initially two main roles namely (a) the symbolic bearer of victory in the games and (b) the goddess of victory in war. For the fourth century B.C. one further example of a winged Nike might have been added: a reverse, from the series of Mytilenean electrum hektai, carries in the field two stars which are clearly part of the type and not merely symbols (BMC Troas, pl. xxxiv, 25). The whole representation is paralleled on a weight of Antiochus IV (see Rostovtzeff, Social and Economic History, pl. liv, i).

The coinage of Alexander the Great opened a new epoch (see further Bellinger, Essays on the Coinage of Alexander the Great, 6 ff.): 'The Nike...embodies a new and abstract idea, for she has now become one of Alexander's attributes or possessions and belongs to him, presented not in commemoration of a particular victory, but to signify the career of conquest that he set out for himself' (21). Bellinger continues with a description of variations of the type under the Ptolemies in respect of its appearance, character and rôle (23 ff.): its influence becomes widespread. One of the most interesting is, perhaps, the Nike employed by Agathocles of Syracuse to commemorate his victory over the Carthaginians in Africa. By 278 B.C., however, the type had lost much of its significance.

The first example of Victory on a Roman coin appears on a silver didrachm from S. Italy—struck perhaps at Tarentum? It is related to a comparable bronze issue from that city and to the Nike of Agathocles: she holds a palm, however, instead of a thunderbolt. Victory appears throughout the period of the Republic and is especially noteworthy at the time of the Social War. Again, for Julius Caesar, Victory was, not surprisingly, a prominent type.

Bellinger concludes with a survey of the Victory type during the Imperial period—it was often used for propaganda purposes—when 'one of the most striking aspects of the coinage is its memorial and repetitive character' (38).

The Victory figure was eventually made into a Christian symbol (cf. bronze issues of Constans and Constantius II inscribed FEL TEMP REPARATIO)—'a lesser companion of the Christian God' (62). Tiberius II, on his accession in A.D. 578, used a cross as a reverse type for his gold issues and so removed the last trace of pagan symbolism on Roman imperial issues.

There are thirteen plates, which together with the text, provide a useful record of the main variations of the Nike type.

J. F. HEALY.

Bedford College, London.

The second half of the fifth century is one of the darkest periods in the numismatic and monetary history of the Roman world. The bronze coinage awaited the reforms of Anastasius; except in a few cities, above all Rome and Carthage, it consisted of nothing but tiny pieces which are generally so badly struck and poorly preserved that the study of them is difficult or impossible. Adelson and Kustas have made a major advance by their publication of a hoard of such coins, far better preserved than is usual, from Vólos, in central Greece. It consisted of 2231 coins, of which 1064 are studied by the authors, while 1167 illegible pieces have had to be disregarded. The date of deposit of the hoard is set by the authors at 476–491, and perhaps c. 480. Their book falls into three parts: an impeccable catalogue of the 1064 legible coins; a descriptive analysis of the hoard, and a metrological analysis. The descriptive section discusses the regional circulation of the issues of the various mints, by a comparison with other hoards of minima, particularly that published by Adelson and Kustas themselves in *American Numismatic Society Museum Notes* ix (1960). There is a valuable commentary on the changing output, and on the characteristics of style, of the mints, including a careful survey of the (limited) evidence relating to the mintmarks CHES and KOC.

The metrological analysis, for which Adelson was primarily responsible, although the study as a whole represents a collaborative effort, is the most ambitious and, one might say, the most exciting part of the book. From 396 to 475, except for the years 450–457, the bronze currency was issued at 1.14 grammes or 288 minima to the pound. This was by way of being their full weight under the law which fixed the price of bronze at 25 pounds to the solidus. (In the middle of the fifth century minima were temporarily slightly heavier.) With the accession of Basilisius the picture changed abruptly. It was he who took the first major step in the process of lightening the minima below the level set during the reign of Arcadius, reducing them to a level perhaps as low as 5 carats (0.94 gm.). To his twenty-month reign is to be dated the change in the official price of bronze to 20 pounds to the solidus. The financial resources of the state, already depleted by the heavy cost of the Vandalic expedition of 468, were strained at this time because of pressure from the Ostrogoths; and the reduction in the weight of the coinage is to be viewed as a move to ameliorate the fiscal problems of the day, by re-valuing the stocks of bronze held by the state. Coins from the (second) reign of Zeno are even lighter, being perhaps of 4½ carats (0.84 gm.); this again was the result of a policy intended to replenish the financial resources of the state.

The conclusions are convincing and valuable, particularly the identification of 475–76 as the date of change. The statistical analysis skates over a certain amount of thin ice to reach them; because of the 1167 coins that were not taken into account, I view with some reservations the discussion on pp. 25–6. But Adelson and Kustas have again made an important contribution on an exceptionally difficult topic.

D. M. Metcalf.

Oxford.


In the present monograph—one of a series produced under the auspices of the Heberden coin room—Dr Kraay discusses the composition of Greek silver coins in the light of evidence obtained by neutron activation analysis.

This completely non-destructive technique, outlined by Miss Vera M. Emeleus in *Archeometry* (Bulletin of the Research Laboratory for Archaeology and the History of Art, Oxford University) i, p. 6, and here described in detail (ch. 2), not only detects minute trace elements but has the added merit of being able to analyse the whole volume of the coin. Earlier techniques, based on X-ray diffraction analysis or on optical spectrometry, were liable to errors resultant from surface enrichment or corrosion (see further *Archeometry* III, pp. 32–3) and depended on the assumption of homogeneity. “The object to be analysed is placed in a flux of neutrons (usually those being generated in a nuclear reactor)” . . . and ‘comparison of the intensities of the different gamma-rays . . . with those obtained from a simultaneously irradiated standard of known composition (and similar in size and shape to the object being analysed) allows the amounts of the different elements present to be determined’ (pp. 9–10).

Most Greek silver coins contain two significant impurities, gold and copper, in varying amounts. In coined silver, therefore, a distinctive percentage of gold may help to characterise a common source of ore. Copper may be present for a number of reasons (cf. *Congrès international de numismatique*, Paris 1953, II, pp. 534 ff.). Variation in metallurgical skill may indeed be reflected in the differing copper content but the purity of Athenian ‘owls’—the surviving quantity of copper is usually less than 0.1 per cent’ (p. 15)—is more probably due to the quality of the ore rather than to a higher degree of skill in refining techniques (cf. p. 37) and so to be associated with the ‘exploitation of the Laurium mines’ (p. 16).

The results of 420 analyses from thirteen important mints including Athens are clearly tabulated (ch. III). One set will perhaps serve to show the importance of the study of the composition of Greek coins. Dr D. Raymond, in her corpus of Macedonian Regal
Coinage to 413 B.C. (ANS Numismatic Notes and Monographs 126), records varieties of tetrobols close in weight but distinguishable by their types: these are described as heavy (about 2.3 gms) and light (1.9–2.0 gms) respectively. The difference, however, of their intrinsic value is greater than their relative weights suggest since the light tetrobols contain up to 24 per cent copper. Kraay convincingly suggests that 'the light tetrobol must be a token piece for circulation within Macedonia, the heavier tetrobol of pure silver being intended for foreign trade' (pp. 21–2); finds at Olynthus support this view, which also explains the significance of the horseman type on the pure silver issues.

Kraay further discusses the interesting problem of the interrelation between mints and areas (ch. IV) and ends with an indication of some useful lines of future research in problems relating to Greek metallurgical and mint practice.

There are, in addition, twelve diagrams which show the results in a clear and immediately intelligible form. A short bibliography of other work in this rapidly growing field of study would have been useful although such information is readily accessible in the volumes of Archaeometry and elsewhere.

The present monograph shows once more the important contribution which modern scientific techniques may make towards the solution of outstanding problems in the field of Greek numismatics.

J. F. Healy.

Bedford College, London.
SHORT NOTICES


This treatise, written in Dutch with a summary in English, is a useful discussion of the place of the Frogs in the history of Greek literary criticism. A chapter on the nature and purpose of comedy as seen in Aristophanes is followed by a detailed account of the criticism in the Frogs and two chapters on the origin of the critical ideas in Aristophanes, their relationship to views of Gorgias and the sophists, and some later developments. There is also an appendix on the starting-point and composition of the Frogs and a good bibliography and index. Some may think that the author is a little too confident about distinguishing the personal views and personal contribution of Aristophanes from dramatic requirements and ideas current at that period; but he has assembled much useful material and discussed it with good sense.

P. T. S.


In this doctoral thesis, in Dutch with a French summary, the author examines the question in what ways and how consistently Aristophanes develops his criticism of the present by contrast with better times and better men in the past. In his inquiry, which covers the whole political, social and literary scene, the author discriminates carefully between different periods, but finds in general that though in some respects Aristophanes is traditional and conservative in outlook, too much stress has been laid on this aspect of his plays. Treatment of so wide a field in about a hundred pages is bound at times to seem rather cursory, but this is a useful and well-documented survey.

P. T. S.


This edition of the I.A., containing a text with introduction, prose translation and notes in modern Greek, was completed some time before 1936, the year of the author's death. The long introduction is clearly the work of an enthusiastic though not always very discriminating admirer of Euripides; the section on the text has become rather out of date, since it and the comments on the text do not take account of the work of Page, Turyn and others. There are places where I prefer G.'s text to that of Murray, but he includes without question a good deal that most editors have rejected or at least suspected, and the critical apparatus is sometimes too sparse for the use of scholars. The notes, mostly short, consist mainly of paraphrase and parallel passages, and these, together with a translation that is as far as I can tell accurate and clear, should be most useful to young students of the play in Greece. The author's wide knowledge of modern literature is shown in interesting parallels from English, French and German.

P. T. S.


This is a useful monograph, setting out all the available evidence on payments for civilian service; wages of jurors, members of the council and those attending the assembly (pay of magistrates is omitted), the dole of the ἀδίκους, the diobelia and the Theorika. The author is thorough, sensible and independent, and writes in a pleasantly lively style. If it does not add much to our knowledge, the work brings much together that was scattered. The author thinks that the Athenians spent too much on subsidising the idle poor, but readers need not accept the author's prejudice on this point.

A. H. M. J.


A popular book about Greek archaeology, describing it in terms of the sites excavated rather than groups of objects. A useful guide for non-archaeologists and for any visitors to Greece, but rather poorly illustrated with many copied plans and photographs.

P. T. S.


Studies in archaeological method and the relations between archaeology and botany, anthropology and scientific aids, by various French scholars. Special essays by Will on the limits, possibilities and tasks in the economic and social history of ancient Greece; Deshayes on Bronze Age implements and economy; Ducat on Byzantine pottery and history (à propos of the Argos excavations); Vallet and Villard on Greek pottery and economic history.

Recent accessions including a Medusa Rondanini, heads of Augustus and Claudius, a good variety of bronzes and Hellenistic terracottas, vases (Proto-geometric, Corinthian, Boeotian, Tarquinia Painter oenochoe, Komaris Painter calyx crater), plastic vases (a fine squatting satyr, Hellenistic), gems (one Minoan, one archaic, silver ring with Nereid).


A brief illustrated account (including four colour plates) of plant life in Greek art; the formal gardens in the area of the Athenian Agora; and the more popular plants, shrubs and trees of antiquity.


A highly imaginative attempt to relate the siting and planning of Greek sanctuaries to the landscape, suggesting that particular settings were thought especially appropriate for the prehistoric palaces (Minoan and Mycenaean contrasted) and for the various classical deities.


Mainly of interest to the Hellenist for minor objects of the later seventh and sixth centuries B.C. which have their kin in the Aegean world. E.g. the pyxis with dancing women (U. 2677: cf. the recently found base from Cyzicus, Akurgal, Künstlerreisen, fig. 200); the ivory sphonix-pot (U. 7904: cf. the ivory sphinx, Perachora i i pl. 171); phialai, jewellery, fibulae.


A new series, in the tradition of the great collections by Brunn, Bruckmann, and Arndt. This is to be devoted to unpublished or under-published works. From the first fascicle it appears that the treatment may range from pure description with minimum commentary to exhaustive publication with comparative material illustrated. Contents: Drei spätantike Porträts (by H. von Heintze; Bronze statue of Merkur von Thalwil, by W.-H. Schuchardt; Bronze statue of Wadsworth Atheneum in Hartford, Connecticut, by E. Biefeld; Two Archaic Korai in Chios, by J. Boardman; Archaischer Jünglingskopf in Istanbul, by F. Eckstein (the head often said to be from Rhodes, but joined by E. to fragments of a kouro on Samos).


The new edition has an improved text with new drawings, and a full description of the permanent exhibition in the Stoa of Attalus.


An abridged version of chapter 3 in the forthcoming volume iii of Samothrace, dealing with the pedimental sculpture of the Hieron.


A new series, intended to describe and illustrate all major works of art from Greco-Roman Egypt. Series A is of sculpture and these fascicles have mainly sarcophagi, and figures of Aphrodite, Nile, Serapis, etc. The grouping is not to be wholly by subject or by collections.


Essays on Attic relief craters of the fifth-fourth
centuries; and Etruscan relief kantharos of the fourth century; a Hellenistic relief bowl from Rhodes (with Ajax and Cassandra; Scylla and Odysseus); and a relief lagynos and Arretine stamp (with scenes of love-making).

POULSEN (V.) Griechische Vasen und Bauten.

A series of excellent photographs of Greek vases and architecture, treated separately, with brief descriptive text.
BOOKS RECEIVED


Buystendijk (F. J. J.) and others. Über die menschliche Bewegung als Einheit von Natur und Geist. (Beiträge zur Lehre und Forschung der Leibeserziehung, 14.) Stuttgart: K. Hofman. 1963. Pp. 80. DM. 4. (For subscribers to the whole series DM. 3.50.)


BOOKS RECEIVED


OPUSCULA ATHENIENSIA, iv. (Skriver utgivna av Svenska Institutet i Athen, 4º, viii.) Lund: Svenska Institutet i Athen. 1962. Pp. vii + 310. 32 plates. 119 text figures. Sw.kr. 90.


Correction: Under “Books Received” in JHS LXXXIII (1963) two volumes of Διαλέξεις by Professor George Thomson were wrongly described as published by the author. Both were published by the *Εκδοτικόν Ινστιτούτον Αθηνών*. 
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