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ΠΡΟΕΔΡΙΑ ΜΕΝΕΛΑΟΥ ΠΑΛΛΑΝΤΙΟΥ

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THE GREEK DARK AGE REVISITED

ΟΜΙΛΙΑ ΤΟΥ ΚΑΘΗΓΗΤΟΥ ΚΑΙ ΜΕΛΟΥΣ ΤΗΣ ΒΡΕΤΑΝΝΙΚΗΣ ΑΚΑΔΗΜΙΑΣ  
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*Your Excellency, Academicians, Ladies and Gentlemen, I hope you will forgive me if I give my paper in English.*

*The traditional means of entry to the dark age of Greece has been through Homer; and it was so in my case as well. It was in this connection that, as a second-year student at Oxford, I used to visit Exeter College in order to attend the lectures on the Homeric and other epics by your Secretary. I have an equally clear memory of another, even earlier decisive moment. It must have been during 1951 that my Classics teacher at school brought into the classroom a book which had then recently appeared, Miss H. L. Lorimer's *Homer and the Monuments*, and briefly introduced it to us before placing it in the library. I remember being surprised at the use, which even then had an archaic sound, of the word "monuments" in the title of the book; but not at the priority implied by the order of the terms, "Homer" before "monuments". After all, the title of an earlier book by M. P. Nilsson, *Homer and Mycenae*, had embodied an even sharper inversion of the chronological sequence, in favour of the order of importance. No one would be interested in the dark age, it was implied, no one would even be greatly interested in Mycenae, if it were not for Homer. Who was I to question this evaluation?*

There came a time, however, when I did begin to question it, as many others were beginning to do. It was clear to any thoughtful reader of works on early Greek history that a paradox had arisen in respect of the centuries before the first Olympiad in 776 B.C. The greater the importance ascribed to the developments of those years, it seemed, the more reluctant were scholars to venture even a surmise as to the nature and sequence of those developments. For many in the 1950's and 1960's, Mycenaean civilisation had come to appear as some kind of overture to the great drama of Classical Greece. But when the overture came to an end, the curtain did not rise. Instead, the lighting appeared to fail and there was a long and embarrassing silence, disturbed only by the audible noise of scenery being moved around. Something had clearly gone wrong with the production. Was it then better to regard the two episodes as a sort of "double bill", two successive but quite separate performances on the same premises? If so, the interval seemed unduly long and the second piece seemed to make too many obvious acknowledgments to the first. On either account, it seemed clear to many that the entertainment would make better sense if the curtain could be raised and, once raised, kept up.

It might have been possible to write a survey of the dark age at any time from about the mid-twentieth century on: that is, after the publication of the first volumes of the Kerameikos excavation report, of Miss Lorimer's book and of Vincent Desborough's study of Protogeometric pottery. But in the event, only somewhat later did the idea apparently occur independently to at least three people within a year or so of each other — Jan Bouzek, Vincent Desborough and myself, all in the late 1960's<sup>1</sup>. Desborough's *The Last Mycenaeans and their Successors* had meanwhile appeared in 1964 and provided a very helpful basis for the beginning of the period for all of us; so that it was only fair that Bouzek's book and (in the form of galley-proofs) my own should have been ready in time for Desborough to see them, however briefly, when compiling his *The Greek Dark Ages*. Today, eleven years have passed since the last of these books appeared, and it is possible to treat them as a collective

1. Jan Bouzek, *Homerisches Griechenland* (Prague, 1969); A. M. Snodgrass, *The dark age of Greece* (Edinburgh, 1971); V. R. Desborough, *The Greek Dark Ages* (London, 1972).

account of the state of knowledge at that time, and to review subsequent developments without entering too deeply into the differences in viewpoint that they represented. As Desborough himself wrote, they could be regarded as complementary to each other.

If there is one respect in which the books appear "dated" today, it is in their relative lack of awareness of the great change in archaeological thought, and especially the development in archaeological theory, which was taking place at that very time and which has since had widespread repercussions in the world of archaeology. The opening sentence of my own book was "The method of this work is empirical". 1968, when these words were actually penned, was probably the very last year in which such a sentiment could be expressed by an archaeologist in innocence. For that same year saw the publication, in America, of *New Perspectives in Archaeology* edited by Lewis and Sally Binford, and in England of David Clarke's *Analytical Archaeology*, works which not only set out to replace the traditional methods of archaeological scholarship with something (at least allegedly) new, but which, above all, castigated the empiricism of traditional archaeology as one of its greatest failings, and the one perhaps most urgently in need of rectification. Their passionate advocacy of a different method of reasoning, the hypothetico-deductive method, may not have convinced or even penetrated to all their fellow-archaeologists; but it made a disturbing impact on me. As a matter of fact, when undertaking an anxious re-examination of my own work, I found with some relief that the book was not after all entirely empirical in its approach. For example, it stated near the beginning (on page 2) a series of hypotheses about the dark age—that it witnessed grave depopulation, loss of material skills, and so on—which were then, after a fashion, tested in the later chapters.

These same hypotheses, about the reality and intensity of the dark age, will also provide a starting-point for a more detailed examination of the discoveries of the past eleven years, and of their effect on the general doctrine of the dark age. But there is a prior question to be examined, that of the break at the end of the Mycenaean age. Did it, or did it not, involve the immigration, from outside the limits of the Mycenaean culture, of a body of people so numerous as to give a new population-base for the later development of Greek culture, and thus to make this development in an important sense a fresh start in the history of Greece? It is clear from

both Bouzek's and Desborough's books that each of them, if with a different emphasis, would have answered "yes" to this question; while my own answer was an almost unqualified "no". Bouzek looked very far afield, so that he was prepared for example to connect the actual downfall of Mycenae with an explosion of population in the area of modern Bohemia. For the later period of the twelfth and eleventh centuries B.C., he joined with Desborough in detecting greatest significance in the spread of single burial in general, and of cist-tombs in particular; and in some of the metal objects—notably fibulae and long dress-pins—found in these graves. On the other side, I stressed the earlier incidence of single burial at different periods of the Aegean Bronze Age, and the persistence of some Mycenaean features within the single graves of the twelfth and eleventh centuries. In this particular issue, the last few years have brought to light surprisingly little new evidence (as distinct from new theory and argument): perhaps the last discovery which bore significantly on the problem was Dimitrios and Maria Theocharis' report, in 1970, of a Mycenaean cist-tomb cemetery of the 14th century B.C. at Iolkos<sup>2</sup>, and that occurred in time to receive a fleeting reference in a foot-note to my book.

But there has been an important new development in an allied field, which concerns this same period. The awareness has emerged, rather abruptly, of a form of burnished, hand-made pottery, present on a range of twelfth-century sites. One after another, excavators have begun to report this ware among their new finds, or after re-examination of earlier finds in museum store-rooms: it has now been published at half a dozen sites in the Peloponnese and at Lefkandi in Euboea, and provisionally reported from one or two Cretan sites as well. At the time when I wrote, this burnished pottery had been only briefly and inconspicuously noticed from Mycenae, and I attached little importance to it. From today's vantage-point, by contrast, that first report looks more like the Biblical "cloud no bigger than a man's hand". Already the significance of the phenomenon has been quite extensively discussed, notably by Sigrid Deger-Jalkotzy, Klaus Kilian, J. B. Rutter and Hector Catling<sup>3</sup>. Other considerations apart, the case has been

2. *Athens Annals of Archaeology* 3 (1970), 198 - 203.

3. S. Deger - Jalkotzy, *Fremde Zuwanderer im spätmykenischen Griechenland* (Vienna, 1977); K. Kilian, "Nordwestgriechische Keramik aus der Argolis

*instructive in showing how archaeologists can find important new evidence when they know exactly what to look for. But, more importantly, some explanation must be offered as to why an area like southern Greece, which for some thousand years had been producing its finer pottery largely or exclusively by means of the potter's wheel, should suddenly revert to the production of burnished hand-made ware for domestic use (for the pottery comes almost entirely from settlement-contexts). Does it not suggest that Desborough was right, and that the roughly contemporary resurgence of the cist-tomb reflects the same phenomenon, the arrival of a substantial non-Mycenaean population element whose culture was both intrusive and relatively backward, having no familiarity with the potter's wheel?*

*I am bound to say that I should be more ready to concede this, if there were some measure of agreement in identifying the outside source from which this type of pottery, and therefore its assumed producers, might have come to southern Greece. For Desborough, the likeliest origin for the cist-tomb had been north-western Greece, for Bouzek Macedonia and Epirus together. But for the hand-made pottery, we have been asked to look to a variety of sources further afield: the north-eastern Aegean, the Balkans, Italy. None of the parallels adduced from these regions seems to me to be really convincingly close: perhaps one should not expect them to be, considering the long distances over which the users of the pottery would supposedly have migrated, but in the absence of such evidence it would surely be premature to surrender the field without a shot. When one considers the long list of technical and artistic skills which vanished after the fall of Mycenaean civilisation—writing, fresco-painting, gem-carving, building in monumental masonry, figure-painting on pottery—is it not conceivable that the regular use of the wheel was another casualty? As for burnishing, it was a time-honoured way to improve the surface of a hand-made ware that was too rough for painted designs. I feel that the great emphasis given to this new factor derives in part from the fact that it is new,*

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*und ihre Entsprechungen in der Subapenninfacies", in Atti dell XX riunione scientifica dell'Istituto italiano di preistoria e protostoria in Basilicata, 1976 (Florence, 1978), 311 - 20; J. B. Rutter in American Journal of Archaeology 79 (1975), 17 - 32; 80 (1976), 187 - 88; and (with E. B. French) 81 (1977), 111 - 112; H. W. Catling in Annual of the British School at Athens 76 (1981), 71 - 82.*

and in part from the fact that it applies to the area where evidence for intrusive features was previously most conspicuously lacking, that of fine pottery. If we had known about it for fifty or even fifteen years, I think that it would have been assimilated without greatly altering the balance of the evidence. For the fact remains that this hand-made pottery is a minority ware: it is not found on all the occupied sites of its period, even in the Peloponnese, and where it does occur, it is still heavily outweighed in quantity by the wheel-made pottery in the Mycenaean tradition.

We may perhaps now return to the central features of the dark age, as represented in the prevalent modern doctrine: the features, that is, which are thought to show that the age was indeed a dark one, for the contemporaries who experienced it as well as for the modern scholar who tries to investigate it. How far has recent work affected the claim that these centuries witnessed depopulation, loss of skills, both material and intellectual, loss of prosperity, or isolation? Have they served to reinforce these claims, or have they on the contrary strengthened the view advanced by Moses Finley in 1970, that "in the sense that we grope in the dark, and in that sense only, is it legitimate to... call... the period from 1200 to 800 a 'dark age'"<sup>4</sup>. Let us begin with the question of population, which for many people is the most fundamental factor of all. If the population of Greek lands really shrank to a small fraction of what it had been in the Late Bronze Age, then whether this was the result of other unfavourable factors or whether, on the contrary, it was itself the cause of further negative developments, the conclusion will still hold good, that the likelihood of a period of economic and political recession is enormously increased. In the conditions of antiquity, when agriculture provided a very large part of the basis of society, a deserted or thinly-populated landscape made it impossible for the inhabitants of the country in question to match the achievements of a period when that landscape had been densely settled, let alone to exert political power over other areas.

To estimate the level of a population by archaeological means, one must look both at the distribution of sites, and at the size and density of the sites when found. By the former criterion, one could claim that recent work has done everything possible to support the belief in a post-Mycenaean

4. *Early Greece: the Bronze and Archaic Ages* (London, 1970), 72.

depopulation. Since I first reached the conclusion that the number of known sites with eleventh-century occupation in Greece was about one-eighth of what it had been two centuries earlier, not a single major new settlement-site has come to light that was occupied between, say, 1100 and 900 B. C. Further, the technique of archaeological survey has now offered a different basis for estimating population-change in the localities where it has been applied. What results has it shown for this period? Again and again, the story has been the same: surface remains of the dark age have either been markedly rarer than those of the preceding and succeeding periods (this was the case with the pioneering survey of Messenia by the University of Minnesota expedition<sup>5</sup>, and with a number of more intensive surveys since); or they have been virtually or totally absent, as has happened with at least three current surveys, the American Argolid Exploration Project and the British surveys of the Megalopolis basin in Arkadia and of the Thespias region in western Boeotia, and with the completed sample survey of the island of Melos<sup>6</sup>. Now it is often pointed out that there could be other explanations for this: that, for example, excavations have not yet brought to light the full range of pottery-types used during the dark ages, so that fragmentary material belonging to this period has not been recognised as such. I admit that this is theoretically possible; but it is usually by their finer decorated wares that we recognise all periods, and we know well enough what the decorated wares of the Protogeometric and earlier Geometric phases were like: why should they be so elusive compared with, say, Mycenaean or black-glaze wares?

The evidence of excavations, although it bears on the same problems, does so in an entirely independent way. What we look for here, of course, is either surviving structures or, failing that, occupation-levels on a settlement-site which belong to this period. What we find is that site after site shows a period of Bronze Age occupation, then a hiatus, then a resettlement, usually beginning in the later eighth century B.C. On a sanctuary-site, where stratified domestic debris is usually absent, we have instead to look at

5. W. A. M c D o n a l d and G. R. R a p p (edd.), *The Minnesota Messenia Expedition: reconstructing a Bronze Age regional environment* (Minneapolis, 1972).

6. C. R e n f r e w and M. W a g s t a f f (edd.), *An island polity; the archaeology of exploitation in Melos* (Cambridge, 1982).

the sequence of portable dedications and see what periods are represented. Here the answer has most often been a simpler one: the dedications simply do not begin before the eighth century and the earlier deposits, if any, are usually domestic in character and date back, once again, to the Bronze Age. There have of course been exceptions, and I shall be mentioning two of them in a moment. But we can sum up the question of settlement by saying that, unless we have been extraordinarily unlucky or unperceptive in two quite separate methods of investigation, the population of Greece in the dark age was much smaller and much less dispersed than in either the earlier or the later periods of antiquity.

A similarly negative picture can be given in some other, more specialised categories of evidence. No one, to my knowledge, has found a single particle of evidence for the existence of the art of writing in Greece for a period of 300, perhaps 400 years before about 750 B.C., in the last decade of research any more than in earlier work. No one has found evidence for the survival of fresco-painting or gem-carving after the end of the Mycenaean world. One or two pieces of representational art have come to light, it is true: figure-scenes on Cretan pottery, for example, are now known to have been attempted earlier and more often than had been thought, but still hardly before 900 B.C.<sup>7</sup> Then there is the remarkable terracotta figurine of a centaur from the site which, before all others, demands extended discussion in this context: Lefkandi in Euboea<sup>8</sup>.

The preliminary results of the joint Greek and British excavations at Lefkandi were already known at the time when Bouzek and I were writing our respective works, but Desborough's book marked an important advance here in that he had access to a much wider range of finds, then not yet fully published. More striking still, however, have been the discoveries since 1972. The cemeteries, when fully published in 1980, provided a third large body of evidence to set beside that of the two mainland sites which had previously produced major cemetery evidence of the dark age, namely Athens and Argos. But Lefkandi was distinctly different from either

7. See L. H. Sackett, *Annual of the British School at Athens* 71 (1976), 117 - 29 and H. W. Catling, *Archaeological Reports* 23 (1976 - 77), 15 - 16.

8. See especially M. R. Popham, L. H. Sackett, P. G. Themelis, *Lefkandi i: the Iron Age: the settlement and cemeteries* (London, 1981 [text], 1980 [plates]).

of those sites: in ceramic development more backward than they, as was shown by the frequent associations of imported and of native pottery, it showed itself in terms of overseas contacts to have been decidedly more enterprising, from an earlier date, than either of the two mainland sites or indeed than anywhere else in the Aegean area with the possible exception of Crete. (The mention of Crete prompts me to say that, with the extensive recent rescue-excavations of Iron Age tombs at Knossos<sup>9</sup>, we shall soon have a fourth very large body of cemetery-evidence from the Aegean for the dark age).

To return to Lefkandi: for all the impressive quality of its grave-finds, there is every sign that the size of its community, as with other sites of the period, was very small indeed. I have calculated that even the four large cemeteries so far discovered there, if excavated in their entirety, would still not represent a population larger than about 70 at any one time<sup>10</sup>. Of course there will be other cemeteries to be found, including some of the same exact phases as those so far discovered; but how many? I doubt whether there can be enough to attest a population even as large as three to four hundred. Calculations from the surface-area of a settlement are another way of arriving at population-estimates; but here I must say that the assumptions derived from the highly-nucleated sites of Mesopotamia and elsewhere seem to me to be altogether too high: for example, those used by Professor Renfrew for the Neolithic Aegean (200 people per hectare within a settlement) and for the Bronze Age (300 people)<sup>11</sup>. In a period like the dark age, when there was no pressure on space, there is no sign that densities reached that level: even the 9th- and 8th-century site of Zagora on Andros can hardly have reached 200 per hectare for the nucleated areas that have been excavated, and it is highly doubtful whether the whole of the fortified area was built up to the same degree<sup>12</sup>. People needed space

9. See now H. W. Catling, "Knossos 1978", *Archaeological Reports* 25 (1978-79), 43-58.

10. "Two demographic notes", in R. Hägg (ed.), *The Greek Renaissance of the eighth century B.C.: Tradition and Innovation* (Skrifter Utgivna av Svenska Institutet i Athen, Series in 4<sup>o</sup>, XXX, Stockholm, 1983).

11. Cf. *The emergence of civilisation* (London, 1972), 251.

12. See A. Cambitoglou and others, *Zagora i* (Sydney, 1971); *The Archaeological Museum of Andros* (Athens, 1981).

for courtyards, gardens and the accommodation of livestock: a modern village in Boeotia, for example, houses only about 30 people per hectare.

I have published some figures, based entirely on relative population change, which draw attention to the very low level of populations in Attica and the Argolid in the 11th, 10th and 9th centuries B.C. and to the extremely abrupt rise which comes in the 8th<sup>13</sup>. These figures are based on the frequency of burials in each generation; no doubt subsequent research will show some need for adjustment, for example by allowing for a standard proportion of child-burials in those cases where the children's graves were located away from the main cemeteries, and have yet to be discovered. But I doubt whether the original emphasis will be much altered: population in these centuries was surely very much lower than its later levels, and this must do something to explain the contrast between the slow economic and political growth in this period, and the sudden explosion which seems to have occurred in the eighth century.

But I must return yet again to Lefkandi, to comment on the discovery which above all you will be expecting me to refer to: the finding in 1981 of the Heroön and its associated burials<sup>14</sup>. If a site like Lefkandi can produce, from the middle of the tenth century B. C., finds like these, then has not the whole modern doctrine of the dark age been severely undermined? As far as the gravegoods are concerned, I would say not: it is of the greatest interest that Mr. Popham now believes that two of the more striking objects were not only imported, but also heirlooms. The decorated bronze vessel which held the ashes of the "hero" himself was probably made in Cyprus several generations before, while as for the gold pendant in the woman's grave, if it is to be dated by the Babylonian parallel which it most resembles, it was perhaps already a thousand years old at the time of its interment. But then what about the building itself? Its form may resemble that of other apsidal buildings, some of them temples, from Greek sites, but it is probably 100 to 150 years earlier than the oldest of these parallels. It may have stood for less than a generation, but the fact remains that the people of Lefkandi were capable of constructing an edi-

13. See *Archaic Greece: the age of experiment* (London, 1980), 22 - 24.

14. M. R. Popham, E. Touloupa, L. H. Sackett, "The Hero of Lefkandi", *Antiquity* 56 (1982), 169 - 174.

fice more than 40 metres long at a date round 950 B.C., and one must admit that this is news to us. Likewise, the whole practice of constructing a monument over the grave of a recently-dead notable may be paralleled at the West Gate of Eretria, but that again is about 250 years later<sup>15</sup>.

The Lefkandi find is an astonishing one for its period; but one should mention almost in the same breath the remarkable sanctuary at Kalapodi in Phokis, where Dr. Felsch's excavation has revealed that rare thing, a site where sanctuary dedications appear to cover the whole Protogeometric period, and include, almost for the first time, much Protogeometric pottery<sup>16</sup>. Nor should we overlook the sequence of substantial temples unearthed by Professor Shaw at Kommos in Crete;<sup>17</sup> the earliest of them must go back to the tenth century B.C. and, even though the ground-plan (or at least, that of its successor) wears an uncharacteristic look for the period, it must occasion some revision of views of the early development of temple-buildings in the Aegean area.

I should like to turn now to the question of metallurgy, which to me has always seemed the most important single class of archaeological material, and in this period especially so. For several thousand years of human history, metals were the key to survival, and a period in which some basic change in the relationship between man and metals took place, such as the transition from bronze-use to iron-use, gives it an added significance. In this field one can report some progress. In *The dark age of Greece*, I advanced the theory that the adoption of iron in Greece was much accelerated by the fact that bronze, and more especially tin which forms a vital component of bronze, had become difficult to acquire by any means other than the melting down and re-use of existing bronze artefacts. The theory has won some support, but two developments have persuaded me that it stands in need of some further refining. First, metal analysis of bronze objects from two dark age sites, Nichoria and Lefkandi, has recently yielded the finding that the tin content in these bronzes, far from being defi-

15. See C. B é r a r d, *Eretria iii: l'Héroun à la Porte de l'Ouest* (Bern, 1970).

16. R. F e l s c h and H. K i e n a s t, "Ein Heiligtum in Phokis", *Athens Annals of Archaeology* 8 (1975), 1 - 24; R. C. S. F e l s c h and others, "Apollon und Artemis: Kalapodi Bericht 1973 - 77", *Archäologischer Anzeiger* 1980, 38 - 123.

17. See *Archaeological Reports* 27 (1980 - 81), 45 - 46; 28 (1981 - 82), 55 - 56.

cient, was actually in most cases too high, to the point that the effectiveness of the objects would be much reduced through brittleness<sup>18</sup>. This at least confirms the view that Greek smiths no longer had total mastery of bronze-working, but it goes very badly with the suggestion that tin was in short supply. One possible explanation (for which I am indebted to Dr Catling) is that small decorative bronze objects—rings, pins, fibulae—such as were used for the tests, are not the best evidence for the bronze-smith's craft; for in them, strength is really less important than beauty, and an increase in the tin content will give a more attractive colouring.

Secondly, I argued that early ironwork will not, in any case, be much superior to the very best bronze in respect of strength and hardness, unless or until the secret of carburisation has been discovered. Carburised iron is in effect mild steel, and once the further technique of quenching the metal in water is discovered, the gain in hardness is dramatic. We know from a simile in the *Odyssey* that quenching was known by Homer's day, but that could still be more than three centuries after the first widespread adoption of iron in Greece. But in the last few years, extensive testing of early iron-work from Cyprus has been carried out (much of it so recent that it has not yet been published), and what it has shown is that, from the very beginning, the iron bears traces of having been in every case carburised, and in most cases quenched as well. I am convinced, for a reason that will emerge in a moment, that when specimens from Greece are tested, they will show similar results. This means that the advantages of switching from bronze to iron were being exploited to the full, and were therefore clearly understood, from very early on.

The connection between early iron-working in Cyprus and in Greece was another theme of my treatment. I argued that practical iron-working was adopted slightly earlier in Cyprus than in the Aegean, and that there were several important resemblances in the typology of the early iron weapons and tools; but there were two awkward gaps in the evidence, in that the dagger and the sword, both of them very important classes of artefact in the early story of Greek iron, were very thinly represented among the

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18. R. E. Jones in *Lefkandi i* (see above, n. 8), 447 - 59; G. R. Rapp and S. Aschenbrenner (edd.), *Excavations at Nichoria in South-West Greece* (Minneapolis, 1978) 166 - 181.

early Cypriot material. Here I am glad to say that Dr Vassos Karageorghis' excavations in the late 1970's and early 1980's at Kition and Kouklia have come forward to fill the gap completely. A dagger from the 1977 season at Kition belongs to the middle or later 12th century B.C., and is thus nearly a hundred years earlier than the first Aegean iron daggers which quite closely resemble it<sup>19</sup>. The tombs at Kouklia<sup>20</sup>, meanwhile, of the 11th century B.C., have produced a whole series of iron knives, spearheads and, above all, swords; these last, once again closely resemble the contemporary and slightly later Aegean iron swords, and they also show an interesting feature which was familiar from the treatment of swords in later burials at Athens and Lefkandi: the swords have been "killed" by doubling them sharply over in the middle of the blade. This was not necessary for fitting them into the tomb in most cases: it must therefore have had a symbolic significance of fitting the sword for the use of the dead and denying it to anyone else, and that symbolism seems to have been common to parts of Cyprus and of Greece in the early iron age. Another dagger has its whole pommel preserved: the first such find, and one which at last explains why swords on Greek Geometric vases are invariably shown with a hilt shaped like a capital T. The figures for the proportional use of bronze and or iron in the Mediterranean world and the ancient Near East, show that Cyprus is at the very forefront of those civilisations adopting iron. No other area, not even Palestine and Syria, shows such an early conversion from bronze to iron for practical objects. The theory that Cyprus pioneered, the Aegean fairly swiftly followed in, the replacement of bronze by iron as the main metal for practical use seems reinforced<sup>21</sup>.

I should like now to move on to a later issue, one on which appreciable light has been thrown by excavations since 1970, especially in the Cyclades: this is the rise of the new Greek settlements in the 9th and 8th centuries. We have already seen that there is reason to expect an increase in popu-

19. V. Karageorghis, "Fouilles de Kition", *Bulletin de Correspondance Hellénique* 102 (1978), 914 - 916, fig. 84.

20. "Fouilles à l'Ancienne-Paphos de Chypre", *Comptes Rendus de l'Académie des Inscriptions et Belles-Lettres* 1980 (janvier - mars), 122 - 136.

21. See A. M. Snodgrass in T. A. Wertime and J. D. Muhly (edd.), *The Coming of the Age of Iron* (New Haven and London, 1980), 335 - 74.

lation at this time, and one way in which it shows itself is in the choice of new sites for settlement. To the examples known from earlier excavations, notably Vroulia on Rhodes, Emborio on Chios and Kastro on Siphnos, we can now add several others. Some of them are not in fact new sites: they are occupied ones of the Bronze Age and, in two cases, they make use of the fortifications of that period which were still evidently serviceable. This provides a hint, both that security was a major concern at both periods, and that there was a recognition of the superiority of the resources which had once been available to those which the new settlers possessed. What we almost never find, however, is occupation that is continuous between these two epochs: there is a period of desertion and then a return.

The site of Zagora on Andros has been mentioned already: its excavation by a Greco-Australian expedition had progressed far enough by 1969 for it to be treated in some detail in the books on the dark age mentioned earlier. More has been added, however, through subsequent study<sup>22</sup>. We now know that the fortification-wall at Zagora is earlier than any of the housing areas so far excavated, and was probably the very first construction to be built once the site had been chosen. Next, there is the hill-top site at Agios Andreas on Siphnos excavated by Mrs Barbara Philippaki<sup>23</sup>, interesting because here the Bronze Age fortification was reused in the eighth century and indeed improved upon then or a little later; the site remained in occupation for some centuries, though the polis of Siphnos was to be established elsewhere, at the Kastro site near the sea. Thirdly, Mrs Photini Zapheroπούλου discovered a remarkable settlement at Vathy Limenari on the tiny island of Dhonoussa, east of Naxos<sup>24</sup>: occupied in the ninth and eighth centuries only, it too has a strong fortification-wall, built perhaps not long after that at Zagora. Finally, Prof. Dimitrios Schilardi has been excavating a small but impressive site at Koukounaries on Paros, on a hill near a bay<sup>25</sup>. This too was not suited to become the polis site

22. See the works cited in n. 12 above.

23. *Arkhaiologikon Deltion* 25 (1970), *Chroniká*, 431 - 434.

24. *Arkhaiologikon Deltion* 22 (1967), *Chroniká*, 467; 24 (1969), *Chr.*, 390 - 393; 25 (1970), *Chr.*, 426 - 428; 26 (1971), *Chr.*, 465 - 467.

25. *To Ergon tis Arkhaiologikis Etaireias* 1978, 51 - 53; 1979, 22 - 24; 1980, 38 - 39.

of Paros, and was presently to be abandoned; but here we again have, as at Agios Andreas, a powerful Late Bronze Age fortification which was apparently pressed into service again in the eighth century. Dr Petros Themelis has also recently published a very useful plan of another site, Xobourgo on Tenos: here occupation begins in the same period, though the fortification in this case is a little later<sup>26</sup>.

I should like to draw your attention to a common feature of many of the sites which I have been discussing, including those occupied for the first time as late as the eighth century B.C.: this is their *n a m e s*: Lefkandi, Kalapodi, Emborio, Vroulia, Zagora, Agios Andreas, Vathy Limenari, Koukounaries, Xobourgo — what have they all in common? The fact that they are all modern names, names which we have to use because we do not know their ancient ones (though there are those who argue that Lefkandi was “Old Eretria”). Why is this? It is because they did not survive, or did not survive as significant places, into the literate epoch of Classical Greece. There are several, including Lefkandi, Zagora, Vathy Limenari and, after c. 550 B.C., Vroulia, which were by then totally deserted. This brings out an important point about the sites—that they are located from motives which were no longer overriding ones by the seventh century B.C. and later—and about the dark age as a whole, its “otherness”. “The past is a foreign country: they do things differently there”, wrote the English novelist L. P. Hartley. A fifth-century Greek, even, could have said something of the same about the dark age. One could illustrate the break in many other ways: to an archaeologist, one of the most striking is that of burial practices. The great armouries of weapons in men’s tombs, the display of jewellery in women’s, which characterise the richest graves of the eighth century, were a relic of primitive ostentation which had no place in Classical Greece. When warrior-graves stop, the weapons (and the jewellery) abruptly begin to appear in another sort of context, the great sanctuary sites. A table will show the effect of the change at Olympia: the great Geometric tripods, the characteristic rich dedications of earlier times, suddenly disappear in the early seventh century B.C. and are replaced by the arms and armour of the Greek hoplite<sup>27</sup>. Many examples of these had no doubt been captured from

26. P. G. Themelis, *Frühgriechische Grabbauten* (Mainz, 1976), 4 - 23, Plan 1.

27. See the table in *Archaic Greece* (above, n. 13), 105.

enemies, but some were personal dedications, taking the place of the wasteful practice of burying them in the owner's grave.

A "foreign country"; but have we not got an outstanding guide and interpreter to that country, in the person of Homer? It is anyway fitting that I should end, as I began, with him. The material background of the Homeric epics is one subject on which, it is safe to predict, universal consensus will never be reached. The picture is quite simply too complicated. I can see no reason to believe that any one society or period, whether in the dark age or earlier, provided the inspiration for the culture that Homer portrays. One of the most striking contributions to the subject in recent years has come, yet again, from Cyprus and the excavations of Vassos Karageorghis at Salamis. Tombs of the eighth and seventh centuries there show a remarkable series of correspondences with the descriptions of the funerals of Patroklos and Hektor in the *Iliad*, which are surely too close to be mere coincidence. The deposition of the cremated ashes—and cremation itself was a very unfamiliar practice in Cyprus—wrapped in a cloth inside a bronze vessel, and accompanying jars full of olive oil (the inscriptions in the Cypriot syllabary prove this), the horse-sacrifices and the occasional human sacrifices, the inclusion of rich objects like this ivory throne which corresponds so well with Penelope's in the *Odyssey*: all of this is redolent of Homer<sup>28</sup>. Yet it is typical of Homeric scholarship that this discovery raises problems as well as throwing new light; for the earliest of these burials, in Tomb 1 at Salamis, dates from no later than 750 B.C. Can we really believe that the Homeric poems were not only composed, but also familiar in Cypriot Salamis as early as this? Is it worth even considering the reverse assumption, that Homer was inspired by a new Cypriot funerary practice? There is, I think, a way out which is preferable to either of these solutions. Homeric scholars have long since advanced the suggestion that Patroklos' funeral, at least, may be a later adaptation of an account of the funeral of Achilles: this, among other things, would explain the surprising fact that Achilles takes no part in the funeral games. Does not this recent discovery confirm the belief that the epic tradition was much older than the *Iliad* itself? Greek-speakers in eighth-century

28. See V. Karageorghis, *Salamis in Cyprus, Homeric, Hellenistic and Roman* (London, 1969), 26 - 28, 31 - 32, 70 - 72, 92, 94.

*Cyprus could well have been familiar with earlier lays like that in which Achilles' funeral was first described, the ancestor of the undoubtedly later Aithiopsis; and let us not forget that, through Stasinus, Cyprus itself was to make a later contribution to the epic tradition. In the same way, another problematic feature of Homer's lifetime, the practice of painting figure-scences on Geometric pottery whose content in some cases is surely legendary, can be explained too: these are legends derived, not from the *Iliad* or *Odyssey*, but from a common store of tales, not all of them necessarily enshrined in epic form, all of them older than Homer, some of them briefly referred to in Homer but only a very few of them narrated at length by him. That is why these vase-paintings, even when they show subjects of a very distinctive kind like Siamese twins, have proved very difficult to explain as illustrations of Homer. They are not: they are the contemporary response of another kind of artist, different from that of the epic poet, to the same body of traditions.*

*The period about which I have been speaking is not the most glorious in Greece's history, but it is one of the most intriguing. It was the period which witnessed the huge changes between the Mycenaean and the Classical ages, changes which must in large part explain why the achievements of the latter period were, in the end, so much greater than those of the former. But its contribution was not, surely, a conscious one; nor was even remembered by the later Greeks of antiquity. One could well apply to the people of this period some verses of your Secretary's:*

...they never learnt  
How their eighty ships rowed  
Into the second book of the *Iliad*...

C. A. Trypanis, *The Stones of Troy:*  
"The Many"