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To Kill a Cemetery: The Athenian Kerameikos and the Early Iron Age in the Aegean

[Reflections on I. Morris, *Burial and Ancient Society: The Rise of the Greek City-State* (Cambridge, 1987) and J. Whitley, *Style and Society in Dark Age Greece: The Changing Face of a Pre-literate Society 1100–700 BC* (Cambridge, 1991)]

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Introduction

Few cemeteries have attracted, and continue to attract, the lavish care and scholarship bestowed on the Athenian Kerameikos. Since the mid-1960s, no fewer than four monographs and two long articles, not to mention shorter papers and reviews, have worked and reworked this one cemetery area to such an extent that one begins to doubt that anything in Greece has ever been excavated outside downtown Athens. Atheno-centrism is very far from dead: for Athens is, to quote one commentator, the ‘most important of Greek poleis and Dark Age sites’ (Whitley 1991: xv). Perhaps only Helen’s beauty launched more forays and expended more mortuary energy, but Helen’s beauty, like the Kerameikos, led to overkill.

The purpose of this article is to set out the history of scholarship on the graves of the Athenian Kerameikos and, from there, to review critically the two most recent offerings on the subject by Morris (1987) and Whitley (1991). Both of these works focus on the Athenian Early Iron Age, the earlier part of which is generally referred to as the ‘Dark Age’, and both are concerned with changes in the structure and symbolism observed in the archaeological record and how these changes relate to the rise of the Greek

polis. The review concentrates on a number of key issues, particularly method, chronology and the artificial construction of a 'Dark Age'. The latter is more fully addressed in the final section of this paper.

History of Scholarship

The original monographs presenting the results of the excavations in the Athenian Kerameikos, praised by one author (Whitley 1991: 69-70) for their 'enviably high standard of photography and documentation', while he condemned the theoretical perspectives of their main author, were published at regular intervals between 1939 and 1976 (Kraiker and Kübler 1939; Kübler 1943; 1954; 1959; 1970; 1976). The order of these volumes has followed roughly the sequence of the burials, with the earliest appearing first. Scholarly attention has focused on the grave plots of the earlier Iron Age, particularly those of the so-called Submycenaean, Protogeometric and Geometric periods. No volume has yet appeared on potters' activity, the very thing that gave the site its ancient name, and the fact is that kilns have been found there in quantity (Cook 1961: 64-67; Hoepfner 1976: 173-75; and annual preliminary reports in *Archäologischer Anzeiger*).

As early as 1939 the notion of 'horizontal stratigraphy' among the tombs of the Kerameikos raised its head. In his publication of the 'Submycenaean' tombs north of the Eridanos stream in the Kerameikos, also known as the Pompeion Cemetery, Kraiker (1939: 1-177) concluded that the earliest graves were those in the southwest part of the cemetery, moving outwards from what he considered to be the original course of the Eridanos, with later tombs spreading to the north and east (Kraiker 1939: 3-4). Kraiker's conclusions of chronological movement within the area of the burial ground were in no small measure dictated by the fact that what he thought were the earliest tombs were arranged in parallel rows oriented NE-SW. Similar rows, apparently even neater, were earlier uncovered in the so-called Submycenaean Arsenal cemetery on Salamis (Wide 1910: 17-36; cf. Styrenius 1962); these tombs, however, were never fully published. Kraiker (1939: 3) further suggested that the Athenian graves were the latest of a cemetery, hugging the south bank of the Eridanos, which extended from the Agora. He therefore not only perceived movement within the immediate area of the later Pompeion but attempted to link the tombs of the Agora to those of the Kerameikos. Although Kraiker's thoughts on the original course

of the Eridanos have been questioned (Krause 1975: 185), and what he took to be the flood area of the stream—which he never excavated, in the belief that it would be void of tombs—has yielded at least one tomb (Hoepfner 1976: pl. 15), he was the first to apply the concept of horizontal stratigraphy to the Kerameikos (not Hachmann [1963] as some scholars have thought—e.g. Whitley 1991: 69). The excavations of the Kerameikos grave plots were subsequently enlarged by Kübler during the war (see Schiering 1991) and excavations have continued ever since (Kübler 1943; 1954; 1959; 1970; 1976; Vierendeis 1964; Schlörb-Vierendeis 1966; von Freytag gen. Löringhoff 1974; 1975; 1976; Knigge 1966; 1976; 1984; Koenigs *et al.* 1980; Kovacsics 1990). Although Kübler had strong thoughts on the religious significance of artifacts deposited in tombs, he did not invoke the concept of horizontal stratigraphy to any great extent. Rather, he placed his faith on what he considered to be the logical principle of style and its development. Chief among Kübler's chronological indicators were imports deposited in tombs, especially the imported Corinthian pottery which allowed the cross-linking of the Attic and Corinthian pottery sequences (Brokaw 1963). Unfortunately, imports were rare in the earlier tombs of the Kerameikos.

Kübler's interpretive framework was addressed by Hachmann (1963) in his review of *Kerameikos V* (Kübler 1954), but Hachmann went further by proposing a chronology for the Kerameikos based essentially on burial customs and horizontal stratigraphy. Hachmann's primary aim was not so much to take exception to Kübler's chronological framework, but rather to question the method Kübler used in arriving at this framework. One year prior to the appearance of Hachmann's methodologically important review, Müller-Karpe (1962), the high priest of the cult of metal above all other classes of object, published his extensive study of the metal finds from the Kerameikos graves. By focusing his attention on the typological features of the metal objects deposited in tombs, Müller-Karpe proposed a chronological system largely in keeping with Hachmann's and, so far as the Submycenaean period was concerned, in almost total agreement with Kraiker, and which, if truth be known, was little different to that of Kübler.

The story, however, does not end there. While the papers by Müller-Karpe and Hachmann were hot off the press, Styrenius was gathering material for his PhD dissertation, entitled *Submycenaean Studies*, which was published in 1967. Although the subject of the dissertation was originally suggested by Gjerstad, Styrenius, implicitly or explicitly, followed in the

footsteps of that other great Swede, Furumark. Styrenius's passion was to establish Submycenaean, what Furumark (1941a) dubbed LHIIC2, as a real pan-Hellenic chronological entity. To this end, he concentrated on the pottery from the Pompeion cemetery published by Kraiker (Styrenius 1967: 24-27, 38-48). Styrenius combined stylistic evidence (that is, the differences in the decoration of the pots), with horizontal stratigraphy, in order to arrive at chronological conclusions. In the tradition of Furumark he divided Submycenaean into Early, Middle and Late—that perennial tripartite system that has served Aegean prehistory so long, if not necessarily so well. Since his approach considers not only style but also the location of the graves, Styrenius's results were very close to Kraiker's, except that he further divided Late Submycenaean into two sub-phases, A and B, and tacked on a 'transitional period' between Submycenaean and Protogeometric (1967: 151). Just like Kraiker, Styrenius considered the graves towards the south-west of the area, those in parallel rows, the earliest. He did, however, omit from his study Grave 106, as Mountjoy (1988: 24) was to point out later, because it contained a stirrup jar dated by Furumark to LHIIC1c (Styrenius 1967: 26) as the only *kterisma* (tomb offering); after all, who was he to challenge the pronouncement of the venerable Furumark? Styrenius, however, never fully trusted horizontal stratigraphy, stating that it 'is not a safe method. . . but the method was considered worth trying' (1967: 151).

Eight years later a further study, yet another PhD dissertation, this time from Hamburg, saw the re-analysis of the Kerameikos cemeteries by Krause (1975). The latter, following Hachmann, championed the cause of horizontal stratigraphy. He assigned not only the Submycenaean graves, but the succeeding Protogeometric and Geometric, into neatly defined chronological categories—*Zeitstufen*—on the basis of variables in burial custom (1975: 9-39). Variables included location of the grave, tomb type, the orientation of the deceased and a more thorough analysis of the tomb contents, including metal. Style of pottery played its part, but only as one component in a larger framework. As Whitley (1991: 69) was to remark sixteen years later, 'Krause tried to combine considerations of style, horizontal stratigraphy, burial customs and metal objects to reach what one sincerely hopes will be the last word on the subject of the relative chronology of the Kerameikos cemetery'. Although citing Mountjoy (1988) in his bibliography, Whitley neglected the implications of her chronological revisions.

Before coming to Mountjoy, however, we must return to Krause, for it is upon his pigeonholing of the Kerameikos graves that the subsequent works

by Morris (1987) and Whitley (1991) are based. Krause divided the Submycenaean through Late Geometric graves into ten *Zeitstufen*. Of these, *Zeitstufen* 1, 2a, 2b and 3 cover the 'Submycenaean' period (1975: 28-34), though *Zeitstufe* 3 is often considered as transitional Submycenaean to Protogeometric. For these phases Krause's conclusions differ little from Kraiker's. *Zeitstufe* 1 is again the same group of graves in parallel rows in the southwest part of the cemetery and Krause, following Kraiker almost to the word, states that the other graves are later because they are no longer in neat rows (1975: 29).

By the end of 1975, the combination of Desborough's syntheses of Protogeometric (1952; 1972), Coldstream's study of Geometric (1968; cf. 1977), Snodgrass's overview of the 'Dark Ages' (1971), coupled with Styrenius (1967), Krause (1975) and Kurtz and Boardman's (1971) overview of Greek burial customs—of which almost one-half was devoted to Athens and the Kerameikos—resulted, so far as Athens was concerned, in a clarity and broad consensus rarely seen in Aegean archaeology. To be sure, there was some bickering, but this was largely confined to those areas outside Athens. Desborough (1964: 17-20), for example, suggested that Attic Submycenaean was in part contemporary with Argive Late Mycenaean (LHIIIC Late). Deshayes (1966: 247, 251), the excavator of the Deiras cemetery at Argos, took exception, mainly on the basis of the Argive metal finds and tomb types, and insisted that LHIIIC Late and Submycenaean were distinct. He found a strong ally in Styrenius (1967: 127-28), another 'Argive man', but Snodgrass (1971: 28-34) chose to support Desborough's view. In a later work, Desborough (1972: 29-111) expanded his definition of Submycenaean and even spoke of a 'Submycenaean culture'. Be that as it may, who in 1975 could imagine that the same Kerameikos tombs would form the basis of not one but two new PhD dissertations, both to be published in the prestigious Cambridge series *New Studies in Archaeology*?

The intervening years, however, between Krause on the one hand and Morris and Whitley on the other, were not idle, for they saw the publication of several important papers which shook the very foundations of Krause's *Zeitstufen* and the established chronology. The first was published in 1978 and was appropriately titled 'A plea for the abandonment of the term "Submycenaean"' (Rutter 1978). Rutter audaciously suggested that LHIIIC Late (Furumark's LHIIIC1c) and Submycenaean (Furumark's LHIIIC2) were contemporary, invoking the novel argument that 'Submycenaean' was the tomb pottery of LHIIIC settlement material. Rutter's view threw

something of a spanner into the over-strict division of tombs, and his argument was of interest not least for the fact that virtually all the 'Submycenaean' material originally collected by Furumark (1941b: 77-78), including that from Athens, was found in tombs. Rutter's idea was picked up by Smithson (1982), working on the publication of the well deposits from the Athenian Agora and Akropolis north slope. Although the three 'Submycenaean' wells in question provided ample evidence of this style of pottery in a non-funerary context, Smithson considered that the deposits yielded enough information to suggest that LHIIIC Late and Submycenaean were contemporary. More accurately, she could not distinguish stylistically between the two. She further hinted, ever so laterally, that this type of pottery—whether it is termed Submycenaean, Final or Very Late Mycenaean—overlapped chronologically with early Protogeometric. For any student of Attic black- and red-figure pottery the notion of coexistent styles would pose no major intellectual or moral difficulty. Athenian black-figure pottery after all was produced for several decades while some of the so-called pioneers were painting in the new red-figure (Beazley 1951: 75-87; 1986: 69-80; Robertson 1992: 1-19), and the black-figure technique continued well into later periods for the production of Panathenaic prize amphorai (Beazley 1951: 88-100; 1986: 81-92). Yet in the overly strict linear development of style which has been a feature of ceramic studies in Aegean prehistory, such a notion would amount to iconoclasm.

From a historiographical perspective, the later 1970s witnessed an important development in Aegean Early Iron Age archaeology. Snodgrass, having established the 'reality' of a 'Dark Age' (Snodgrass 1971), turned his attention to archaeology and *The Rise of the Greek State* (1977). The sowing of this idea was, as we shall see, to bear ample fruit.

Mountjoy (1986: 194; cf. Frizell 1986: 85-86; Jacob-Felsch 1987; 1988), eager to re-establish Submycenaean to its previous glorious position, took the blade to Rutter's argument and imparted the first flesh wound. She cited what was at that time unpublished material from Mycenae and Tiryns where Submycenaean was stratified above LHIIIC Late. Two years later Mountjoy (1988: 3, figs. 1-2) published, in lavish German reproductions, the handful of scraps of pottery from Mycenae which 'proved' that LHIIIC Late and Submycenaean were distinct chronological entities. Her basic assumption was that style is pan-Hellenic, so if a distinction can be made for the Argolid it can therefore be applied elsewhere. Mountjoy wrote much on the niceties of the wavy line as opposed to the tremulous line or true zig-zag on the

fragments in question. In so doing she struck the mortal blow—or so she thought—to Rutter's argument.

Fragments are, however, as Sparkes and Talcott (1970: 2) warn, 'dangerous allies' and this was probably at the back of her mind when she dropped the blade and took up the battle axe which she wielded deftly, to great effect. Her target was, of course, the pottery from the Kerameikos graves! Mountjoy (1988: 1-33) contended that the burials originally published by Kraiker, and re-worked to the point of overkill by others, did not begin in the Submycenaean phase but, in fact, began in LHIIIC. This in itself was not immediately alarming, another case of what Whitley (1991: 10) would later call a chronological 'quibble rather than debate'. But Mountjoy's conclusions tore apart the neat rows of the earliest, previously labelled 'Submycenaean', graves, with the result that what she considered LHIIIC Late graves were scattered throughout the Pompeion cemetery. The axe fell heaviest on Krause's *Zeitstufen*, with the earliest tombs now spread throughout *Zeitstufen* 1, 2a and 2b, though the much smaller *Zeitstufe* 3, often considered transitional Submycenaean to Protogeometric, was spared violation (Mountjoy 1988: figs. 21-22). It should be stressed that Mountjoy's conclusions were formed on the basis of pottery style and style alone. As far as I know her conclusions have not been challenged. If history is any judge, there is more than enough room here for yet another dissertation or two. Since Morris's (1987) seminal work appeared a year before Mountjoy's revision, he could not have anticipated the argument. For Whitley (1991) it was obviously a 'quibble' not to be taken seriously.

Thanatos Revised

The appearance in 1987 of Morris's *Burial and Ancient Society* created something of a furore, so far as established notions among classical archaeologists were concerned. Not least among the casualties was Camp's 'drought theory' (Camp 1979: 397-411) and Snodgrass's idea of depopulation at the beginning of the Early Iron Age and the sudden increase in Late Geometric on the basis of total numbers of graves (Snodgrass 1977: 10-18; 1980: 22-24; cf. Snodgrass 1983; also Wells 1992: 186-87). Morris's work can only be described as bold and innovative; its scope far-ranging; and his depth of knowledge profound. He casts his net wide into social, linguistic and anthropological theory, and far beyond the geographical confines of the

Aegean, in order to focus on the changing relationships between burial and social structure in Early Iron Age Athens. His time frame, ca. 1125–500 BC (1987: 11, Table 1) incorporates not quite the entire chronological span of the Kerameikos cemeteries, but almost so. He includes the evidence of graves elsewhere in Athens and Attika, but throughout it is the Kerameikos that forms the sturdy foundations of the colossus. By employing an ingenious set of calculations, Morris extracts from the mortuary record, as if blood from the stone, what a traditional approach could not even begin to fathom. His central argument is that the ideology of the Greek city-state, the *polis*, can be identified in changes in the *structure* of the archaeological record. Though he musters what support he can from settlement contexts, his thesis is by necessity based on mortuary data. The linchpin of his theoretical framework is Saxe's (1970: 119) hypothesis 8, revitalized and reformulated by Goldstein (1976; 1981), and enjoying the papal stamp of approval (Binford 1971: 6–29).

The Saxe/Goldstein hypothesis, sometimes referred to as the Binford/Saxe hypothesis (e.g. Metcalf and Huntington 1991: 16), has not met with universal approval. By the late 1970s and into the 1980s, a number of archaeologists expressed their concerns. Randsborg (1980; cf. 1981), for example, argued that in certain periods of Danish prehistory mortuary remains reflect social ranking in life, whereas in others they do not. The strongest criticisms have, naturally enough, come from the post-processual quarter (Hodder 1980; 1982: 196–99; 1984: 53; 1991: 24–25; Shanks and Tilley 1987: 43–44). Some of these criticisms are addressed by Morris (1991: 148–50), in a paper strongly defending the Saxe/Goldstein hypothesis. Similar concerns were also expressed by social anthropologists. Bloch (1981), by looking to the Merina and Sakalava of Madagascar, doubted whether archaeologists could correctly reconstruct living social systems from funerary remains; he tempered his pessimism by recommending a diachronic approach to the problem, as was indeed adopted by Morris (1987). Okely (1979: 87), in her study of the Gypsies in Britain, warned that mortuary practices may disguise or invert the identity, social structure and ideology of the living, while similarly Jacobson-Widding (1988), in her comparison of Swedish and African funerals, convincingly showed that the normal social structure and ideology in life could be inverted or transformed into its opposite during funerary rituals. Most recently, Metcalf (Metcalf and Huntington 1991: 16), in dealing with the 'Binford/Saxe' hypotheses, speaks of the series of 'slippery subsidiary assumptions' involved.

Though not entirely unknown in Aegean archaeology (e.g. Pullen 1985; 1990; Wright 1987; Dabney and Wright 1990; Nordquist 1990), the names of Saxe and Goldstein can hardly be described as household words. The slightest hint, however, of Binford's name is enough to invoke a highly emotional response from any self-respecting classical archaeologist, particularly after the publication in an English translation of Courbin's full frontal assault on the 'new archeology', amusingly prefaced by Bahn (1988: xvii-xx). Perhaps it is this, more than the mathematical acrobatics, that has resulted in a certain resistance to Morris's thesis among the fraternity of more traditional classical archaeologists. Much of what has been written against Morris is unfair and in some cases it is clear that some of his colleagues have launched into an attack without reading the book (e.g. Wells 1990: 125), or simply not understanding it (e.g. Berlin 1990: 311-12). Perhaps what has met the greatest resistance, implied if not explicitly stated by classical archaeologists, is Morris's notion of archaeologically invisible burials. That is to say, that in the political vicissitudes of the 'Submycenaean' through 'Early Red Figure' periods—to use Morris's terminology—his *agathoi*, a group which includes both aristocrats and wealthy but non-governing peasants, received formal burial, whereas in two quantifiable periods (Protogeometric–Middle Geometric and again in Protoattic–Black Figure) his *kakoi*, the poor, were excluded from formal burial on the basis of social rank (Morris 1987: 95-96 and ch. 6).

I, for one, would welcome an illuminating ethnographic analogy for the schematic representation of the hypothesized changes in the membership of adult grave plots and cemeteries in Athens between ca. 1100 and 500 BC vividly rendered in Morris's fig. 29 (p. 95). Within this model, all members of the descent group bury together during 'Submycenaean', 'Late Geometric', and 'Early Red Figure', whereas in 'Protogeometric–Middle Geometric' and 'Protoattic–Black Figure' (these two periods being separated by 'Late Geometric') only the *agathoi* are given formal burial. What seems odd, at least on first impression, about this model is not the argument of exclusion and archaeologically invisible burials, which are facts established for many cultures, but rather the 'toing-and-froing' in time of exclusion to formal burial of part of the community—that is to say, the blatant reversals of the fate of Morris's *kakoi*, who at one time receive formal burial, then they do not, then they do, then they do not and finally they do again. Although, as Morris persuasively argues, this may well be the result of intense struggle within a highly stratified community, it may equally be the

result of the way one measures structure, in this case mortuary variability, in the archaeological record (see below). Similar concerns have been expressed by other reviewers; for example, D'Agostino and D'Onofrio (1993: 48) state: 'it is difficult to imagine that a cultural trait such as selection according to rank could have vanished, to reappear again after fifty years. There must be some other explanation to this oscillation.' Similarly, Garland (1989: 66) notes: 'The unconvincing seesawing movement which Morris proposes in connection with those who now are/now are not entitled to formal burial in Attica becomes completely untenable as a model for all Greek city-states. . . '

Be that as it may, there is nothing new in the idea of archaeologically invisible burial. Within an Aegean perspective it was first intimated by Snodgrass (1980: 21; also 1977: 12), though quickly dismissed by him; nor is classical literature void of the notion of the unburied, the *ataphoi* (Garland 1985: 103, 165). Moreover, it is now generally agreed that Athenians of the classical period did not raise all their children, but exposed at least some to the elements. The question of demography and exposure of children—infanticide—is well treated by a number of classicists, particularly Golden (1981), whose work appears to have escaped more general notice. It would appear (although this is not made explicit) that Morris drew on Atkinson (1968) for the idea of exclusion. In dealing with Neolithic England, Atkinson convincingly deduced that only a selected part of the population received formal burial in chambered tombs and that some, if not the majority, of the dead were disposed of in ways which have left no archaeological trace. Atkinson's idea, itself supported by some adroit calculations, was summarized by Ucko (1969: 269) in an important paper often referred to by Morris.

It is not my intention to deconstruct this notion simply because I find much of value in it, nor do I wish to offer a full critique of Morris's important work, much of which I admire. Rather, I wish to express my concern, as an Aegean archaeologist, on essentially two points: (1) chronology, and (2) the artificial construction of a Dark Age.

The second of these concerns, I will come to later; the first is not merely confined to the question of overlapping styles, as was first expressed by Rutter (1978) and Smithson (1982). To his credit, Morris anticipated such problems and attempted to come to grips with them early on in his text (1987: 15, with fig. 4). My primary chronological concern is with method. On p. 117 Morris states:

The primary aim of the analysis. . . is to demonstrate changes through time. The study of temporal variation is, after all, the greatest strength of archaeology; but diachronic approaches pose particular problems for the analysis of burials. Burial custom changes through time, often rapidly, and ancient Greece was no exception in this. The changes themselves, and particularly the rate of change, can be of great interest, but various difficulties arise. *We must not confuse diachronic change with synchronic differentiation. To do so would be disastrous.* Fortunately, the relative chronology is very fine grained at Athens. [My emphasis]

Without even invoking the controversial challenges to established Athenian chronology by James *et al.* (1991), or by Francis and Vickers (1983; Vickers 1987; Francis 1990) for the later periods, it is clear from the discussion above of the so-called 'Submycenaean' phase that the relative chronology of Athens is far from clear, let alone 'fine grained', and I believe similar chronological problems exist throughout the Early Iron Age (for similar chronological problems pertaining to the archaic phases see D'Agostino and D'Onofrio 1993). But this is not the main issue. My main concern is mortuary variability and its measurement. Fundamental to the notion of 'horizontal stratigraphy' as perceived by Hachmann (1963), Styrenius (1967) and Krause (1975) is the assumption that variations in mortuary custom are the result of *diachronic change*, which lead, therefore, to chronological conclusions (see also the earlier work of Clark 1960: 130). Although Ucko (1969: 276-77) had provided a number of ethnographic nightmares illustrating the potential shortcomings of horizontal stratigraphy, it was this that nevertheless formed the basis of Krause's study of the Kerameikos. For Krause, a number of tombs sharing the same variables were contemporary; that is to say, it was the variables themselves and how they clustered that formed the basis for measuring diachronic change.

Morris, though accepting Krause's chronology, used essentially the same variables to establish *synchronic differentiation*, which, he argues, is based on the *status* of the deceased in life. The result is a frightfully circular argument. The problem is straightforward: can we attribute observed variation within a cemetery or grave plot to diachronic changes in symbolism or structure on the one hand, or to a conscious or unconscious status differentiation on the other? Surely, in order to argue the latter, as Morris does, one cannot first use mortuary variation to establish changes through time. Morris's attitude to chronology and variability is perhaps best seen diagrammatically in his variability scores for the Kerameikos during Krause's *Zeistufen* 1-10 and the succeeding Archaic phases 1-8 (1987: 116, fig. 36). Although his V-scores, as is clearly stated on p. 115, might be seen

to conform to his expectations of the exclusion hypothesis, there are nevertheless some alarming peaks in the raw data. Notable among these is a peak in the Protogeometric *Zeitstufe* 5 and again in Archaic phase 3 (cf. D'Agostino and D'Onofrio 1993: 49-50). In both of these phases the *V*-scores were as high or higher than those of the Submycenaean phases. If, however, these periods were to conform to the expectations of his exclusion hypothesis, the *V*-scores should have been significantly lower. The increase in variability in *Zeitstufe* 5 is said to be (p. 121) 'probably due to the great length of the phase (perhaps more than 75 years)'. No such length of time, however, characterizes Archaic phase 3, which Morris dates with some precision to ca. 630/625-610 BC; the surprisingly high *V*-score here is merely seen as a temporary peak only (p. 130). The whole problem is then 'solved' by applying a moving 2-period and a moving 3-period average which flattens out the high and low peaks of these problem phases, but at the same time makes a mockery of his statement that the relative chronology at Athens is very fine grained.

It is not insignificant that Saxe's (1971) published case study of his hypothesis that an individual's treatment at death is a reflection of the position occupied in a status system in life was based on data collected from a Mesolithic occupation and burial site in the northern Sudan. Similarly, Goldstein (1981), in her study of the spatial components of cemeteries with its refurbished Saxian hypothesis, chose two Mississippian cemeteries in the lower Illinois river valley in west-central Illinois, both dated to ca. AD 1100-1150. The first of these, Schild, was a large cemetery of about 300 individuals, the other, Moss, a smaller cemetery of about 50 souls (Goldstein 1981: 59). In both of their independent studies, Saxe and Goldstein were careful in choosing cemeteries where diachronic changes in symbolism or structure were reduced to a minimum, thus enabling them to embrace status differentiation.

Essential to the Saxe/Goldstein hypothesis is the 'use and/or control [of] crucial but restricted resources' (Saxe 1970: 119), and more specifically the 'critical resource' passing from parent to offspring (Goldstein 1981: 61). The logic linking burials and property transmission was recently re-examined by Morris (1991; cf. 1992: 22-23), and is a topic, so far as the classical literary sources are concerned, well covered by, among others, Alexiou (1974). The crucial resource assumed by Morris is land. Similarly Whitley (1991: 192) visualizes Early Iron Age Athens as predominantly agricultural. Though I do not doubt the strong agricultural base of early

Greek society, I find this view a little too restrictive as it does not consider viable alternative resources, such as metal, be it the control of the resource itself or its distribution. Nor does this view consider the important role played by the enterprising merchant—Greek or Phoenician—nor the reality of ‘oriental’ immigrants to the Aegean, the progenitors of the later metics of Athens and the Piraeus, who played their part in the formation of Early Iron Age Greek society, not least in the adoption of the alphabet (Burkert 1984; Coldstream 1988; Stampolidis 1990a; 1990b; S.P. Morris 1992; Wells 1992: 188).

Other reviewers of Morris have expressed their concerns on quite different matters. Bradley (1988), in a favourable review, wished to see more on other aspects of contemporary culture, since he believes that any explanation for the emergence of the *polis* involves much broader issues. It is perhaps worth noting here an old argument, well entrenched by the earlier 20th century but generally overlooked by more recent commentators, that the *polis* grew most quickly in Asiatic Greece, ‘since the new settlers needed protection against the natives: but in old Greece also protection against neighbours or pirates was necessary’ (Cook 1946: 88, with references to earlier works). In a more daring review, Humphreys (1990: 263-64) argued that the Saxe/Goldstein hypothesis is irrelevant to Athens, since classical burial groups tended to be bilateral kindreds. Humphreys also argued that the urge to identify kin groups in cemeteries is a product of the history of burial practices from the early nineteenth century in the West. Both of these points were set out and defended by Morris (1991: 158) on the basis of the Greek literary sources.

In yet another review, Bintliff begins by stressing that Morris’s study is not on the Greek city-state, but the Athenian, correctly stating that ‘there is no treatment in depth of the remaining Greek world’ (Bintliff 1992: 287; cf. Antonaccio 1989: 296-97; Wells 1992: 187). Bintliff takes this argument one step further by claiming that the Athenian model, with its extreme case of citizen democracy where all but the slaves and foreigners had full civic rights, was the exception, not the rule (Bintliff 1992: 287-88). He notes that throughout the rest of the Greek world the shift to communal burial in archaic times occurs without an equivalent rise in general citizen franchise and goes on to state (p. 288):

Indeed it is a glaring omission of Morris’ analysis that he fails to mention that in almost all states apart from Athens, for all or most of Greek history, full citizen rights were confined to the aristocratic (knightly) class and the wealthier farmers

(the hoplite class). It is thus quite erroneous for him to equate the rise of the Greek *polis* with a classless society.

Bintliff further notes the parallel developments in state formation in Early Iron Age Etruria, Phoenicia and Hallstatt Europe and draws attention to Morris's mistaken but conventional view of the uniqueness of the Greek political experience. A demographer at heart, Bintliff (1992: 289) cannot accept Morris's relegation of the widely assumed cycle of rapid growth in the 8th century, followed by a decline in population in the 7th, as an archaeological mirage. He restates the validity of the cycle and notes that all available data still point to 'Dark Age' population being a fraction of Mycenaean (cf. Viviers 1989: 447). As we have seen, it was Snodgrass who proposed the sudden increase in population in the 8th century on the basis first of the observed increase in the quantity of pottery preserved and then of the total number of graves (see above). It should be stressed that the demography of Early Iron Age Greece has suffered by being largely dependent on the evidence of mortuary remains. In a recent study, Green (1990) has convincingly argued for a population increase during the second half of the 8th century BC on the basis of settlement evidence from the most fully excavated Geometric settlement site in the Aegean, Zagora (cf. Cambitoglou *et al.* 1971; 1988).

In a more recent review D'Agostino and D'Onofrio (1993) draw attention to Morris's unsystematic use of the literary sources (cf. Garland 1989) and go on to question the validity of some of his statistical analyses, particularly with regard to mortuary variability. More significantly, they seriously question Morris's data set of Athenian and Attic tombs, and his criteria for placing any given tomb, or groups of tombs, in a specific chronological phase. By thoroughly reviewing the data set for tombs of the archaic period, D'Agostino and D'Onofrio provide numerous examples of uncertain chronological subdivisions, omissions and misinterpretations throughout Morris's classification of the evidence.

Both of the above-stated concerns hold equally true of Whitley's recent work (1991). Following in the tradition of Styrenius and Krause, Whitley examines the relationship of pot style and changes in mortuary practice and goes further by attempting to relate them to social developments in what he, and Morris, perceive as the 'Dark Age'. To this end the by now tired Kerameikos tombs are taken out of mothballs and marshalled for one more parade. Pot shape, painted decoration, metal artifacts, grave architecture, and age and sex of the deceased all get an airing. They are, however,

rejuvenated, as if resuscitated with an artificial lung, by the computer and those wondrous FACTOR and CLUSTAN analyses, the latter of which never quite worked for Morris (1987: 112). Whitley's methodology is so sensitive that the earliest figured representations, dating to Protogeometric—surely significant symbols in his theoretical framework—do not appear anywhere; neither do Protogeometric and Geometric potters' marks. One is tempted to ask whether Whitley ever consulted the artifacts in question in the flesh; or worked exclusively from photographs.

The careful reader will, of course, note a minor problem in the selection of tombs to be analyzed. On p. 88, for example, Whitley notes that there are, in his estimate, over 204 Submycenaean graves, of which only 117 were included in his sample. On what basis the remainder were excluded remains a moot point, except that Whitley does state (p. 88) that 'some of these graves are in too poor a state to merit inclusion'. Similarly for the Protogeometric period, only a sample of 93 out of 195 known Athenian graves was incorporated in his study (p. 103). For the Early Geometric to Middle Geometric I period we are given a run-down of the various grave plots and cemeteries in Athens and then told (p. 120): 'Of these graves, all those from the larger cemeteries . . . have all been included in the computer analysis,' as were some of the graves in the smaller cemeteries and grave plots. Following this, Whitley states (p. 120): 'It may seem paradoxical to state that though a greater percentage of the visible graves have been included in the analysis for this period, the sample is both less representative and smaller in absolute terms than the sample from the preceding period.' This is gross manipulation of the data; the actual number of Early Geometric to Middle Geometric I graves is never stated, nor are the reasons why those graves not included were jettisoned. Here, also, there is a minor chronological concern. Whitley, unlike Morris, does not over-define the various periods and phases; rather, he puts various phases together to form periods, such as 'Submycenaean', 'Protogeometric', 'Early Geometric to Middle Geometric I', and so on. His reasons for dividing the Athenian Early Iron Age into such periods are not always convincing. Why not put, for example, 'Late Protogeometric' with 'Early Geometric' (for which there exists a good deal of stylistic overlap), or treat them separately; why should 'Middle Geometric I' be separated from 'Middle Geometric II', and so on? Again, this amounts to an unfortunate manipulation of the data, since the absolute number of graves in any assigned or re-assigned period would be different, as would the overall results of the analysis.

On pages 69-70 Whitley lets his hair down and launches into a fully frontal nationalistic attack of Greek, American and German scholarship in the study of burials, style and society in Athens. Following a copious review of mortuary practice and social form (already treated by Morris), personal musings on the *polis* (a term which is only really defined on p. 194) and on Geometric art in general, misconceptions on the impact of Orientalization (p. 45; see now Burkert 1984; S.P. Morris 1989b; 1990; 1992), and that evergreen historical background to Athens and Attika to which we have been subjected untold times before, one is bombarded by rigid Motive lists, tables of the results of FACTOR analyses and CLUSTAN dendograms in their full glory. Between pages 156 and 157 some 39 plates of pottery interspersed with tombs—all old favourites readily available in most textbooks—alleviate the *ennui*, but we quickly return to the tables and dendograms. Given the state of the world's rainforests, this data might well have been microfilmed, summarized or referenced (as was done by Morris).

What follows is even more mind-boggling: ch. 6 (pp. 181-94) is headed 'The Wider Dark Age World'. Here we are again subjected to the Athenian sequence, this time a merciful summary that could easily have replaced the more verbose treatment earlier on, and we are told about 'other regions' of Greece, namely Argos and Knossos. About 90 per cent of the Greek mainland is omitted, as is every island of the Aegean and Ionian seas, not to mention the rest of Crete. The chapter ends with an ethnographic analogy of a most alarming sort. Whitley perceives a striking correspondence between Nuristan and ninth-century Athens. He writes (pp. 192-93):

Both of these societies have 'geometric', non-figurative art styles. More significantly, there exists in Nuristan a system of social rationing in the use of symbolic tokens, a maintenance of the exclusivity of certain items and motifs and a strict regulation of symbols according to achieved rank. The symbolic system is enforced, and the social order maintained, by vigilant policing. . . . In these communities, decoration has a direct relationship to status.

No doubt anticipating the rejoinder that such decoration is found on Early Iron Age Aegean pottery over a wide area, perhaps making it unwise to assume that such a simple comprehensive social model could be applied to the rest of Greece, Whitley writes (p. 193):

Only in Athens during the ninth century was society organised on the principle of social rationing and the exclusive access of certain groups to exotica, particular symbolic tokens, and the right of formal burial. Social rationing was clearly a rarity in Dark Age Greece.

There the argument is left dangling, unsubstantiated, except that Lefkandi is thrown in, a site where, he states, social rationing was noticeably absent. Never once are we told on what *archaeological* evidence such generalizations are based; and this time about 99 per cent of Greece and the Aegean is omitted from the argument. Further on we return to 'the rest of Greece', namely Argos and Knossos, because they, like Athens, developed into *poleis*. Why these three settlements are singled out we are left wondering. As for 9th-century Athens and Nuristan, the analogy is all well and good if Athens lived and functioned in a vacuum unto itself. Implicit in the analogy is the assumption that the symbols of achieved social rank, in this case the decoration on Athenian pottery, were produced by the potters (the artisan class, if the analogy is fully invoked), specifically for the use of members of clans or lineages which maintained separate grave plots. We are never told who, exactly, these exalted individuals were, except that they were 'patri-lineal, patriarchal and predominantly agricultural' (p. 192), and certainly not of the lower class(es). Again, one would appreciate a modicum of archaeological evidence to back up such pronouncements. What has obviously never crossed Whitley's field of vision is the fact that Athenian Protogeometric and Geometric pottery, including that specifically of the 9th century BC, is found in quantity in, among other regions, the Peloponnese, central Greece, Thessaly, coastal northern Greece, Crete, the Cyclades, the west coast of Asia Minor, Cyprus and coastal Syria and Palestine. The fact that Athenian pottery production may have been in no small measure directed towards an active export trade and not restricted to use by the Athenian 'upper classes' is nowhere considered. Similarly far-flung is Euboian pottery, which is also found in quantity in Italy, particularly at Pithekoussai.

The full thrust of Whitley's study is most succinctly given in his conclusions (ch. 7, pp. 195-98). As implications for the study of the history of early Greek art he states (p. 196):

the distinct preference for particular vase shapes, particular decorative elements and a particular stylistic logic in arrangement of motif can clearly be seen, such that specific motifs, representational and non-representational alike, may be said to have a specific social reference, a specific meaning. I have argued that it is when such social preferences are at their strongest, and when the artist has the least artistic freedom, that this art is at its most successful, at its most aesthetically pleasing... A purely iconographic approach is misplaced, since it always tends to treat every element as representational, and imparts inappropriate *value judgements* derived from an acquaintance with later Greek art. [My emphasis]

Indeed, a purely iconographic approach may well entail a certain amount of value judgment; but to espouse Whitley's notion—namely, that times when social preferences are at their strongest and when artistic freedom is at its least are also times when art is most *aesthetically pleasing*—is surely a value judgment in itself. To whom is 9th-century Athenian pottery aesthetically pleasing? To the contemporary 9th-century Athenian (including those excluded from formal burial)? To the bourgeois academic of the later 20th century AD? Or to everybody?

The final section on 'historical conclusions' arranges the results in seven neat points. I would like to address only two of these. The fifth point states (p. 198):

A major social change occurred between the 10th and 9th centuries. Stable settlements become more important. Between the 10th and 9th centuries Athens underwent a transition from a relatively egalitarian to a more hierarchial form of society, whose organising principle was the social rationing of valued tokens, exotic artefacts, certain decorative features, and the right of formal burial. . . . A good analogy of this type of society may be found in the Nuristan communities described by Jones (1974). Analogous, but not identical, forms of social re-definition took place in Argos and Knossos.

Here Whitley's account is purely descriptive. We are presented with a major social change, but not once is any explanatory thesis suggested. What is left essentially amounts to a stylistic change in pottery accompanied by differences in burial custom, already well known from more traditional approaches; it lacks explanatory power. In a similar vein, the Nuristan analogy—the bulwark of Whitley's reasoning (pp. 11, 198)—though enlightening, fails to explain the causes of the Athenian change. Moreover, the period in which Whitley visualizes the change, the 9th century BC, is not quite in synchronization with the period in which Morris sees a change, namely the Protogeometric period, traditionally beginning in the 11th century BC and again in the 8th. On a more minor, perhaps pedantic, chronological point, Morris (p. 11, table 1) assigns some 75 years to the Submycenaean period on the basis of the work of Desborough (1952; 1964; 1972) and others, whereas Whitley, citing the same venerable sources, gives no more than 50 years to the same period. As contemporaries at Cambridge, sharing the same supervisor, and both dealing with Early Iron Age Athens, not to mention the Kerameikos, it is unfortunate that Morris and Whitley were not a little more careful with their chronology.

In many ways point seven comes as something of a surprise and a rather limp finale to the book; it states (p. 198):

The diversity of Late Geometric and Archaic Greece is a fact which has to be more widely acknowledged. The term 'Archaic Greek society' does not denote a uniform mass. Rather it encompasses a great variety of social forms, habits and institutions.

I seem to recall my ancient history teacher in high school going to great lengths in order to impress upon us the fundamental social, economic and political differences between, say, archaic Athens and Sparta. Indeed, the diversity of the latter two states has been a feature, if not a preoccupation, of ancient historians and classicists alike.

The essential problem of *Style and Society in Dark Age Greece* is that the work was originally conceived of as a comparison of Athens and Knossos between 1100 and 700 BC (Whitley 1986). In revising the original PhD thesis for publication, Whitley excluded the analysis of Knossos on the grounds of instantaneous obsolescence in view of the imminent publication of the North Cemetery at Knossos (Whitley 1991: xv). In the author's own words 'the omission of Knossos is unfortunate, since the work loses most of its comparative force'. Even without the North Cemetery, the database for Knossos was already substantial (e.g. Brock 1957; cf. Coldstream 1991) and the reasons for recasting the work as a purely Athenian study are not at all convincing. The monograph as it stands is a hastily constructed patchwork that could easily have been condensed into a nice article. The blame, however, should not totally rest with the author, for in this day and age academic tenure requires a book or two. The blame in part rests on the shoulders of the readers and publishers of the work: does every Cambridge thesis need to be published in full?

Discussion and Conclusion

As the foregoing paragraphs perhaps hint, although there are, on first impression, similarities between the works of Morris and Whitley, there are also great differences. Throughout his text Morris marshals the literary evidence. Indeed, his judicious intertwining of this evidence has made his work far more accessible to ancient historians and this, among many, is one of its great strengths (though some reviewers see Morris's use of the literary

evidence as unsystematic, even weak or ephemeral [D'Agostino and D'Onofrio 1993; cf. Garland 1989]). Morris states his case thus (p. 44):

the literary sources are studied as 'unintended evidence'. That is, they are scrutinized not for what their authors wished to say, but for the unarticulated assumptions they carry with them.

In contrast, Whitley eschews the literary evidence (p. xv):

It is a mistake to believe that Dark Age society can be reconstructed from literary sources . . . since these project an idealized, ahistorical 'heroic' past, a poetic amalgam of the Bronze and Iron Ages.

The one thing on which Morris and Whitley strongly concur is the existence of a *Dark Age* and the fact that it is during this period that the seeds of the Greek *polis* were first planted. Here, the guiding hand of their mentor, Anthony Snodgrass, the guru of the Greek Dark Age, lurks behind the scenes. The fundamental weakness in both Morris's and Whitley's theses lies in the assumption—approaching religious belief—that the demise of the Mycenaean way of life heralds a clean, almost surgical break; that what went before and what follows are two very different entities. In this, one can detect more than just paternal influence, for the Dark Age is, and has been, a tenet, staunchly upheld in the traditions of both Aegean prehistory and classical archaeology, forming a great divide. Most surveys of 'Greek' material culture begin with Protogeometric pottery as the 'seeds of Greek art' (e.g. Robertson 1975: 14-33). Protogeometric is thought to witness the first glimmers of an 'awakening' following the bleakness—the deep sleep—of Submycenaean. It is here we find the initial steps along the path which ultimately leads, via the Late Geometric 'renaissance' (Snodgrass 1971: 416-36; 1983; Hägg 1983), to the Classical Moment.

Because this Dark Age does not readily belong, for reasons difficult to fathom, in the intellectual realm of the prehistorian nor, largely on account of the lack of contemporary literary sources, in that of the classical archaeologist, it floats—rather uncomfortably—in between, fair game for anyone in search of a dissertation (or two). For the classical archaeologist, it is still viewed as the beginnings of something distinctly Hellenic, even though the decipherment of Linear B, forty years ago, should have revolutionized the teaching of early Greek history (Chadwick 1984: 1-14). For the prehistorian on the other side of the iron curtain, the great Bronze Age palaces are destroyed and the advent of iron (the latter a popular subject in its own right) heralds a convenient, if artificial, stopping point. Deonna

(1926: 321, 371; cf. Carpenter 1962: 28), perhaps more so than others, viewed developments in the Early Iron Age, particularly those in the figurative arts, as a spontaneous self-generation. For him, the Bronze Age represented a 'mirage égéen'. What *is* a mirage is the Dark Age and the deliberate distance maintained between the second millennium and the culture of classical Greece (S.P. Morris 1989b: 48). The opening paragraph of Morris's text states (p. 1):

This book is about the rise of the Greek city-state, one of the most remarkable social formations in history. The city-state, what the Greeks called the polis, was the world's first political community; it was also the first true slave society, and within it the concept of the freedom of the citizen was first articulated. Using a combination of archaeological and literary evidence, I will argue that this uniquely important state form appeared in the eighth century B.C., as a result of intense struggle within communities which were already highly stratified.

There are, it would appear, a remarkable number of 'firsts' in a period which the author continually refers to as the 'Dark Age'; a period that has no pedigree, a period which appears out of nowhere. In inimitable form, Whitley (p. xv) does not mince his words:

We understand so little about the kind of society that existed in the Dark Ages, the kind of society from which the *polis* emerged, that any attempt to elucidate social change must be welcomed.

Elsewhere (p. 9), Whitley's argument is even more clearly stated:

For the Aegean is a region where state formation, where the 'emergence of civilisation', happened twice: the first took place in the Bronze Age, and led to the Palace civilisations of Minoan Crete and Mycenaean Greece; the second took place in the Early Iron Age, and led to the civilisation of Archaic and Classical Greece, the civilisation of the *polis*.

Both Morris and Whitley turn their backs on anything prior to ca. 1125–1100 BC, categorically assuming that any developments in the Late Bronze Age bear no relation or had no effect on what was to follow. Surely the developments seen in, and the collapse of, an advanced social system such as the Mycenaean in the period immediately prior to 'Submycenaean' is worthy of *some* consideration? And should have left *some* trace on the structure of the archaeological record? Perhaps it may even have had something to do with state formation—the rise of the *polis*—or its shortcomings? Indeed, a number of scholars have argued that a myriad of archaic and classical Greek institutions, not only in religion and poetry, but also other traditions,

including the origins of the *polis*, are attested in Minoan and Mycenaean material culture, with considerable continuity from the Bronze to the Early Iron Age (e.g. van Effenterre 1985; cf. 1991; Hooker 1988; S.P. Morris 1989a; 1989b; 1992; various papers in Musti *et al.* 1991; Deger-Jalkotzy 1991; Muhly 1992).

Such voices, it would appear, have not won general acceptance, but their arguments deserve to be more fully addressed, not swept under the Dark Age carpet. If, as Morris (p. 117) so eloquently states, the study of temporal variation is the greatest strength of archaeology, why use 'Submycenaean' as an historical backdrop and not the later Bronze Age, particularly if LHIIIC and Submycenaean, as some scholars have argued, overlap chronologically? In this respect, Athens would permit a classic case study on account of the Mycenaean tombs excavated and fully published from the area of the later Athenian Agora (Immerwahr 1971), an area often referred to in classical literature as the 'Kerameikos' or 'Ceramic Agora' (e.g. Pausanias I, *passim*), and only several hundred metres from the overkilled 'Kerameikos' graves. A cursory glance at the distribution of Mycenaean tombs in the area of the later Athenian Agora (Immerwahr 1971: pl. 91)—including chamber, pit, cist and other grave types—and the distribution of the Submycenaean through Geometric graves (Brann 1962: pl. 45), shows quite some overlap. This is particularly true for the northeast corner of the Agora, in the area partly under the later Stoa of Attalos, and only about 50m south of the Eridanos. Athens is only one of many Early Iron Age Greek sites which has formal disposal areas exclusively for the deceased. At the same time, it is one of the few sites in Greece where the same area, or part of it, is also used for Late Bronze Age burials. Surely this is of some significance in the study of corporate group structure and changes through time? Yet both Morris and Whitley blatantly neglect the Late Bronze Age graves.

The final sentence in Whitley's book reads (p. 198): 'Greek civilisation did not, like the goddess Athena, spring fully armed out the head of Zeus.' Indeed it did not. 'Greek civilization', and more particularly the rise of the *polis*, might be better served and understood if archaeologists of various persuasions turned their attention not only to a wider geographical area encompassing the greater Greek world—not just Athens—but to a broader time scale, incorporating state formation in the Bronze Age. The *polis*, like Greek civilization, would certainly be better served if the spectre of the Dark Age, a phantom that has haunted the 'musty confines of Cambridge'

(Schiffer 1990) for too long, is finally laid to rest. As Snodgrass (1971: 1-10; cf. Whitley 1991: 5) has shown, the Greeks themselves knew of no Dark Age. Why not trust their better judgment?

Acknowledgments and Apology

The main lines of the review of Morris (1987) were jotted down in 1988, soon after its appearance; at that time I thought it best not to publish the review, but rather to enjoy it in private. Five years later I still share my original conviction. The main lines of the review of Whitley (1991) were noted down on a sleepless summer night in a hot, nameless, Thessaloniki hotel; these musings I also savoured in private. Responsibility for cajoling this paper out of me rests squarely on the shoulders of A. Bernard Knapp, aided and abetted by John Cherry. Both editors were supported, directly and indirectly, by my wife, Sarah Morris. Certain sections of this paper, particularly those dealing with James Whitley, may seem harsh; but they were written in the knowledge that the editors of *JMA* would actively pursue written responses. I am grateful to both Ian Morris and James Whitley for bringing to my attention many aspects of the Aegean Early Iron Age that would otherwise never have crossed my field of vision.

I have not, except in passing, made reference to Ian Morris's latest offering (1992). It is an innovative, thorough and engaging work which should be read by all students of classical antiquity. The evidence from the Athenian Kerameikos is there, but it is sparingly used to great effect (especially pp. 78-80, 111-18, 130-36). The 'Bibliographical Essay' (pp. 205-210) will serve many and I wholeheartedly concur with its closing sentence (p. 210)!

As the foregoing pages have intimated, and the wealth of monographs and articles published validated, little could be said were it not for the meticulous care and promptness with which the German excavators both dug the Kerameikos and published their results. It is a tribute to Kübler especially, but also to Kraiker and those that have followed, that their work has provided so much fuel for discussion; all the more so, considering *when* Kübler and Kraiker worked. If only all sites were so well treated.

Author's Biography

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To Kill a Cemetery 199

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