Archaeology, Standards of Living, and Greek Economic History

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Greek economic historians work mainly with texts, which were mostly produced and read in contexts that have few close parallels in other parts of the ancient Mediterranean world. There were certainly similarities between the literary cultures of Athens and Rome, but the differences often seem even more striking (compare Ober [1998] and Fantham [1994]). The Near East and Egypt were too varied for us to make the kind of sweeping generalizations that Finley often favored (see Liverani and Bedford, Chapters 2 and 3, respectively), but even a casual review of scholarship on cultures of reading and writing indicates major differences from both Athens and Rome (for example, Bottéro 1992; Roccati 1997; Berlev 1997). The palaces, temples, scribal schools, and private trading firms that provided the contexts of production and consumption for much of the extant Near Eastern and Egyptian textual records are very different from the legal and aristocratic settings so important for Athenian and Roman literary activity. These contrasts between literary cultures pose real problems for historians interested in systematic economic comparisons across regions and through time. Demosthenes' twists and turns lead us into a demimonde of shifty moneylenders and swindling guardians, with few obvious points of contact with the mountains of paperwork with which Zenon or the Murashûs buttressed their enterprises (see Manning and Bedford in this volume). Small wonder, then, that Hellenists have imagined the ancient economy in very different ways than historians of other parts of the ancient Mediterranean. The fourth-century

Athenian orators' carefully wrought literary texts made citizen status a central consideration, and Finley followed them in seeing status as the overriding preoccupation in Athenian life (Finley 1985a: 35–61). In his view this set Greece and Rome apart from Egypt and the Near East, where very different status structures existed. In the 1990s some scholars went further still, relating the Greek texts to a specifically Hellenic "economic imaginary," driven by ideological conflicts peculiar to the archaic and classical poleis (von Reden 1995, 1997a; Carson 1999; Kurke 1999). But if the contexts of production and performance of the literature that survives from Athens were so radically different from those of the surviving texts from Babylon, Memphis, Susa, or (to a lesser extent) Rome, are we justified in assuming that the Athenian economy was structured by unique ideological principles?

The main issue in this book is whether the differences between the accounts that historians now write of economics in different parts of the ancient Mediterranean accurately reflect real differences in antiquity. One way to try to answer this question would be to relate the textual evidence that we actually have to the textual evidence that we do not have: that is, to ask whether the difference between, say, Greek and Babylonian economic texts is more an artifact of differences in literary production, consumption, and preservation than one of differences in the forms of economic activity. After all, unlike the situation in Egypt and the Near East, most Greek and Roman literature survives only because monks and scholars thought it was worth copying. Their criteria for selection determine the shape of our evidence; had some truly eccentric monastic order preferred agricultural accounts to rhetoric—or if the Greek climate had preserved different materials—we might have an Athenian version of the Kellis Account Book (see Bagnall, Chapter 9). An assumption that is rarely made explicit underlies all text-based comparative work on ancient Mediterranean economics: The texts that survive are a sample that more-or-less accurately reflects the larger population of texts that were originally produced, and therefore the extant documents reflect the nature of economic activity in different regions.

I see two ways to test this assumption. One is through arguments from silence: If, for example, the ancient Greeks had had institutions that functioned like the Assyrian palaces or Ptolemaic temples, we would hear about them in the extant sources. If a substantial amount of fourth-century Athenian foreign trade was organized by officials working for the Priestess of Athena, the practice would surely come up somewhere in Demosthenes. It does not, so we can probably argue from silence that there really were huge institutional differences between fourth-century Athens, Babylon, and Egypt. But there are many other questions, such as whether the

fourth-century Athenian economy performed better than that of fourth-century Babylon, or did a better job of fairly distributing the fruits of economic activity, for which arguments from silence are much harder to make. This is where a second way of addressing the implied assumption comes in. We need to look for new kinds of data that all the regions under consideration have in common. That means, of course, archaeological evidence.

By archaeological evidence, I mean all artifacts surviving from antiquity. Coins, inscriptions, papyri, and other objects with writing on them fall under this heading, as well as house walls, graves, and broken pottery. Artifacts with writing on them have their own interpretive problems, which have been ably discussed in Routledge's recent series of handbooks (Bagnall 1995; Howgego 1995; van de Mieroop 1999; Bodel 2001). But there are also problems and possibilities that inscribed and illustrated artifacts share with all categories of material remains from antiquity, whether excavated or found on the ground's surface.

I make five arguments:

1. Using archaeological evidence as a source for economic history is considerably more complicated than historians have generally recognized;
2. Despite the difficulties, we can still base economic analysis on archaeological data, although it will have to be analysis of a very different kind either from that advocated by Finley or most of his critics;
3. In the case of first-millennium BC Greece, we will need to construct new kinds of models, focusing on new questions;
4. These new questions are mostly about standards of living and, indirectly, economic growth;
5. These methods and model-building procedures can be transferred from Greece to other parts of the ancient Mediterranean, thereby providing the basis for a much broader, comparative economic history.

The Formation Processes of the Archaeological Record

Archaeologists gather the detritus of ancient life. That means that archaeologists never get direct access to ancient economic activities. They find washeries where Athenians separated silver from rock and lead, amphoras that once contained wine, and wrecks of the ships that transported these commodities from one end of the Mediterranean to the other. But they do not find the actual processes of production, exchange, and consumption. Consequently, archaeological data are always proxy data. In the process of excavating we actually create a static archaeological record that exists in the

present. Then we try to figure out what dynamic processes in the past might have produced it. In doing this, we face severe problems of ambiguity and equifinality.

The links between contemporary material and ancient life are the formation processes of the archaeological record. All inference depends on understanding these processes, so, not surprisingly, archaeologists have devoted tremendous energy to theorizing them.[1] But just as nonhistorians rarely understand the technical dimensions of historical source analysis, nonarchaeologists rarely grasp the range of issues involved in formation processes. Elton (1967: 30) made a famous distinction between professional and amateur historians, saying that "the hallmark of the amateur is . . . a readiness to see the exceptional in the commonplace, and to find the unusual ordinary." The professional, steeped in the sources and the methods developed by generations of other professionals, "knows the 'right' questions—those capable of being answered and those that lead to further questions" (1967: 32). In archaeology, formation processes occupy much the same place that source criticism does for the text-based historian. Here we reach the heart of the professional/amateur distinction: Most ancient historians have approached archaeology in a spirit of high amateurism.

To some extent, analysis of formation processes is a matter of common sense; but it is a *refined* common sense, and historians who fail to take advantage of professional archaeologists' refinements are likely to misinterpret the evidence seriously. For example, it should be obvious that the people who lived in the past did not leave behind on their sites examples of everything that they used. If we forget this, and assume that we are digging up a constant cross section of past material culture, we will go awry. Prehistorians (for example, Binford 1981a; Schiffer 1985) usually call this fallacy the "Pompeii premise" (even though Penelope Allison [1997] shows that it is as misleading at Pompeii as anywhere else). But how are we to move from the remains that we do recover to the activities of people in the past? Archaeologists sometimes like to say that their craft is like assembling a jigsaw puzzle without the benefit of a picture on the box lid and with 99 percent of the pieces missing. To make it worse, we do not know in advance whether the loss of pieces is caused by random or systematic distortion—that is, whether the loss of evidence is the equivalent of most of the pieces of the puzzle falling down the back of the couch or of someone carefully removing every piece with the color red on it. The answers to these questions set the limits for what we can infer from the material record.

Most archaeologists find it useful to break formation processes down into two types, depositional and postdepositional. Michael Schiffer (1976) speaks

instead of *C-transforms*, meaning cultural processes that cause people to use some objects in ways that make them enter the archaeological record while using others in ways that mean that they probably will not survive; and *N-transforms*, or natural processes that alter the material record between the time of its original deposition and its recovery by modern scholars. As Binford (1981a) insisted, Schiffer's assumption that the archaeological record is fundamentally a distortion of some preexisting neutral state is illogical. Further, some of the most important postdepositional processes are just as much C-transforms as the activities that originally produced deposits. Particularly when dealing with classical antiquities, this is a decisive advantage for the depositional/postdepositional classification, although its disadvantage is that the distinction between these processes sometimes has to be more arbitrary than that between Schiffer's C- and N-transforms.

POSTDEPOSITIONAL PROCESSES

I begin with postdepositional processes because the problems that they pose are often more obvious and commonsensical than some of those raised by depositional factors. Objects and traces of ancient activities survive in two main ways: by remaining in use (for example, a building like Hadrian's Pantheon in Rome) or through burial (usually under the earth, but also under the sea). The boundary between these processes is permeable, of course. An object may be buried, then returned to circulation through either natural processes or human activity. Some objects have very complex histories: they may get buried (say, as a cadaver), then be returned to circulation (as holy relics), then get discarded again (in the destruction of a monastery by Vikings), then return to the world of the living again (as museum exhibits). But most of the time, the basic categories of burial and continuous use remain useful.

Burial can be deliberate or accidental. Once an object or context of activity has been discarded, abandoned, or deliberately buried, it may survive more or less unchanged until an archaeologist unearths it, or—as virtually always happens—its situation may be significantly altered by postdepositional forces. Erosion may destroy entire sites or new construction may disperse the original depositional context, effectively destroying the ancient object. The rapid expansion of Greek cities and holiday resorts since the 1960s has devastated the archaeological record in some areas while leaving others virtually untouched. Even without human intervention, certain soil conditions may destroy all traces of pottery or iron; in Greece, wood, papyrus, cloth, and other organic materials hardly ever survive. On the other hand, geological events like the deposition of "Younger Fill" soils in some parts of Greece in late Roman times may bury the ancient landscape so

deeply that archaeologists rarely reach it (see Sbonias [1999] for a general discussion). Alternatively, artifacts that lie exposed on the surface may gradually disintegrate, with the result that more recent periods of the past are better represented than more distant ones (Bintliff et al. 1999).

After a century of collaboration with geophysicists and geomorphologists, archaeologists understand well the stratigraphic processes that have buried artifacts (E. Harris et al. 1993), and Mediterraneanists have made particular contributions to the study of how once-buried objects return to the surface (Francovich and Patterson 2000). Scientific advances allowing us to examine subsurface remains without destructive excavation have been widely applied in the Mediterranean (Pasquinucci and Trément 2000). Serious attention to soil science is more common on surface surveys than on excavations, but the tools are available, and attention to micromorphology and systematic sampling can clarify our understanding of the physical processes that have intervened between deposition and recovery (Courty et al. 1989; Orton 2000).

The processes of recovery are themselves part of the postdepositional transformation of the archaeological record. Recovery is incomplete; we know that we have not excavated every archaeological deposit in Greece, and that we have not published every deposit that has been excavated. For many purposes, this does not matter. But for economic historians who want to quantify their data, it can matter a great deal. For example, several million Greek pots have been found. But if we ask how pottery production worked, we often need to know what proportion of the pots originally made have been found so that we can extrapolate to the original output. For one category, the prize amphoras given out at the Panathenaic games every four years, we have rough estimates of the original numbers and thus can calculate a recovery rate. In a classic paper, Robert Cook (1959) argued for a recovery rate of 0.2 percent, which he applied to Athenian ceramic production generally. He concluded that pottery production employed so few people that it was basically a household craft, not an "industry" in any meaningful sense of the term (Arafat and Morgan 1989). But problems abound, even when we have such a well-known category of finds as the amphoras. Using a slightly different sample, T. B. L. Webster (1972: 3) calculated a 0.3 percent recovery rate; M. Bentz (1998: 17–18) puts it as high as 1 percent. Further, archaeologists do not agree on how to relate the Panathenaic amphoras to other categories of pots. Webster (1972: 4, 6) argued that probably about 1 percent of all Greek pottery had been found as of the time of his writing; Ingeborg Scheibler (1983: 9) estimated 3 percent, and M. Eisman (1974: 52) no less than 10 percent. Vladimir Stissi (1999: 405 n. 4), on the other hand, taking a more rigorous approach to quantification, concludes that even the 1 percent recovery rate "is far too high, especially for less finely decorated pots." These arguments have enormous implications for our understanding of ancient industry.

Stissi (1999) draws attention to a second major problem for quantification: the attitudes and ideologies of the archaeologists themselves. Alluviation, erosion, and natural decay are not the only filters between archaeologists and ancient behavior. At the most general level, the belief that classical archaeology should aim to illuminate a particular Greco-Roman ethos, which stands at the origins of European culture (see I. Morris 2004), means that many classical archaeologists see little to gain from careful quantification, or even saving sherds of plain pottery. Further, there are important differences among national traditions in the archaeology of Greece, which must be factored in to any study combining evidence from different research teams. European prehistorians have given much thought to such differences (Hodder 1991), but classical archaeologists have neglected this topic. Detailed histories of particular projects (for example, Fotiadis 1995) or of the guiding assumptions behind fieldwork in a particular period (I. Morris 2000: 77–106) can also help us control for variations in data collection and quality.

A final factor is the cultural history of the long time span between deposition and the beginning of professional archaeology in the late nineteenth century (Schnapp 1996). The Pantheon is still standing because in 609 Pope Boniface IV consecrated it as a church. If such activities had occurred more often, the archaeological record would be very different. Postdepositional cultural history is as important as natural processes in deciding what survives. It means that different kinds of objects have different chances of being preserved in constant use or through burial. Bronze, gold, and silver can easily be melted down and turned into new objects; they are inherently less likely to enter the archaeological record than pottery. Once broken, pottery is practically worthless and is hard to destroy completely. We can safely assume that a far higher proportion of ancient Greek pottery survives than of ancient Greek gold work—but how much higher?

We might draw three main conclusions about the role of postdepositional factors in the formation of the archaeological record. First, we know far more about some categories of evidence than others; second, the distribution of our evidence in time and space may not reflect the ancient activities that we are most interested in; and third, arguments from silence are even more problematic when we are dealing with artifacts than when we are dealing with texts.

Arguments from silence and quantitative claims based on inadequately controlled samples bedevil economic archaeology. Finding Athenian pottery in an Iron Age village in Sicily definitely tells us that such pottery reached this site. But unless the sample is large, drawn from different activity areas within the site, and accurately quantified, it will reveal little more than this. And unless all these conditions are met, the absence of Athenian pottery from the site will indicate nothing at all. An abundance of Athenian pottery in one location may indicate the house of a trader, or an émigré Athenian, or a collector; or it may mean that Athenian wares were particularly popular at this site. Unless we can quantify the entire range of our evidence and test theories against a null hypothesis, we simply cannot say. The normalization problem can only be addressed through massive statistical syntheses and more systematic attention to sampling (Orton 2000). Otherwise, attempts to interpret pottery (or other artifacts) as evidence for trade are highly vulnerable. For example, in a well-known article, Colin Renfrew (1975) proposed a series of models correlating the fall-off in frequency of finds of a given provenance as distance from the source increases with different exchange mechanisms. But in practice, archaeologists have not been able to control both depositional and postdepositional factors across a wide range of sites and to specify the level of background noise with sufficient accuracy to use such quantitative tools. As Ian Hodder and Clive Orton (1976) put it, archaeological formation processes are often equifinal: Radically different activities in the past can produce much the same archaeological record in the present.

Generally, the fuller the data, the less susceptible they are to postdepositional processes. One of classical archaeology's greatest strengths is the sheer quantity of evidence available (Snodgrass 1987: 14–35; Stissi 1999: fig. 156), although this benefit is partly canceled out by biases in collection strategies. Greek archaeologists have often used painted pottery to make claims about the history of trade, but insofar as these are quantitative claims, they are generally undermined by failure to think hard enough about postdepositional processes (see Snodgrass 1980: 126–29). The constraints of paper publication mean that even with the best will in the world, archaeologists cannot include everything they recover in their catalogues of finds. A few archaeologists understand the need for quantifiable data sufficiently well that they explain the relationship between the excavated record and the published record, and the criteria by which they decide which parts of the former end up in the latter; but most do not. The result is that the data are systematically skewed, but in unpredictable ways. Nor is it even possible in all cases to go back to the primary data, the finds themselves. Many archaeologists in

Greece discard all pottery but the "diagnostic" sherds (rims, handles, bases, decorated fragments, and other pieces that can be dated), often keeping no records of the undiagnostic finds. Human bones are routinely thrown away, and few digs systematically collect animal bones or pollen samples. The situation has improved in the last twenty years, but the postdepositional distortion of much of the record is still large and unpredictable.

DEPOSITIONAL PROCESSES

Archaeologists know a great deal about postdepositional processes. But even in a situation with the best imaginable preservation and the most up-to-date recovery techniques, and controlling for as many postdepositional factors as humanly possible, archaeologists still face problems in moving from a static, contemporary material record to the dynamics of past behavior (Binford 1983). In the 1960s, the self-styled "New" or "processual" archaeologists (so-called because they felt that they studied universal behavioral processes, as opposed to the particularistic details that 1950s culture historians had worked on) began taking this problem very seriously. They tried using ethnoarchaeology (the study of the material culture of contemporary populations) to build up "middle-range theory," linking the material fragments that archaeologists recovered with the activities that would have produced them. This important work put the study of archaeological formation processes on a firmer and more systematic footing.

But critics in the 1980s (Hodder 1982a, 1982b) pointed out that the New Archaeologists approached deposition as if all people in all places thought and behaved in the same ways, effacing the role of culture. In searching for cross-culturally valid laws of the behavior behind the archaeological record, New Archaeologists were simply ignoring human agency, at the very moment when cultural anthropologists were focusing more and more on meaning, culture, and representation (for example, Geertz 1973; Bourdieu 1977 [1972]). For Lewis Binford (1981b), ethnoarchaeology was the royal road to middle-range theory; for Ian Hodder (1982a), it was a way to reveal the complexity and cultural specificity of the ways people use material culture.

The critics of the New Archaeology, who called themselves "postprocessualists," shared in the widespread anthropological turn toward linguistic and constructivist theories of meaning (Hodder 1986). Claude Lévi-Strauss (1949) had drawn on structural linguistics to interpret kinship as a communication system, and French theorists went on to demonstrate that material culture also functioned as a kind of language (Baudrillard 1981 [1972]; Bourdieu 1984 [1979]). The postprocessualists agreed with the New Archaeologists that understanding postdepositional factors does not give us

direct access to the past. But the postprocessualists went further, arguing that understanding deposition is not a matter of formulating a general middle-range theory but of reaching historical understandings of particular cultures. Material culture is a text (Shanks and Tilley 1987a, 1987b; Tilley 1990, 1991), just as heavily implicated in language games as is verbal culture (Lyotard 1984). The material text is there to be read and interpreted, not explained through reduction to underlying principles (Tilley 1993). Where New Archaeologists had looked to the natural sciences for models, postprocessualists turned first toward history and later toward literary theory. Metaphor, experience, and performance seemed like more useful analytical categories than causation (Shanks 1992; Tilley 1993, 1994, 1999; Hodder et al. 1995; Hodder 2001; Pearson and Shanks 2001).

Hodder (1986: 3) suggests that "in archaeology *all* inference is via material culture. If material culture, all of it, has a symbolic dimension such that the relationship between people and things is affected, then *all* of archaeology, economic and social, is implicated." That is, we do not simply add postprocessual archaeology to New Archaeology to get a fuller picture. Rather, it undermines the central assumption of the New Archaeology, that a general middle-range theory will allow us to interpret depositional processes.

The more complex the societies we deal with, the more obvious the postprocessualists' point seems. For example, no gold cups have been found in archaeological deposits in classical Athens, even though we know from examples excavated in Bulgaria that classical Athenian workshops were making such cups (Filow 1934; Vickers and Gill 1994). Further, classical texts mention gold cups, and make clear—as the postprocessualists insist—that the meanings of such cups were contingent, context-dependent, and negotiable. Thucydides (6.32) tells how patriotic Athenians poured libations from gold cups in 415 BC as the fleet sailed for Sicily. Demosthenes (22.75) also mentions gold cups, but only to imply that any man who took pride in owning gold cups lacked the qualities of a true citizen. To say that Meidias went around positively bragging about his gold cups was to hint that he harbored antisocial hubris, posing a threat to democratic society (Demosthenes 21.133, 158). And when pseudo-Andocides (4.29) told a jury that Alcibiades not only bragged about having such cups but even pretended that cups belonging to a state embassy were in fact his own, he represented him as living beyond the pale of civilized society.

Given this cultural milieu, it is hardly surprising that we find no gold cups in the three thousand or so graves known from fifth- and fourth-century Athens. Athenian expectations about the use of material culture were complex and restrictive. It seems that there was no single meaning to a gold cup.

Depending on who used it, and how, and who was interpreting that use, it could demonstrate a person's piety or hubris. By focusing on the active role of culture and the importance of beliefs, postprocessual archaeology in a sense requires us to know what was going on in people's heads before we can draw conclusions from the material record. This largely accounts for the common criticism that it is an "anything goes" method. Archaeologists weave different stories about prehistoric values from their meager materials, then use these stories to interpret the data to fit with their initial assumptions. This is not a problem if we start from the poststructuralist position going back to Hayden White's classic *Metahistory* (1973) that there are no grounds other than political or aesthetic ones for choosing between competing historical accounts. It is no longer unusual to find archaeologists, including those interested in Greece, arguing that the only way to judge an archaeological interpretation is by its political implications (for example, Hamilakis 1999). But if we accept Douglass North's proposition (quoted in Chapter 1) that in economic history, "'explanation' means explicit theorizing and the potential of refutability," then postprocessual archaeology poses a serious challenge to economic archaeology.

I have argued elsewhere (I. Morris 2000: 3–17) that it is possible to take poststructuralist arguments seriously yet still evaluate interpretations, regardless of their political implications, by using the standard historical methods of contrasting different kinds of sources and varying the geographical or temporal scale of analysis. If we find that texts (whether literary or material) seem to tell the same story, even though they were generated out of entirely different language games, or that very similar patterns recur in otherwise very different cultural zones, we know we are onto something deeper than the linguistic and nonlinguistic structures within which historical actors constructed and contested meaning. Close analysis of the evidence is likely to tell us a great deal about the peculiarities of each community and struggles over meaning within them; but that does not mean that readings that move past form to content are necessarily false. There will always be problems with moving from a discursively constituted material record to nondiscursive economic phenomena, but the problems are of well-known types, and historians and archaeologists have methods for dealing with them. On some occasions the complexities of discursive conflicts may indeed rule out such attempts to go beyond form, but that needs to be demonstrated empirically, not simply asserted.

Classical Greek archaeologists have three tremendous advantages over prehistorians. First, as noted above, their evidence is extremely rich and varied; second, its absolute chronology is fixed unusually well; and third, it

includes written sources. This density and variety of data points constrains interpretation far more than in the study of the European Neolithic, where much of the most imaginative postprocessual work has been done (Hodder 1990; Tilley 1996; Thomas 1996, 1999; Bradley 1997, 1998). It seems to me that archaeology is, in the first instance, always a form of cultural history. As the postprocessualists insist, all inference is via material culture, and all material culture and depositional practice is implicated in representational strategies. We must always begin analysis by trying to understand these strategies. But where the data are dense enough and varied enough, we can narrow the range of plausible interpretations to the point that we can move beyond the realm of discourse to prediscursive economic phenomena (I. Morris 2000). Few postprocessual archaeologists explicitly deny this, but their near-total focus on the construction and contestation of meaning, at the expense of ecology, demography, and economics, suggests that economic history in the sense defined in Chapter 1 (Morris and Manning) has been stricken from most postprocessual research agendas.

Archaeology and Greek Economic History

Most Greek economic historians have not known enough about archaeological theory and method to grasp the significance of the formation processes of the material record. They have tended either to underestimate or overestimate the problems involved. The best example of the former is still Mikhail Rostovtzeff's magnificent *Social and Economic History of the Hellenistic World* (1941). Classicists regularly hold this work up as a model for the integration of objects and texts. Rostovtzeff knew what was involved in field archaeology, having been involved with large-scale excavations at Dura Europus in Syria, and he filled his three-volume study with references to and illustrations of artifacts. But the reader will search in vain for any systematic analysis of archaeological sources and formation processes, statistical summaries of artifact distributions, or even close arguments over the interpretation of specific finds. Rostovtzeff made the bold claim—one well established among Greek historians since the 1890s, though Rostovtzeff gave it its strongest form—that from the age of Alexander onward, the east Mediterranean world was a realm of interlocked markets (see particularly Rostovtzeff's 1935/36 summary essay; and Reger's detailed response [1994]). Rostovtzeff produced careful textual editions of Egyptian papyri and was sometimes explicit about how he deduced economic patterns from estate accounts (Manning, Chapter 8); but he did not apply the same high analytical standards to the evidence of things as to that of words. His use of

material culture was a classic example of what I have called the "bits-and-pieces" approach to using archaeological data as historical sources (I. Morris 1992: 1–2, 200–204). Eschewing systematic quantification and analysis of formation processes, he selected data that he thought were either exceptionally interesting or wholly typical and used them to illustrate a story founded on the texts.

The second tendency is best illustrated by Moses Finley's use of archaeology. In principle, Finley strongly advocated archaeological history, arguing that

> it is false to speak of the relationship between history and archaeology. At issue are not two qualitatively different disciplines but two kinds of evidence about the past, two kinds of historical evidence. There can thus be no question of the priority in general or of the superiority of one type of evidence over the other; it all depends in each case on the evidence available and the particular questions to be answered. (Finley 1985b: 20)

His main programmatic essay on archaeology (1975: 87–101) raised some of the same issues that I outlined above, but in practice, he virtually ignored archaeological data. Noting the complexity of the arrangements for pottery production described in a third-century AD papyrus from Oxyrhynchus, he very sensibly concluded that "archaeological evidence or archaeological analysis *by itself* cannot possibly uncover the legal or economic structure revealed by the Oxyrhynchus papyri or the alternative structures in Arezzo, Puteoli, Lezoux or North Africa" (1985b: 25). Few archaeologists would dispute this. But whereas archaeologists would then presumably ask how we might combine excavated kiln sites with the texts, or what questions the archaeological remains *could* answer, Finley simply dropped the subject. Similarly, he was much impressed by Mortimer Wheeler's cautionary tale about how reports of Roman sherds found in Gotland grew in archaeologists' minds into significant Roman trade with Sweden. In fact, all thirty-nine fragments came from a single pot (Finley 1985a: 33). An archaeologist might conclude from this that distribution maps rarely tell the whole story, and that we need more detailed work on the precise types, quantities, and contexts of Roman artifacts in the Baltic. Michael Dietler (1995, 1997) has done just this, undermining world-systems models of archaic colonial encounters in southern France. Finley, however, seems to have taken Wheeler's tale as a demonstration of the limitations of archaeological data. His longest discussion of Roman pottery (1985b: 23–24) emphasizes how little it tells the historian, even though the 1980s saw spectacular advances in economic

interpretations of amphora data (Tchernia 1986; *Amphores romaines* 1989; Laubenheimer 1992).

Finley was reacting against the excesses of earlier generations of economic historians, for whom archaeology was often just grist for the mill of crudely modernist speculation. But by avoiding this whole class of data he did just as much damage to ancient economic history. While archaeological formation processes raise more serious issues than most economic historians seem to realize, this is not a counsel of despair. It just means that we must think very carefully about how we use the data. The archaeological data have the potential to transform radically the questions we ask about Greek economic history and our ability to answer them. Like many Roman archaeologists (see Hitchner, Chapter 10), I suggest that had Finley taken the material record more seriously, he might have developed a different model in *The Ancient Economy*. If we combine archaeological and textual data, we may be able to compare the different regions in the ancient Mediterranean far more rigorously and systematically than when using texts alone.

Archaeology and Standards of Living

But how do we do all this? Only, I suggest, by shifting the focus of our inquiries. Using the archaeological data appropriately calls for theoretical as well as empirical reformulations. In our introduction (Chapter 1), J. G. Manning and I quoted Douglass North's brief definition of the task of economic history: "To explain the structure and performance of economies through time." Finley concentrated almost exclusively on structure at the expense of performance. Economic historians of most other periods have emphasized economic growth (either in the sense of increasing total output or increasing per capita output; see Saller, Chapter 11) as the central issue. Finley was very clear that growth was not a useful concept in Greek economic history, and Paul Millett has recently claimed that "scope for sustained growth in the centuries BC was elusive or non-existent" (2001: 35).

I see three likely reasons for Finley's position (and Millett's elaboration of it): first, a reaction against the excesses and sloppy scholarship of the modernist historians of the 1890s–1950s; second, the nature of the literary sources, which prevented discussion of economic growth in any serious way; and third, his political position within a tradition going back to Polanyi and the Frankfurt School's critiques of Marxism. Given these factors, Finley's arguments make a great deal of sense and were certainly more consistent with the textual data than their precursors, even if this came at the cost of giving up even the fragments of archaeological data that Rostovtzeff had

pursued. What makes far less sense is the failure of those many classical historians who have rejected Finley's claims out of hand either to pursue the economic theory and methods that they implicitly claimed were superior to Finley's substantivism, or to seek out new forms of evidence that would allow them to refute him.

We can perhaps explain why Finley and the substantivists spent so little time on economic growth; but why do economic historians of nonancient periods spend so much time on it? In the most widely read economics text of the twentieth century, Paul Samuelson and William Nordhaus spelled out the answer, under the upbeat heading "Cool Heads at the Service of Warm Hearts":

> You might well ask, What is the purpose of this army of economists measuring, analyzing, and calculating? The ultimate goal of economic science is to improve the living conditions of people in their everyday lives. Increasing the gross domestic product is not just a numbers game. Higher incomes mean good food, warm houses, and hot water. They mean safe drinking water and inoculations against the perennial plagues of humanity. . . . Although there is no single pattern of economic development, and the evolution of culture will differ around the world, freedom from hunger, disease, and the elements is a universal aspiration. (Samuelson and Nordhaus 1998: 7)

The history of economic growth explains differing standards of living in the past; and if we can explain why standards of living differed in the past, we may be in a better position to create institutions that foster continuing improvements in the future. As the economist Dan Usher asks, "Why, after all, would we want to measure economic growth, why is public policy directed to the promotion of economic growth, if we are not better off after economic growth than we were before?" (Usher 1980: 2).

No direct data for economic growth survive from antiquity, and Finley was probably right to argue (1985a: 23–26) that no such data were collected in the first place. However, direct data for standards of living *do* exist, if we examine the archaeological record against the textual record in appropriate ways. We might take our lead from the ways in which historians of the Industrial Revolution have worked.

For fifty years historians and economists have argued fiercely over whether English workers' living standards rose or fell between 1780 and 1850. This debate often came to stand in for the larger question of whether capitalism as a whole was a good or a bad thing (Taylor 1975; Cannadine 1984), and—hardly surprisingly in light of the high stakes—the protagonists

developed rigorous methods and penetrating polemics. The standard of living, or "the material side of happiness," as Peter Lindert puts it (1994: 359), can mean many things. Some historians focus on real income, calculated by estimating the output of the country's agricultural, manufacturing, and service sectors and then comparing these figures with changing prices of consumption goods. Others put more faith in real wages, comparing evidence for the changing incomes of specific groups of workers with weighted price indices for bundles of consumption goods. Ferocious debates can develop over the weighting of different commodities: In the case of the English Industrial Revolution, slight changes in the importance attached to clothing prices can completely alter our perception of whether workers' material lives were getting better or worse (Lindert and Williamson 1983, 1985; Crafts 1985; Mokyr 1987; Feinstein 1998; R. C. Allen 2001). Concerned by the fragility of these measures, some historians have turned to direct measurements of diet, mortality rates, and morbidity (that is, patterns of health and sickness), while others have looked to evidence for age-specific heights as proxy data for all of these factors. Housing has attracted much attention, as have pollution and the amount of time people spent working. The variety of ways of looking at standards of living creates its own problems. If, for example, we think of the increasing life expectancy of nineteenth-century British workers as something exogenous to the workers' own households, created by government regulations, we should use it as a multiplier in calculating living standards on the basis of the real wage. By some estimates, this would mean that economic growth was as much as 25 percent higher than is normally thought. On the other hand, if workers effectively purchased longer lives by spending more of their income on nutritious food, fuel, or medical care by cutting consumption or saving somewhere else, it would be a mistake to use longevity as a multiplier (Williamson 1984). Roderick Floud, K. Wachter, and A. Gregory conclude that "each way [of estimating standards of living]—mortality, morbidity, diet or housing conditions—presents only a partial view of reality, while attempts at combining them give rise to the problems of double counting" (Floud et al. 1990: 286).

But for all the difficulties of the exercise,[2] a consensus has emerged that after stagnating between 1750 and 1820, English workers' living standards did increase significantly in the later nineteenth century (Feinstein 1998; Voth 2001). There is even some agreement on how to evaluate workers' perceptions of a declining quality of life relative to this material improvement (Lindert 1994: 375–78). Most important of all for the ancient historian, archaeologists are at much less of a disadvantage relative to modernists than

they are in most areas of economic history. The indices that received most attention in the 1990s—stature, nutrition, mortality, morbidity, and housing (see Floud et al. 1990; Komlos 1996; Steckel and Floud 1997; Steckel 1999; Haines and Steckel 2000)—are archaeologically observable. There is some evidence that age-specific stature and the real wage moved together in early modern Europe (Floud 1994; Steckel 1995).

A few economic historians have recognized the huge potential of excavated skeletons for charting very long-term trends on standards of living (for example, Steckel and Rose 2002), but in practice, of course, the archaeological data present even more problems than modern textual ones. All the depositional and postdepositional factors described above apply. And the most severe difficulty is of our own making: Classical archaeologists rarely submit human bones, with all their potential as evidence for stature, mortality rates, and morbidity, to systematic analysis; and they rarely collect floral, faunal, and pollen remains at all, even though these are the most important information for food preparation and consumption.

In the final part of this chapter, I focus on the one category of evidence that is relatively abundant and well documented: housing conditions. This index of the standard of living points to substantial gains all around Greece in the half-millennium between 800 and 300 BC—so substantial, in fact, that they cannot be reconciled with the Finleyan model of essentially static economic performance. The gains are also hard to reconcile with the common assumption that prior to about AD 1750, improvements in productivity always triggered Malthusian cycles in which population expanded to outrun resources. Some economic historians even use preindustrial population sizes as a direct measure of economic output (De Long and Shleifer 1993). Recent work on early modern China—Malthus's classic case of the "positive check" of starvation and disease—has shown that its demographic history was much more like that of Western Europe than Malthus had realized (Lee and Wang 1999). Walter Scheidel has also revealed unsuspected complexities in Roman demography (Scheidel 2001a, 2001b, 2001c). I suggest that ancient Greece also defied simple expectations. In the language of economists, we might say that the Greeks not only widened capital to keep pace with population growth but also deepened it. An archaeological history of living standards has the potential not only to allow more systematic comparisons between different regions of the ancient Mediterranean than is possible with a purely textual approach but also to make a contribution to debates among economists on the conditions of economic growth.

Figure 5.1 shows the trend in median house sizes between 800 and 300 BC, based on a database of more than three hundred examples.[3] This

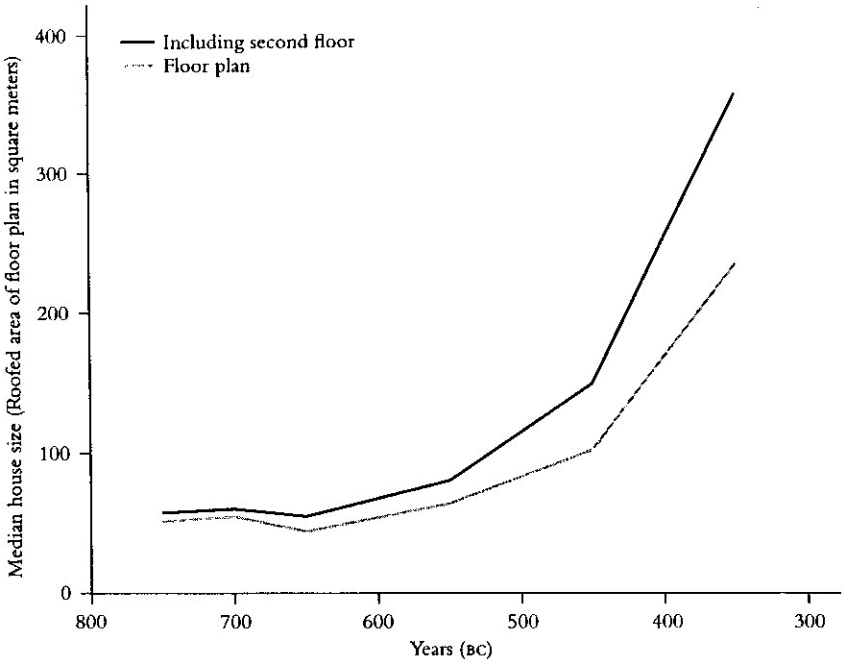

Figure 5.1. Median house sizes in the Aegean, 800 to 300 BC

catalogue is not complete, and a more thorough collection may require revision of the values shown in the provisional estimate in Figure 5.1, but the graph is unlikely to need serious changes.

The curves shown in Figure 5.1 require some explanation. First, the lower line is based on the floor plans of the fully or almost fully excavated houses. I have counted only those parts of the house that were probably roofed. Counting the entire floor plans would increase the median sizes for the sixth-, fifth-, and fourth-century houses by between 15 and 30 percent but would have less impact on the seventh-century houses, when courtyards were less common, and very little impact on eighth-century houses, when courtyards were rare. Finally, I use the median rather than the mean size because the fourth-century mean is skewed by a few massive houses, such as House IV at Eretria (1,950 square meters) and the House of Dionysus (3,183 square meters) and the House of the Rape of Helen (1,700 square meters) at Pella (see Fig. 5.2). These houses stand at the beginning of the tradition of Hellenistic palaces (Kiderlen 1995; Hoepfner and Brands 1996: 2–6). The mean value for the cluster of houses dated around 700 BC is also significantly

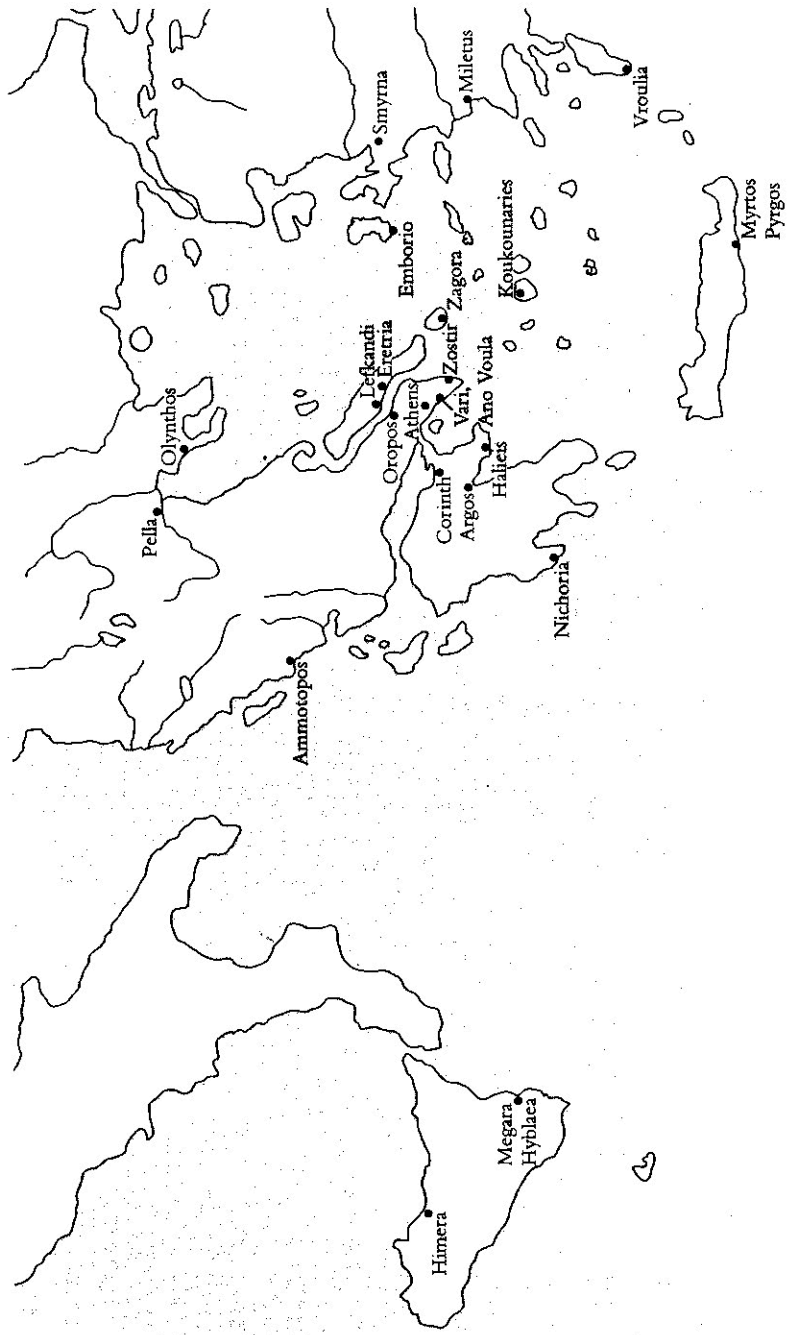

Figure 5.2. Sites mentioned in this chapter

TABLE 5.1
Mean and median house sizes, 800-300 BC, in m²

Period	Mean	Median
800–700	53	51
c. 700	69	56
700–600	53	45
600–500	92	67
500–400	122	106
400–300	325	240

higher (23 percent) than the median, because of the "Great House" (H 19/22/23/28/29; 256 square meters) at Zagora on Andros.[4] Table 5.1 shows the differences between the mean and median values.

Focusing on median values and the probable roofed area of the floor plan minimizes the differences between the eighth- and fourth-century houses, but there is still a very strong upward trend. The floor plan of the typical fifth-century house included more than twice as much roofed space as that of its eighth-century predecessor; that of the typical fourth-century house, nearly five times as much.

House Sizes: Postdepositional Problems

The whole range of depositional and postdepositional factors discussed above bear on these simple calculations. I discuss five particularly serious ones in this section.

First is the obvious problems of preservation. We know from literary sources (Lysias 1.9–10) and vase paintings that some houses had two floors, and on occasion parts of staircases have been found. The bottom line in Figure 5.1 accurately represents the state of our archaeological knowledge but not the realities of life in ancient Greece. But we are able to control for the distortion created by the disappearance of upper floors. The most extreme counterargument against the trend represented by the bottom line in Figure 5.1 would be to assume that all eighth- and seventh-century houses had second floors, as did many (let us say 50 percent) sixth-century houses, but no fifth- or fourth-century examples. But this was obviously not the case. The foundations of the Great House at Zagora and the peculiar apsidal room L (following the lettering system in Mazarakis Ainian's plan [1997: fig. 321]) at Koukounaries, both dating around 700, could have supported an upper floor, and there is some actual evidence for such a floor in the seventh-century Double Megaron at Smyrna. But very few of the flimsy eighth- and

TABLE 5.2
Estimated percentage of houses with second floors, 800–300 BC. Column (a) makes realistic assumptions, based on surviving data; column (b) makes assumptions that weaken the pattern visible in Table 5.1 and Fig. 5.1 as much as possible

Period	a	b
800–700	10	100
c. 700	10	100
700–600	25	100
600–500	25	50
500–400	50	25
400–300	50	0

even seventh-century foundations could have carried this much weight. Nearly all of our direct evidence comes from the fifth and especially the fourth century. At Ammotopos in Epirus, much of the second floor of fourth-century House I is actually preserved (Hoepfner and Schwandner 1994: 147–50). But even making this patently false assumption that all eighth- and seventh-century houses had second floors, and no fifth- and fourth-century houses did, would mean that instead of doubling between the eighth century and the fifth, house sizes stayed more or less the same; then after 400 BC they suddenly increased by a factor of 2.4. This assumption would discount any growth until the end of the Peloponnesian War, creating even more striking expansion thereafter. This would be the extreme case of biasing the data against the hypothesis. If instead of these implausible assumptions we make any reasonable estimate of the frequency of second floors, the rate of increase in house size between the eighth and the fourth century will only increase. The upper line in Figure 5.1 represents changes in the median house size if we assume that 10 percent of the eighth-century houses had second floors, 25 percent of those of the seventh and sixth centuries, and 50 percent in the fifth and fourth centuries (Table 5.2). While these guesses no doubt miss the mark, the upward curve in Figure 5.1 is probably as accurate an estimate of changes in house size as we can get.

A second problem is how we decide which rooms belong to which house. Most eighth-century structures are single-roomed, but Mazarakis Ainian (forthcoming) argues convincingly that at Oropos, Eretria, and Lefkandi, two or more small oval or apsidal structures would typically be grouped together in family compounds. This, he suggests, was an intermediate stage between single-roomed Dark Age houses and multiroom archaic and classical courtyard houses. It may be a typically Euboean pattern, with freestanding

single-room structures preferred in other parts of Greece. But after 700, interpreting the boundaries of multiroom rectilinear houses is increasingly an issue. At some classical sites, like Himera and Olynthos, the dividing walls are fairly obvious, but some earlier cases, such as Zagora and particularly Vroulia, are less clear. My own suggestions for Vroulia (I. Morris 1992: 193–96) differ somewhat from Franziska Lang's (1996: 101 and fig. 65), though not enough to have a measurable impact on the seventh-century median figure. Overall, ambiguities about classification are not a major concern.

Third, the developmental cycle of the household raises a related problem. As Tom Gallant (1991: 11–33) has illustrated with a series of computer simulations, the size of any particular Greek family would have changed through time. When a family gets bigger, it may try to expand its house at the expense of a neighbor, if houses abut one another. When the family shrinks, it may sell off or rent parts of its house. Using a combination of house remains and texts, Elizabeth Stone (1987) has traced this process in a group of houses at Nippur in Iraq between 1742 and 1734 BC, and although no Greek site provides such detailed evidence, we must take this process into consideration. Overall, though, it does not seem to pose serious problems for interpreting long-term trends in house sizes. Where it is possible to trace the gradual expansion of houses, as at Zagora, Megara Hyblaea, and Himera, I have distinguished the different phases. On the whole, these phases all fall within the century-long chronological divisions I use. Where the standards of excavation or the quality of the report prevent such detailed analysis, I have concentrated on the houses' final plans (as at Vroulia: I. Morris 1992: 197–98).

Fourth, there may be problems of differential preservation from one period to another. For example, many Cycladic villages were abandoned in the seventh century. At Zagora, the final phase is quite well preserved, but virtually nothing survives of the early-eighth-century houses except scatters of pottery. At sites like Athens or Eretria, which remained in use for long periods, the tendency for fourth-century and Hellenistic houses to be larger and sturdier than earlier buildings means that sixth- and fifth-century houses suffer a disproportionate rate of disturbance. However, while these factors affect the relative numbers of houses surviving from each phase, they should not distort the typical size of the surviving houses.

Finally, the distribution of excavations may distort the pattern. If, for example, all the excavated eighth-century houses came from small village sites, and all the excavated classical houses from wealthy centers like Athens, Corinth, and Argos, the evidence would probably exaggerate the median size of classical houses relative to that of Late Geometric houses. However,

that does not seem to be the case. In the eighth century, Smyrna and Eretria—surely two of Greece's major towns—are as prominent in the excavated record as Zagora; and in any case, the Zagora houses are among the largest examples known. There may be more of a problem in the seventh century, when Vroulia, Emborio, and Megara Hyblaea dominate the record, although Miletus and Smyrna are also prominent. The sixth-century sample is small, and no one site dominates, but in the fifth century Himera and (to a lesser extent) Athens are prominent. However, the range of house sizes does not vary strongly. In the fourth century, the relatively wealthy city of Olynthos dominates the record, although more and more houses are now coming from Ano Voula and Stylida, and as in the fifth century, the typical house sizes seem consistent from one site to another. In his recent attempt to document the housing of the poor in classical Greece, Brad Ault (forthcoming a) was able to find very few examples of small, squalid houses. The vagaries of excavation inevitably distort the record, but there is no obvious reason to think that they have created a spurious pattern in Figure 5.1 and Table 5.1.

House Sizes: Depositional Problems

As always, matters become more complex when we move from postdepositional factors back to depositional ones. Decisions about the form and structure of houses are always ideological as well as economic. Home is where the heart is, and few things are dearer to people than their houses. In a classic essay, Pierre Bourdieu (1990 [1970]: 271–83) showed how the normative house type of the Algerian Kabyle expresses deeply held cosmogonic principles, and ethnographers have illustrated similar practices all around the world (Blier 1987; Carsten and Hugh-Jones 1995). Richard Blanton (1994: 13–15) concluded from a cross-cultural survey that because houses embody wealth as well as standing for it, they are "less subject to falsification" than other indices of status. Blanton's claim is certainly valid if we limit our study to the question of the standard of living, since the lavishness of housing is itself a major part of that standard. However, to determine whether underlying economic growth made rising standards possible, we must factor in belief systems, which influence the proportion of wealth invested in housing. This raises many problems, but here I will concentrate on just two.

First, the most obvious way in which depositional factors might affect interpretation of Figure 5.1 is if the buildings that I have cavalierly been calling "houses" in fact changed functions in significant ways across these five centuries. Alexandra Coucouzeli (1999) has argued that the fifty-meter-long tenth-century apsidal house found at Lefkandi (Popham et al. 1993) was in

fact rather like the long houses common among native peoples in the Americas, and home to a dozen families. Comparing the size of this building with that of single-family, fourth-century homes would be a complex and potentially misleading exercise. Rival claims that the Lefkandi apsidal building was a chief's house (Crielaard and Driessen 1994; Mazarakis Ainian 1997: 48–57) seem more plausible to me, but Coucouzeli's argument nevertheless raises serious interpretive issues.

Changes in the crowding of houses are a major component of studies of living standards during the Industrial Revolution (Burnett 1986: 43–46, 61–69, 144–46, 326–27), but this is not archaeologically observable. As in the case of second floors, the most serious uncertainties concern ninth- and eighth-century houses. The literary evidence for residence strategies is largely anecdotal (Pomeroy 1997; Cox 1998; Patterson 1998), but it seems clear that the normative house in classical times was a single-family residence, and that families were typically nuclear. The precise size would vary according to the phase of the family's developmental cycle and the presence of dependents; and, as in modern case studies (McKinnon 1994: 277–78), we should imagine what scholars of urban blight since Seebohm Rowntree (1901) have called the "life cycle of poverty." Families with few income-earners and several young children face severe pressure on resources, which usually leads to the children going hungry so that the breadwinners can remain healthy enough to keep working. As the children grow up and begin to earn for themselves, the family should move out of poverty; but as the family's heads age and themselves become dependent on their children, they may slide back into dire need. The young and the old are always the most exposed to poverty, with devastating results for growth, morbidity, and mortality. Although it is not documented, the same was likely true in classical antiquity, along with the common consequence: Families in the grips of severe poverty would move into smaller and smaller accommodations, increasing the ratio of people to roofed space and creating unhygienic conditions.

Many classical households would have included some slaves, while fewer households would have done so in the eighth through sixth centuries. The data for the numbers and distribution of slaves are, however, poor. The enormous fourth-century houses at Eretria and Pella must have had staffs running into dozens, but the textual evidence—such as it is—suggests that few households had more than a handful of slaves (Garlan 1988: 55–60).

If we assume that before 700 the typical domestic residence was shared by several nuclear families, or by extended family groups, then the amount of roofed space per capita in the early periods was even smaller than is represented in Figure 5.1, and the subsequent increase in the standard of

living was even more dramatic. Instead, we might extend Mazarakis Ainian's argument that before 700, nuclear families lived not in single houses but in compounds of several buildings, thereby increasing the amount of roofed space per person and reducing the sharp increase in Figure 5.1.

To some extent, these competing possibilities can be addressed empirically. In a critique of claims that the Early Bronze Age hamlet of Myrtos Pyrgos on Crete was the home of an undifferentiated "herd," sharing communal facilities, Todd Whitelaw (1983) showed that the distribution of hearths, storage, food preparation, and so on, suggested that the settlement was divided up between distinct families, each practicing the full range of domestic activities. Similarly, in Early Iron Age Greece—at all sites where the evidence is well enough published for analysis—we find that each structure has traces of spinning and weaving, preparation of food, storage, basic craft activities, and occasionally more specialized ones (I. Morris 1991: 31–32). We can probably conclude that eighth- and seventh-century settlements contained "houses"—permanent homes of nuclear families—in much the same sense as those of the sixth through fourth centuries.

The other obvious depositional problem is that house designs may tell us more about ideology than about economics. Down to 700 BC many houses in central Greece were simple, single-roomed, curvilinear structures, in which all activities went on within the same space or in the open air. At some sites, two or more huts of this kind were grouped together in family compounds. After 700, multiroomed, rectilinear structures became more popular, with clear evidence for functionally specific spaces. These new house styles, best seen at Zagora, were much more complicated and more expensive than the older styles. The question is whether we should see them as evidence that economic growth and increasing wealth raised the standard of living, or whether we should assume that ideological factors—a commitment to restraint and community homogeneity—had limited the scale of house building before 750, in which case the late eighth-century change reveals more about attitudes than economics. I have argued (I. Morris 2000: 280–86) that the shift was intimately linked to the creation of stronger gender distinctions. But did an increasing desire to separate male and female space dictate that people spent a higher proportion of their income on housing to create new spatial forms against an unchanging economic background, or did a rising standard of living present new opportunities for reinforcing gendered space? Or both?

Questions of this kind are central to postprocessual archaeology. We have no direct access to economic patterns, because all our evidence comes to us already implicated in strategies of self-representation. I suggested above,

though, that standard historical methods of contrasting different kinds of sources do allow us to move beyond this impasse. In the case of eighth- and seventh-century houses, we might look at size against the background of building techniques. If people built small houses before 700 because they believed that simplicity and homogeneity were appropriate, and larger houses after this date because they wanted to engender space more rigidly, there is no particular reason why the methods of building should have changed. Yet we find that people not only built bigger after 700; they also built better. Drains and permanent, stone-lined hearths become quite common in the seventh century, and foundations were often more substantial. When houses really began to grow, around 500 BC, roof tiles (used on temples since the seventh century) replaced flat stone and clay roofs or pitched thatched roofs (Lang 1996: 108–17). This need not mean that increasing wealth was more important than changes in beliefs about gender or hierarchy in causing the expansion of house size around 700 BC; but it is hard to see why a new gender ideology would lead people to protect their homes better against water damage. The most plausible interpretation is that economics and ideology were inextricably linked. Growing wealth enabled typical citizen families to spend more on housing, producing more comfortable homes, and also homes that emphasized more strongly than before the distinctions between male and female spheres.

Median house sizes increased sharply between the sixth and fifth centuries. In an influential study, Wolfram Hoepfner and Ernst-Ludwig Schwandner (1994 [1986]) argued that a standardized "Typenhaus" appeared in the fifth century. Grouped into uniform blocks, these Typenhäuser were intimately involved with the egalitarian ideology of democracy. Although this thesis has its critics (Hoepfner et al. 1989; Etienne 1991; Nevett 1999: 27, 64), fifth-century houses were much more uniform in size than those of earlier times. This uniformity broke down in the fourth century. Olynthos is the only site where we have much evidence for the market value in drachmas of identifiable houses, but these data show that the fancier homes commanded significantly higher prices than the more typical ones (Nevett 2000). "Super houses" were built, like those at Eretria and Pella mentioned above; but more significantly for my arguments here, the median house size also increased by 128 percent. I have suggested (I. Morris 1992: 109-155; 1998) that the Hippodamian leveling ideology that Hoepfner and Schwandner see behind classical housing was part of a larger ideological system of restraint, involving burial, dress, private honorific monuments, and worship of the gods. This system was particularly rigid between about 500 and 425 BC, then relaxed, leaving more room for the rich to express

their status in material forms. The civic egalitarianism that took hold in the fifth century and survived in attenuated form through most of the fourth was certainly a leveling ideology, but it is important to recognize that the selected level was higher than that of archaic times. Houses were much larger and much better built. As with the changes around 700, ideology and economics likely worked together. The fact that the general increase in wealth meant that fifth-century houses were spacious and comfortable probably made it easier to persuade would-be aristocrats to conform to the norms; and the fact that further increases in wealth allowed typical fourth-century families to live in grander houses than even the richest men of the eighth century may have made it easier to relax the constraints on contemporary noblemen.

Beliefs about the nature of the good society and economic realities worked together throughout this period, both constraining and enabling Greek home construction. We cannot hope to disentangle these factors. The very obvious role of beliefs in the great changes around 700, 500, and 400 mean that there was no simple and straightforward relationship between the curves in Figure 5.1 and per capita economic output. But on the other hand, the factors discussed above mean that we cannot cut these curves loose from prediscursive economic forces either. The richest fifth-century Greeks probably spent less on their houses than they could have afforded to do, while their fourth-century successors may have spent more than was prudent. But the five- or six-fold increase in the size of houses shown in Figure 5.1 still provides proxy data for a significant and sustained increase in per capita consumption across this half-millennium.

HOUSE CONTENTS: POSTDEPOSITIONAL PROBLEMS

Houses not only got bigger between 800 and 300: their contents also got richer. Anyone who has excavated first-millennium houses knows this, but documenting contents is even more difficult than tracking house sizes (Nevett 1999: 57–61).[5] First, most of the objects that were left behind have decayed in the intervening two or three millennia. The written sources suggest that some fourth-century houses had elaborate textiles and wall paintings (Walter-Karydi 1994: 32–52), but nothing survives of these.

Second, people took most things with them when they left their homes. Abandonment processes vary greatly through time and space and are driven largely by cultural concepts of cleanliness. Some people burn down houses when they abandon them, symbolically "killing" them, leaving excellent deposits for archaeologists; others sweep them thoroughly; others still simply walk away, leaving their last meal on the table. Even within a single cultural

context, houses that were deliberately abandoned and those destroyed by sudden disasters leave very different deposits for excavators. Consequently, abandonment processes are an excellent example of Schiffer's C-transforms but straddle the line between depositional and postdepositional forces (Cameron and Tomka 1993; Hodder 1982a: 190–93).

Abandoned first-millennium BC Greek houses generally have clean floors. Even those houses destroyed suddenly by fire are relatively poor. There is much debate over whether this means that Greeks really were poor, as Herodotus repeatedly says by way of contrast with Persia (for example, 7.102), or whether they cleaned out their homes particularly thoroughly. The texts are ambiguous. The "Attic stelai" (*IG* I³ 421–30), inscriptions recording property seized from fifty or so Athenians condemned in 415, mention one silver object and silver coins (*IG* I³ 427.93; 422.182) but no other precious metals. There are plenty of bronze and iron tools, sold off for quite high prices, and lots of pots, including 102 Panathenaic prize amphoras (422.41–60). The editor of the texts concluded that "our record of the sale of confiscated property seems to show that there was little sense of personal luxury in Athens in the last quarter of the fifth century, even among men of wealth" (Pritchett 1956: 210). However, there is more to the evidence than this. David Lewis (1966: 183) pointed out that precious metals would have been sold off by weight at the going price, and would thus not show up in these texts, which are auction records; and Thucydides (6.60) and Andocides (1.52) tell us that many of the suspects fled Athens, presumably taking their most valuable possessions with them. The Attic stelai are the end products of abandonment processes every bit as complex as those of the excavated houses. Against them, we may set the equally complicated evidence of Aristophanes' comedy *Wealth*, produced in 388 BC. The slave Karion describes Chremylos's household after the blind god Ploutos (Wealth) has visited it: "Every pot in the house is crammed with silver and gold—you'd be amazed. . . . Every bowl, every plate and vinegar cruet, has turned to bronze, and all our rotting fish dishes are solid silver. Even our lamps are suddenly turned into ivory" (*Wealth* 808–15). We have no way to say which text evokes better the material experience of a rich Athenian house. At least in fifth- and fourth-century Attica, abandonment and post-abandonment cleaning were generally very thorough: At the Vari farmhouse near Athens, abandoned around 275 BC, even the several tons of roof tiles had been carried away (Jones et al. 1973: 361). Some inscribed house leases specify that the renters had to provide their own tiles, suggesting that careful tile removal was common (*IG* II² 2499.11–14, 30–37; XII 5.872.52, 53, 63, 94); and when the departing occupants did not take their tiles and wood, they could rely on looters to finish the job (Thucydides 2.14, 17; Lysias 19.31; *Hellenica Oxyrhynchia* 17 [12] 4–5).

That said, abandonment processes clearly affected different parts of the archaeological record in different ways (see also Pettegrew 2001). Broken pottery was virtually worthless, and no one would deliberately take it away from an abandoned house. Different attitudes toward cleanliness and garbage disposal will have had a huge influence on the quantities of pottery we recover from houses, but a pattern nevertheless emerges. At the Dema farmhouse near Athens, abandoned around 400 BC, red-figured pottery made up just 3 percent of the assemblage, and at the Vari farmhouse, just 2 percent (Jones et al. 1962: 89–100; 1972: 374–94). Ault meticulously published finds from five fourth-century houses from Halieis, where red-figured pottery averaged 3.9 percent of the minimum number of vessels, ranging from 1.6 percent in House C to 8.5 percent in House D (Ault, forthcoming b). But the garbage pits from a public building in the Athenian agora contained large quantities of red-figured pottery (Oakley and Rotroff 1992). We can probably conclude that red-figured wares were largely restricted to public and ceremonial contexts, with just a handful of sympotic vessels being used in most houses; and we can perhaps extrapolate from this conclusion to suggest that we can safely compare quantities of pottery found in different regions and periods, on the assumption that ceramics were far less vulnerable to abandonment processes than other categories of material.

But this is a very limited gain. When we turn from pottery to the base metals, we find enormous variation. The fourth-century "Priest's House" at Zostir held eighty-nine bronze coins, forty-eight bronze nails, fifty-six bronze fishhooks, nineteen bronze ornaments, and numerous lead fragments (Stavropoullos 1938), while most excavation reports mention no metal from houses. Some people are more careful than others, but can we really believe that there were houses where for centuries no one lost a coin or threw out a broken nail? There were, of course, variations in the richness of personal property; combined with different degrees of thoroughness in abandonment cleaning and reuse of the site, such variations might account for the observed differences. However, we probably need to introduce two further important sources of postdepositional distortion: the quality of excavation and publication. Fragments of broken metal tools and ornaments (particularly iron) can be hard to find, especially on salvage excavations under intense time pressure; and even if they are found, the excavator may not feel the need to provide exhaustive catalogues, especially in brief preliminary reports in journals like *Archaiologikon Deltion*.

The general impression from the site reports is that there was a massive increase in the numbers, variety, and expense of household goods between the eighth century and the fourth, but postdepositional factors interact in complex ways, undermining straightforward quantification. A thorough cleaning at or after the abandonment will leave only small scraps of pottery and metal. The less careful excavator may miss fragments of bronze and iron but will probably find most of the pottery. A brief publication will mention the more glamorous finds but will not discuss the rest of the assemblage. If we can safely assume that these factors randomly distort the relationships between the published record of excavations and the original material culture, then for the purposes of assessing overall change through time in the wealth of domestic goods, we can go ahead and make comparisons. If, on the other hand, we suspect that the distortions are nonrandom—for example, that some archaeologists dig and publish more thoroughly than others—we may not be able to address change in household goods at all. For instance, we may suspect that archaeologists working on small ninth- and eighth-century sites, where every excavated house is a significant addition to knowledge, will tend to work more carefully and publish their finds in more detail than those digging large fourth-century towns. Experiments have shown that systematic sieving not only increases the number of artifacts found but seriously changes the ratios between different types of artifacts (Payne 1972). We can expect to recover a larger proportion of the artifacts in use from sites destroyed violently than from sites abandoned peacefully, so we must be sure to compare like kinds of sites. If such destructions are more common in some periods or places than others, they may significantly bias our results.

For example, we might compare the total number of finds from Nichoria, at the beginning of our half-millennium, and Olynthos, at its end. These are among the best excavated and published sites of their respective periods, and both were dug by the same institution (the American School of Classical Studies at Athens). But even in these cases, digging and reporting styles changed so much between the work at Olynthos in the 1920–30s and that at Nichoria in the 1970s that we can make no simple comparison. At Nichoria, excavators recovered 125 metal fragments and 118 clay spindle whorls from an excavated area of roughly 1,700 square meters.[6] Unit IV-1 was abandoned peacefully and thoroughly cleaned out, but eighth-century Unit IV-5 burned down (McDonald et al. 1983: 32, 39, 49–50). Olynthos was also destroyed by fire, when Philip II of Macedon sacked it in 348. Several thousand square meters were cleared. The published lists

of small finds fill eight large volumes, and include 4,402 bronze, silver, and gold coins, 1,278 terra-cottas, and 457 clay lamps, as well as many fragments of sculptures, worked stone basins, altars, millstones and querns, mortars, olive presses, and so on. Finds of iron, bronze, and lead included braziers, keys, figurines, weights, door knockers, meat hooks, and a whole range of other items (Robinson 1931–52; Robinson and Clement 1938; Robinson and Graham 1938). The 1928 season alone produced 25 spindle whorls, more than 200 spools, and 793 loom weights. These categories of artifacts were published in less detail (see Robinson and Graham 1938: 307–54). Styles of excavation and publication changed drastically between the 1920s and 1970s, adding to the difficulties of comparison, although we might note that the 1987–89 excavation in Olynthos House B.VII.1 produced finds very like those from the prewar digs (Drougou and Vokotopoulou 1989).

What should we make of this comparison? Despite the higher level of excavation technique at Nichoria, vastly more artifacts were recovered from Olynthos. Even after taking into consideration the full range of postdepositional factors, there is no way to avoid the conclusion that fourth-century Olynthians had far more things, and far more sophisticated things, than eighth-century Nichorians. But we cannot move on to make such a precise quantification of the differences as with the house sizes unless we are prepared to make a series of unjustified assumptions. I suggested above that typical fourth-century houses were five or six times as big as those of the eighth-century. Did the typical fourth-century house also have a collection of household goods five or six times as rich as that of the eighth century? Probably. Was it ten times as rich? Perhaps. Twenty times? Probably not. But these are only guesses, and the margin of error is compounded when we take depositional factors into account.

HOUSE CONTENTS: DEPOSITIONAL PROBLEMS

As noted above, beliefs about purity and dirt play a massive role in abandonment processes. They also influence what people do with refuse while they still occupy houses. If it was normal simply to throw garbage out of the back door, to lie around in the yard, we might find little of it; if, on the other hand, it was normal to dispose of it in carefully dug pits, we are likely to find far more. The literary sources suggest that purity beliefs hardened in Greece around 700 BC (R. Parker 1983); I link this to the widespread division of space into discrete domestic, funerary, and divine spheres at just this point (I. Morris 1987: 189–96). However, there is little

indication that this practice had much impact on domestic garbage disposal. With the exception of dumps of sacrificial material in sanctuaries, garbage pits were rare throughout the first millennium. Abandoned wells and cisterns were popular dumps, as the Athenian Agora shows clearly (Brann 1962: 125–31; Sparkes and Talcott 1970: 383–99; and annual reports in *Hesperia*), but Brad Ault's (1999) publication of large groups of sherds from *koprones* (dung-storage bins) at Halieis suggests that much household debris was simply thrown out. Bintliff and Snodgrass (1988; Snodgrass 1994) suggest that great quantities of domestic refuse were carried out to the fields with manure, to form the "halos" of medium-density artifact scatters found around many settlements.

Changing ideas about purity may not have had much impact on the disposal of artifacts, but changing ideas about status may have been very important on the actual accumulation of household goods. Pritchett (see above) argued from the Attic stelai that fifth-century Athenian houses were modestly furnished, which certainly fits with the larger pattern of fifth-century restraint (I. Morris 1992: 118–27). The literary sources refer far more often to luxurious interiors in the fourth century than in earlier periods (Walter-Karydi 1994: 32–52), and nonportable luxury goods like mosaic floors only become common after 400 BC (I. Morris 1998: 69–70, 71, 73, 81). We might hypothesize that cultural factors restrained lavish consumption and display in the fifth and perhaps also the sixth century, before loosening in the fourth. The issues are much the same as with house size: After we have allowed for postdepositional factors, should we conclude that the increase in household goods between the eighth century and the fourth was caused by rising wealth, or by changes in beliefs against a stable economic background? It seems to me that ideological factors would be most important at the upper end of the scale. Pericles probably felt less comfortable than Alcibiades offering his guests wine from gold cups or having famous artists paint his house, and fifth-century values seem to have suppressed such activities. But would egalitarianism have discouraged a successful farmer from buying extra pottery, or perhaps replacing plain table ware with black-glaze? Or installing a stone drain to keep water away from his foundations? The attitudes in the literary sources that I have described as "middling" (I. Morris 2000: 113–85) seem perfectly consistent with a generally rising standard of living. There is no obvious reason to suppose that ideology had any significant impact on the kind of changes in household assemblages that we see when we compare Nichoria and Olynthos, and we can probably assume that the rise in living standards (however vague our quantification must be) was driven by increases in per capita economic output.

Conclusions

House sizes increased something like five- or six-fold between 800 and 300 BC. This represents a dramatic improvement in the standard of living, particularly when we factor in improvements in construction, drainage, and illumination. Fourth-century Greek houses were large and quite comfortable, even by the standards of developed countries in the early twenty-first century. It is hard to say how well furnished they were, but the impression (presently it can be little more than that) is that classical household goods were far richer than those of archaic times. Again, a five- or six-fold increase may not be far from the mark.

These observations are only a beginning; we need to compare the evidence for housing with that for changes in stature, nutrition, mortality, and morbidity. We also need to go beyond simply asking, "How well off was the average person?" (MacKinnon 1994: 265) to break down overall well being into subcategories such as children, the elderly, men and women, free and slave, rich and poor. Furthermore, we must look at the top and bottom quartiles as well as the mean and median figures. But the initial impression created by the data is that for most people in Greece, living standards increased substantially across this half-millennium. This increase is all the more remarkable when we bear in mind that population expanded enormously in just the same period. Table 5.3 shows the estimates reached by two of the most rigorously conducted settlement surveys of the 1990s: the Kea survey and the Argolid Exploration Project. Population increased roughly tenfold between the eighth century and the fourth. Economic historians commonly assume that prior to the Industrial Revolution, living standards improved only at a glacial rate, because any significant increases in per capita economic output were quickly converted into extra mouths to feed. Demographic expansion would outrun resource expansion, leading to famine and disease until a new equilibrium was established. But in Greece, across a long period of time (twice as long as the period that has elapsed since the Industrial Revolution began in earnest), population and living standards apparently increased together. As Saller notes in his discussion of Rome (Chapter 11), ancient economic expansion was trivial compared to the changes of the nineteenth and twentieth centuries. But compared to the expectations mainstream economists seem to have about premodern economic performance, and the expectations that the Finleyan model leads to, economic expansion was massive, sustained, and desperately in need of explanation.

The first step is to link increases in the standard of living to quantified changes in economic output per capita. This will be far from easy. If a

TABLE 5.3
Population estimates from the Kea survey and the Argolid Exploration Project

Period	Population	Relative Size[1]
Koressos on Kea		
900–700	100?	1.0
700–480	570–810	5.7–8.1
480–323	1020–1455	10.0–14.5
323–31	495–600	5.0–6.0
Southern Argolid		
750–650	1100	1.0
650–480	5880	5.3
480–c. 200	10,855	9.9
c. 200 BC–AD 100	4570	4.2

SOURCES: Cherry et al. 1991: 340; Jameson et al. 1994: 544–45.
[1]Treating first period as 1.0.

typical fourth-century house cost five or six times what one of the eighth-century cost, that does not mean that income per capita had expanded 500–600 percent.[7] Over time, consumption bundles change. If our income doubles, we probably would not spend twice as much on bread as we did before: Spending on certain goods is relatively income-inelastic, while spending on others (particularly luxuries) responds vigorously to changes in income. If we could draw up a cross-culturally valid table of income elasticities of demand, we could simply multiply the likely increases in Greek spending on housing to generate a figure for overall changes in per capita income. But we cannot. A huge variety of historically specific forces, both economic and cultural, shape what people will do with changes in income. Around 1800, English working-class families typically spent 4–5 percent of their income on housing (Horrell 1996: 580), but by 1900 the figure had risen to somewhere between 9 and 15 percent (Burnett 1986: 147). A hundred years later in California, 40 percent is not an uncommon figure. The only way to move from specific indices of changing standards of living to underlying trends in per capita economic output is by working out a macroeconomic model including food, housing, ritual, and the complex ways in which wealth was siphoned off to pay for the state's building activities, sacrifices, and wars. This exercise will necessarily be largely speculative but can at least establish some basic parameters. Keith Hopkins's work on Rome (1980, 1995/96, 2000) and Daniel Jew's on fourth-century Athens (1999) might provide useful frameworks.

The second step is to try to explain what I suspect will turn out to be a surprisingly high level of economic growth. This calls for new models, probably owing more to economic historians like Douglass North (1981, 1990) and development economists like Debraj Ray (1998) than to Weber or Finley (or, for that matter, to the modernist ancient historians of the 1890s). Finley struggled to explain why there was no capitalist takeoff in the ancient world, and by extension why antiquity was so backward. The evidence for living standards seems to suggest that we should in fact explore why there was (by preindustrial standards) such sustained growth, and what it contributed to the very long-term economic development of Europe (see Greif, Chapter 12).

Finally, returning to the core questions of this book, such calculations make possible direct comparisons between the economic performance of ancient Greece and that of other regions of the Mediterranean. The depositional and postdepositional factors affecting the archaeological record in different periods and regions vary significantly, and much careful empirical work is still needed. But there is in principle no reason why archaeology should not provide a unified economic history of the Mediterranean, once we ask appropriate questions. In fact, the study of Greek standards of living and economic growth demands such a large-scale study, because without this comparative base, we have no way to know to evaluate the Greek experience. Did the Greeks expand their economy faster than other ancient peoples? Or were they in fact typical of a much larger pattern of cyclical economic growth and contraction (Wrigley 1988; E. L. Jones 2000; Goldstone 2002)? If the latter, then ancient economic historians will be challenging one of the fundamental orthodoxies of modern economic historians, that between the rise of the state in later prehistory and the Industrial Revolution after AD 1750, agrarian economies were essentially static (Lucas 1998). The ancient economy matters.

Notes

1. Renfrew and Bahn (2001: 41–60) offer a good introduction; and Kristiansen (1985), Schiffer (1987), Nash and Petraglia (1987), and Lucas (2001: 146–99) give more advanced accounts.

2. See the essays collected in *Explorations in Economic History* 24.3 (1987).

3. Lang (1996), Mazarakis Ainian (1997), and Nevett (1999) provide catalogues of varying degrees of completeness for the period 1100–300 BC, with full references.

4. Assuming that these rooms all belong to a single structure: see Mazarakis Ainian 1997: 171–74.

5. I would like to thank Doctors Demetrius Schilardi and Petros Themelis for giving me the opportunity to excavate Dark Age, archaic, classical, and Hellenistic house deposits at Koukounaries and Eretria in the 1980s.

6. Measured from the plans of Areas III and IV in Coulson 1983: 10–11, 20–21, 45. I focus on excavated area rather than the volume of earth removed because the depth of the deposit is itself a function of the level of activity on site.

7. Our data for house prices are sketchy. Nevett (2000) reviews the inscriptions from Olynthos, documenting a range of prices from 230 to 5,300 drachmas. Also in the fourth century, Isaeus (2.35) refers to an *oikidion* being worth no more than 300 drachmas. If Olynthos House A.V.10 went for 5,300 drachmas, a typical fourth-century house must have cost at least 1,500 drachmas (Hoepfner and Schwandner [1994 (1986)] opt for 3,000 drachmas); and if the poorest fourth-century houses, like that from Block II at the Silen Gate on Thasos (Grandjean 1988), were the kinds of thing Isaeus had in mind as a 300-drachma *oikidion*, then the price (in Athenian fourth-century drachma equivalents) of typical eighth-century houses probably was in the 150-drachma range. As a very rough estimate, spending on housing probably increased tenfold across these five centuries.