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Discussion and Debate: Fusing the Horizons, or Why Context Matters: The Interdependence of Fieldwork and Museum Study in Mediterranean Archaeology

A Response to Robin Osborne's 'De-contextualising and Re-contextualising: Why Mediterranean Archaeology Needs to Get out of the Trench and Back into the Museum' (*JMA* 28.2, December 2015)

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Abstract

In a recent JMA article (JMA 28.2, December 2015), Robin Osborne argued that Mediterranean archaeology places too much emphasis on fieldwork and too little on museum study. In doing so he has set up an antithesis between two kinds of archaeological practice: the former 'in the field', undertaken chiefly by specialists in prehistory, and the latter in the museum, undertaken principally by Classical archaeologists who are also experts in the traditional subjects of sculpture and vase painting. I argue that this antithesis is at best misleading, and a poor guide to how best to turn material evidence into historical knowledge. These issues are explored in relation to a set of case studies where recent survey and fieldwork have shed light on old 'museum' material, and where a reappraisal of that material has in turn affected research design in the field as well as historical interpretation. This study concentrates on the results of a number of fieldwork projects (both excavation and survey) covering Archaic and Classical material in eastern Crete, with a particular focus on first on pithoi (storage jars) found in the excavation of houses and then terracotta plaques from various sanctuary deposits. New fieldwork, conducted to modern standards, when combined with a reappraisal of older 'museum' material can, when conducted using a range of both new and traditional methods, yield new insights. This combination can offer a true 'fusion of horizons', in Gadamer's (1975) sense.

Keywords: Athenocentrism, Classical, context, Crete, iconography, household, pithos (storage jar), terracotta plaque

Introduction: The Museum and 'the Trench'

In a recent article in this journal, Robin Osborne (2015) makes a number of claims about the relative merits of museum versus field research, particularly field research conducted through excavation. In so doing he implies that Mediter-

anean archaeology (though there is a continual slippage in his text between 'Mediterranean' and 'Classical' archaeology, a point I return to later) faces a choice: it is to be primarily museum focused or field focused? Is it to give primacy to the object in the trench, or to the object in the

museum case? Osborne argues that Appadurai's (or rather Kopytoff's [1986]) notion of object biography renders the emphasis on 'archaeological context' somewhat redundant, since the object is always changing context, and within an overall 'biography' no one context (the trench) can be given greater weight than any other (the museum display case).

Of course, Osborne is right when he says that an object can have all kinds of contexts, and that the kinds of contexts we prefer depend in part on the questions we ask of the material. Nonetheless it is not entirely clear where his argument ultimately leads. If all contexts are equal, and if different contexts create different universes of objects (based on iconography, attribution, shape and so forth), does this lead to a kind of archaeological (or art-historical) relativism, where the significance of an object depends on its *latest* context? Or is this argument a belated acknowledgement of Gadamer's (1975) concept of the 'fusion of horizons', where the history of interpretation of an object or class of object necessarily becomes part of the interpretation we judge to be best? It would be more generous, of course, to infer the latter. But even if it was Osborne's intention to suggest we adopt a broadly 'Gadamerian' approach to the way in which we approach objects, the manner in which Osborne sets out his argument still betrays a fundamental misunderstanding of what most practising archaeologists mean by 'context' and a misunderstanding of the reasons why so much weight has been placed on archaeological context in recent years. It is also at odds with a view recently put forward by two Classical archaeologists who have worked and continue to work in the field. Donald Haggis and Carla Antonaccio (2015a: 1) fear that

classical archaeologists have become more comfortable with ideas, and strands of discussion and debates, than with the material itself, while in effect teaching a new generation of students that theory is more important than field practices.

And they go on, perhaps inadvertently, to summarise what I take to be Osborne's position (Haggis and Antonaccio 2015a: 2):

The mantra of the twenty-first century is that we have done enough fieldwork, found enough stuff, know what it is, and now we simply need to sit back and think about what it all means, while dissuading our students from taking on ambitious fieldwork and the study of primary assemblages for dissertations and field research.

These authors also maintain that archaeological context is central to any interpretation of objects that recognises their complexity—that is, to any kind of interpretation that would be consistent with Gadamer's principles. I share their view, and would argue that Classical archaeology is at its best when the results of fieldwork (conducted to modern standards) are integrated with a concrete reappraisal of older finds (as Osborne recommends).

Another odd feature of Osborne's argument is the timing. Why claim now, in 2015/2016, that university museums have been 'neglected'? Britain's two largest university museums, the Ashmolean in Oxford and the Fitzwilliam in Cambridge, have recently spent much time and effort refurbishing their Classical collections. Both refurbishments have been unqualified successes. The new displays at the Fitzwilliam allow us to place E.D. Clarke's acquisition of a Roman-era caryatid from Eleusis or the inscriptions Robert Pashley brought back from Crete in the context of the history of ideas. These displays have been very thoughtfully put together by Lucilla Burn and Anastasia Christophilopoulou, the latter an experienced field archaeologist. In this respect she is following in the footsteps of Winifred Lamb, perhaps the best British field archaeologist to work in the Mediterranean during the 1920s and 1930s, who was responsible for the earlier *CVA* (*Corpus Vasorum Antiquorum*) catalogues to which Osborne refers. Osborne's antithesis between a 'museum'-focused and a 'field'-focused discipline may then be a false one.

As for the Ashmolean, the new displays allow us to see more clearly how objects relate both to the cultures that produced them and to the scholars who studied them. The new displays of Greek material at the Ashmolean Museum in Oxford reflect, to some degree, the new interest in Mediterranean ‘connectivity’ (Horden and Purcell 2000; cf. Broodbank 2013: 445–610) and contemporary debates about identity. ‘Greek’ is no longer a self-evident category, such that we can confidently make generalisations about ‘the Greeks’ as users of red-figure pottery or free-standing sculpture. The rooms devoted to Cypriot antiquities, the Aegean Bronze Age and Archaic and Classical Greece emphasise the material diversity of various Greek-speakers in the Bronze and Iron Age Mediterranean, while also shedding new light on the intellectual biographies of J.L. Myres, A.J. Evans and J.D. Beazley, respectively. The Ashmolean’s refurbishment thus also serves to provide us with a new kind of intellectual history.

Osborne is then clearly exaggerating a little to make his point. Polemical exaggeration has its place in robust academic debate of course—we would all be poorer without it. It is where exaggeration becomes misrepresentation that his views must be questioned. His argument is impelled in part by a misunderstanding of what fieldwork entails and what the word ‘context’ implies for most practising field archaeologists. These misrepresentations cannot remain unchallenged.

Museums: Art, Ethnography, Context and ‘the Classical’

Before I get down to refuting the central claims of Osborne’s argument, let me first emphasise that I am not in any sense anti-museum. The only point where I am completely at odds with Osborne is in his use of the term ‘theology’ when applied to Greek religion (see also Osborne 2011: 185–215)—a religion without any central revelation and without any body of sacred text

upon which a systematic theology could be constructed (Parker 2011). I agree with most of what Osborne has to say about the role of university museums. I agree that university museums have (until recently) been neglected, and that those universities lucky enough to possess their own collections (including sherd collections, whose curation requires archaeologists with field experience if their full pedagogic value is to be realised) have not made the best use of them. I agree that collections of casts provide opportunities to explore such matters as polychromy in ancient sculpture. I agree that museum displays make iconographic comparison that much easier, and that renewed iconographic study of such objects as Classical Athenian *pelikai* can yield new insights; and I agree that the nature of collections *as collections* is a worthwhile object of both archaeological and historical study.

There is, however, one telling omission in his discussion: the rarity of the juxtaposition of the ethnographic and the classical in any museum display. *Primitive Art in Civilized Places* (Price 1989) made the important observation that, in major Western museums, art and classical objects are ‘aestheticised’ (treated as objects that are self-evidently beautiful, and require no explanation), whereas ethnographic material is ‘contextualised’ (that is, treated as the outcome of specific cultural practices, different and alien from our own experience). This antithesis (between the aestheticisation of the classical and the contextualisation of the ethnographic) still exerts a profound gravitational force in scholarship. It is an antithesis embodied in institutions. Those familiar with New York will know of the two museums on the opposite sides of Central Park: the Metropolitan Museum on the east side, and the American Museum of Natural History on the west side. The former is devoted to ‘art’ (and everything Greek and Classical in this museum is treated as ‘art’), the latter to placing the material culture of Native Americans in relation to natural history.¹ For those who know (or knew) Oxford, this antithesis

is best expressed in the very different ways in which the (older, pre-refurbishment) displays in the Ashmolean and the displays in the Pitt Rivers museum treat objects.

This tendency to aestheticise rather than contextualise Classical objects can also be seen in the biography of one of Osborne's main examples, the Sarpedon krater, potted by Euxitheos and painted by Euphronios. When this vase first came to light it had no agreed provenance (Boardman 1975: 32-33; Von Bothmer 1976; Watson and Todeschini 2006: ix-xx). We now know where it came from. Yet Osborne states (2015: 244):

When the Metropolitan Museum in New York in 2008 returned Euphronios's Sarpedon krater, which it had acquired in 1972, to Italy for display in the Villa Giulia Museum in Rome [...] it earned a positive press; yet no new knowledge was created...

This is just plain wrong, as Osborne inadvertently acknowledges when he admits that there was “a substantial or highly probable chance” that it had been illegally excavated from an Etruscan tomb at Greppe Sant' Angelo' (Osborne 2015: 244; see also Watson and Todeschini 2006: 206).

For Greppe Sant' Angelo is one of the cemeteries of ancient Caere (Cerveteri): the return to Italy allowed us to place this calyx krater in two important but related contexts. First was its archaeological context—at least in relation to Caere; second was in the context of other late Archaic Athenian red-figure imports to Etruscan cities, including other examples by Euphronios whose provenance was Cerveteri (Beazley 1963: 13-17). Returning the Sarpedon krater to Italy allowed us to see it as part of a distinct archaeological *pattern* that its retention in New York had obscured. Such scholarly recontextualisation provides a glimpse into the *agency* of the krater as an object that was purposely made to go on its travels (Whitley 2012: 586-91; in press). The Sarpedon krater is an object whose biography is part of its meaning, both in modern

and in ancient times. Our understanding of its biography depends on our understanding of its context of discovery (*Fundort*—see below), that is, its *archaeological* context in an Etruscan tomb near Cerveteri, not a house, sanctuary or tomb in Athens.

'Context' in Context

Most practising field archaeologists who are also readers of *JMA* will find the following discussion of what we mean by context to be somewhat laboured. But Osborne's insistence on talking about the 'context of the archaeological trench' betrays a misunderstanding that is now widespread amongst ancient historians—historians who (on the one hand) are keen to make use of archaeological evidence but who also (on the other) are generally without field experience. It is for these scholars that the word 'context' (in its archaeological sense) stands in urgent need of clarification.

Scholars mean different things by the word 'context'. For ancient historians, it is perhaps easiest to start with the distinction that epigraphers routinely make between *Fundort* and *Standort* (to use the German terminology). *Fundort* refers to the context in which an object, in this case an inscription, has been found, which may not of course be the original context of use, which is *Standort*. So, for example, the fragments of the late fifth century BC law code of Nicomachos in Athens may not all have been found near the Royal Stoa in the Athenian Agora—they may have been found in numerous *Fundorten* scattered across Athens—but that is what is inferred as being their *Standort* (Rhodes 1991). Contrary to what Osborne implies, archaeologists who are also excavators are very much interested in an object's biography (*sensu* Appadurai 1986b; Kopytoff 1986) and even if they do not use specialist terms, they make routine distinctions between context of discovery and context of use. I can think of no current field archaeologist who would make

the elementary mistake that Osborne attributes to archaeologists as a group: that we are blind to the previous and subsequent contexts of the objects we find; and that we imagine archaeological context provides the only real indication of the changing uses and meanings of any one thing in the archaeological record.

The point about archaeological context is not that it provides an unambiguous indication of ancient use, meaning or agency but rather that it provides some kind of a datum that relates securely to the ancient rather than the modern world—a point in an object's long biography where it can be placed firmly in time and space. An object does not stand by itself: it is part of an assemblage of other objects (which in turn is part of a deposit, which is part of site), not simply a thing in itself. Objects exist within a web of connections, and it is these connections that are revealed by the context of excavation. These webs are in turn what archaeologists refer to as an object's entanglements (Hodder 2011), which in turn do not merely reflect but constitute the culture of the time. Classical Greek culture was a web of material contexts as much as it was anything else. Archaeological contexts reveal social and behavioural contexts (albeit refracted through different taphonomies) that a museum display can never reveal.

When field archaeologists talk about 'context' they rarely mean 'the context of the archaeological trench'. A distribution map of finds by *trench* would tell us very little. Whether objects were found in houses, in tombs or in sanctuaries; whether within those places in floor levels, in pits or in middens; and what these associations to objects within these contexts are—all these by contrast can tell us quite a lot, especially when we can apply some kind of quantitative analysis to the kinds of objects we find. An excellent recent example of the value of this contextual approach is Lynch's (2011) study of the finds from Well J 2:4 in the Athenian Agora. Lynch's study combines archaeological context with a concern for the nature of the objects themselves.

She draws an interesting contrast between the kinds of late Archaic red-figure objects found in domestic contexts in Athens with the kinds of Athenian imports found in Etruscan tombs (those found in Athens, that might relate to 'the symposium', being much smaller in size, for example). Her conclusions challenge directly arguments put forward by Osborne (2001).

Lynch's study tried to place late Archaic red-figure cups in relation to wider concerns—in her case the wider issue of 'the symposium' (and so the role of various kinds of commensality within the Greek-speaking Mediterranean). The history of research into this problem illustrates precisely why so many archaeologists working in the field have come to place greater and greater emphasis on archaeological context in order better to understand this phenomenon. Previous studies, following the conventions of museum-based study, had relied primarily on literary sources and iconography to reconstruct what took place within a typical 'Greek' symposium (Athenian imagery, as always, being taken as typically 'Greek'). Objects were placed in the context of other vases painted by the same painter, or in that of vases that had the same shape or the same iconography, but little effort was made to relate these to provenance or archaeological context. Lissarrague (1990), for example, took no account of the fact that most of the Athenian cups, kraters and other vessels on which he placed so much emphasis came from Etruscan tombs. Lynch (2011) exposes the disparity between Athenian red-figure vessels in domestic contexts and those in Etruscan funerary ones. A purely museum-based approach could not have uncovered this disparity.

If we want to move on from 'the symposium' and embrace a fully integrated approach to ancient commensality then archaeological context is, at the very least, a *sine qua non*. A minimum requirement is a full analysis of patterns in the record of the kinds of non-artistic material that traditional classical archaeology has neglected: seeds and animal bones (e.g. Kyriakou

and Tourtas 2015). The potential for exploring the relationship between the consumption of animals or plants, and the patterns we find in the deposition of ceramic vessels for eating or drinking can only be pursued through further excavation. Excavations that focus on sanctuaries, such as Mt Lykaion in Arcadia (Romano *et al.* 2014), allow us to link the deposition of animal bones with that of pottery, and thus provide information on public and ritual commensality and their relation to political communities. Excavations that focus on settlements, such as those at Molyvoti in Greek Thrace (Arrington *et al.* 2016) or at Azoria in Crete (Haggis 2014; 2015), furnish us with the necessary contextual information that links the consumption of food with the wider use of material culture.

It is for these reasons that many have come to describe the best contemporary field practice in Classical archaeology as examples of ‘contextual archaeology’ (Haggis and Antonaccio 2015a; 2015b). That contextual analysis such as Lynch’s often undermines the accepted views of traditional, museum-based Classical archaeology is, obviously, not a point that Osborne wishes to emphasise. Osborne wants us to believe that

museum study has been neglected, while field archaeology on Classical sites in the Mediterranean has been encouraged. There are certainly many excavations taking place in the Mediterranean at present, and most of these are (or have been) rescue excavations undertaken by members of relevant archaeological services linked to the nation states of Greece, Italy or Spain (in the Aegean these were, until recently, summarised in an annual report, *Archaeology in Greece*).

It is the volume of finds from such excavations, in advance of new roads, railways and other developments, that is the principal cause of the storage crisis in Mediterranean lands (to which Osborne alludes). The finds generated through research excavations have a much smaller impact on storage. Few of these research excavations in Mediterranean lands focus principally on the Classical period. I have compiled a rough-and-ready table of the period focus of field projects, interim results of which have found their way into *JMA* (Table 1, below). This provides some indication of the overall focus of at least Anglophone research over the last three decades.

The table shows that Mediterranean archaeology and Classical archaeology are not the same

Table 1. Chart of the period focus of contributions to the *Journal of Mediterranean Archaeology* from 1988 until the first volume of 2016 (divided by decades). Period focus relates to the principal periods covered by articles (so historiographic contributions on Arthur Evans or ‘the Minoans’ belong to the Bronze Age), however short. The category ‘other’ represents those contributions that are not concerned with periods in the past *as such* but rather with overall methodology, historiography (except when period focused), archaeological politics and general theory.

JMA decade by decade	<i>Neolithic and earlier (Mesolithic, Palaeolithic)</i>	<i>Bronze Age</i>	<i>Early Iron Age</i>	<i>Archaic to Classical</i>	<i>Post-Classical (Hellenistic, Roman, Medieval)</i>	<i>Other (methodological, theoretical, political, historiographic)</i>	<i>Totals</i>
1988–1990	10	7	2	9	2	0	30
1991–2000	27	36	5	5	4	30	107
2001–2010	14	38	8	9	9	21	99
2011–2016	10	16	12	3	12	9	62
Totals	61 (20.5%)	97 (32.5%)	27 (9%)	26 (8.7%)	27 (9%)	60 (20.1%)	298

thing. There seems to be little evidence here to support Osborne's (implied) contention of a heavy concentration of field research into the Classical Mediterranean. There are 185 contributions with a primarily prehistoric focus (Neolithic and earlier, Bronze Age, Early Iron Age), representing 62% of contributions. There are only 53 that focus on 'historic' periods (Archaic to Medieval), representing 17.8% of contributions, and of these only 26 (8.7%) have a Classical (or Archaic) focus. Contributions to *JMA* seem to represent field interests—Bronze and Iron Age Cyprus, Nuragic Sardinia and 'Minoan' Crete—that are at variance with the principal concerns of Classics and Ancient History (Athens and Rome).

Now, of course one could argue that contributions to *JMA* are unrepresentative. After all, the American School of Classical Studies at Athens has a longstanding commitment to excavation at the major Classical sites of Corinth and the Athenian Agora. But elsewhere in Mediterranean archaeology articles on Classical *archaeological* subjects are rare, and those that concentrate on the Classical period proper, which is where Osborne's chief examples (sculptures by Praxiteles and red-figure *pelikai*) belong, are rarer still. Indeed, only two recent British field projects have concerned themselves with the Classical period in the narrow sense used by Osborne: the excavations at Olynthos by Zosia Archibald, Bettina Tsigarida and Lisa Neveit (begun in 2014), and one season (2007) of excavation at Praisos in Crete (Whitley 2011).

The excavations at Olynthos are ongoing, and their focus falls both on the household and on a multi-scalar understanding of the use of objects in various contexts. As such the excavators have to be alive to the implications of recent advances in methodology, such as the application of a quantitative and spatial approach to archaeobotanical remains for understanding the household (e.g. Margaritis 2015). Over the past 30 years or so, there has been a thorough reappraisal of this topic, undertaken through a study of earlier lit-

erature, excavation archives and older finds now held in museums (Neveit 1999; Cahill 2002; papers in Westgate *et al.* 2007). The directors of the Olynthos project seek to address questions that cannot be dealt with simply through reappraisal. Rather, project directors have to frame both their research questions and their methodology in such a way as to address questions that a reappraisal of the archive cannot.

Research at Olynthos is very different from research in Crete. Questions about the Classical household in Olynthos are, partly at least, framed by ideas about 'the household' and of male and female space that derive from Athenian literary sources (Neveit 1999). Houses in Olynthos do resemble Classical Athenian houses; both are arranged around central courtyards. To some extent—and even though the excavators at Olynthos have been and still are engaged in a thorough and detailed critique of this set of ideas—this 'Athenocentric' perspective still sets the agenda for the study of the 'Classical house', an agenda where Athens is presumed to be the 'norm' for anything 'Greek', unless proven otherwise.

Getting away from Athens: The Importance of Crete

Athenocentrism—the idea that Classical Athenians were in some sense the exemplary 'Greeks', and that Athenian material culture (or 'art') defines what is Greek in the Classical period—runs throughout Osborne's article. Praxiteles was an Athenian, and his sculptures derive from an Athenian tradition; similarly, red-figure *pelikai* are quintessentially Athenian products. It is slightly alarming that Osborne's participation in the refurbishment of the Fitzwilliam's galleries seems, if anything, to have reinforced his Athenocentrism. There are plenty of examples of how distinctly different other regions were from an Athenian 'norm' to be found in the Fitzwilliam's collections. There are, for example, no Athenian parallels for the lead figurines from Artemis Orthia near Sparta.

This Athenian bias runs deep in Classical studies. The word ‘Athenocentric’ is misleading, as it does not simply reflect a bias caused by ‘taking the Athenian point of view’. For any text-based historian, Athenocentrism in this sense is, to some degree, inevitable: most of our Classical textual sources are Athenian, and Attica has produced the bulk of our Classical inscriptions. Athenocentrism, however, is more than a bias caused by our sources. For example, the fact that Egypt has produced most of our Hellenistic and Roman papyri has not produced an equivalent ‘Egyptocentric’ or ‘Oxyrhynchocentric’ bias. Athenocentrism is rather a kind of Atheno-teleology, one that sees democratic Classical Athens both as the inevitable outcome of Archaic Greek history and as the exemplification of the Classical achievement. It is precisely to redress the Athenocentric bias in both archaeology and history that so much attention has been given to Archaic and Classical Crete in recent years. Historians (Seelentag 2015; Gagarin and Perlman 2016) have undertaken a thorough reappraisal of Cretan epigraphy. Cretan political institutions, and the Cretan *polis*, turn out to be something very different from a dim reflection of ancient Athens (Vlassopoulos 2007). Similarly, for archaeologists, it is the very un-Athenian austerity of Cretan material culture that has proven so attractive (Erickson 2010; papers in Pilz and Seelentag 2014); an austerity that directly contradicts art-historical notions of ‘Greekness’ that still pervade much recent scholarship (e.g. Osborne 2011).

Crete really is the place that undermines any Athenocentric idea of ‘the Classical’. For one thing, Cretan houses remain much simpler than mainland ones throughout the period concerned (Westgate 2007). For another, Cretans made minimal use of Athenian material culture. Excavation of late Classical and early Hellenistic levels in Praisos in 2007 (Whitley 2011) have uncovered 24,147 pottery sherds, of which only one is a piece of Attic red-figure ware. This sherd represents 0.004% of the total ceramic

assemblage. Praisos thus provides a very different view of the significance of red-figure from that provided by museum displays such as that in the Fitzwilliam, or implied by any standard textbook on Greek art.

At Praisos (Figure 1), no less than at Olynthos, we had to be alert to frame our research questions for both excavation and survey in the light of what had gone before (see Whitley 2011; 2015). Excavators have to have much wider interests than simply what goes on in the ‘excavation trench’. They have to be aware of the variety of intellectual traditions that have informed previous research and alive to the methodological consequences of changing research questions (Whitley 2015): the objects that are found are not simply placed in the context of the deposit in which they were found, but in multiple material and intellectual contexts and multiple frames of reference.

This can be illustrated by an example of a kind of vessel rarely discussed in textbooks on ‘Greek art’—the *pithos* or storage jar, a shape that has particularly strong links to the household (Ebbinghaus 2005). One of the finds that intrigued us most when we came to study our material from the 2007 excavation season at Praisos was a small *pithos* found abandoned on a floor. The abandonment must date to the site’s destruction by Hierapytna ca. 140 BC (Whitley 2015: 40–42). This *pithos* turned out to be much older than the deposit in which it was found; it was the product of a workshop based in Afrati, active in the years around 600 BC (Brisart 2007). It exemplifies the utility of the concept of the ‘object biography’ (Appadurai 1986b; Kopytoff 1986), on which Osborne has placed so much emphasis. Its discovery also prompts a re-evaluation of older finds. Archaic *pithoi* are relatively common in Hellenistic destruction or abandonment horizons on Crete (Vogeikoff-Brogan 2011; Galanaki *et al.* 2015: 326–28): here the *pithos* comes to ‘embody’ the household in a much stronger sense than does the house itself (as many of the *pithoi* are older than the

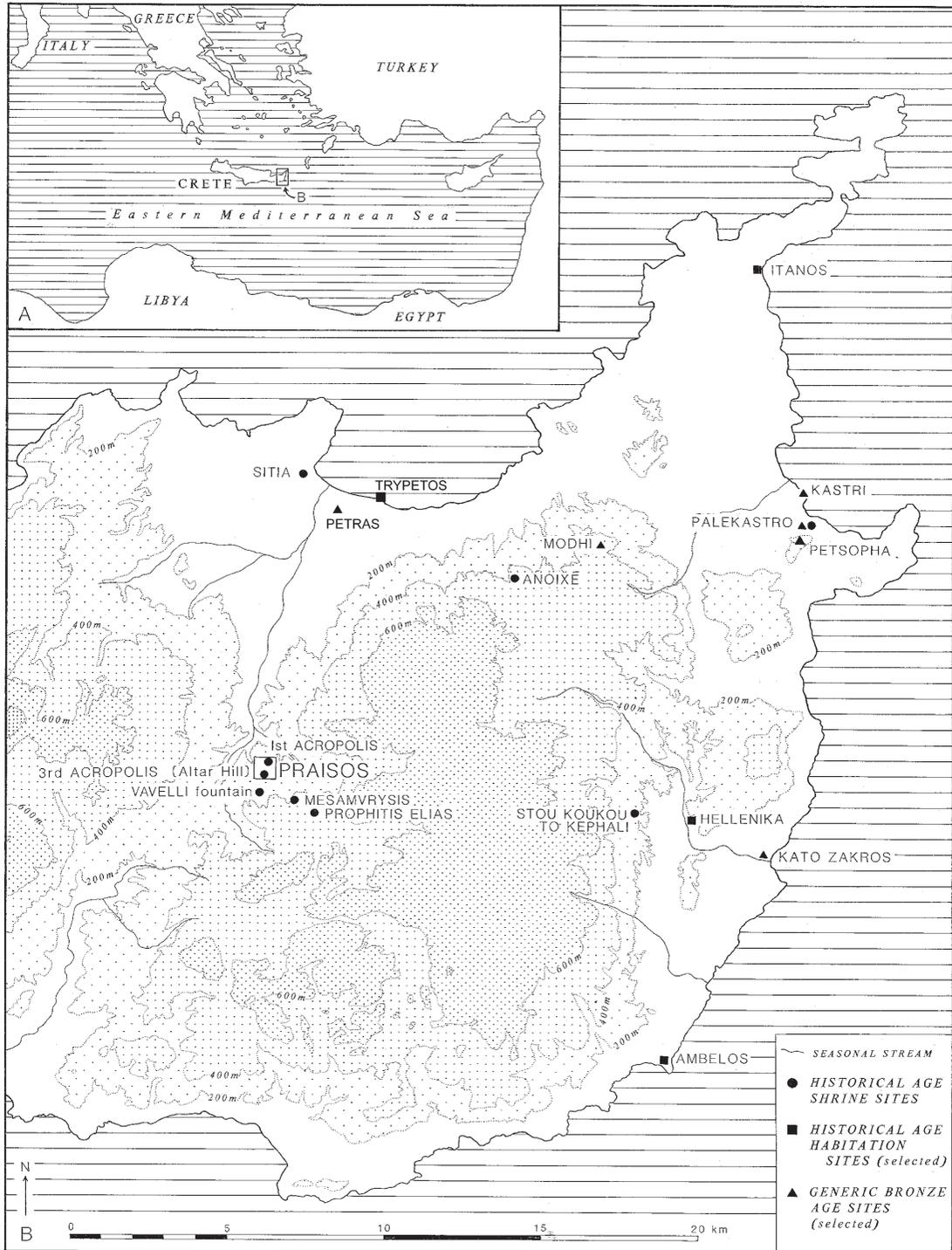


Figure 1. Map of eastern Crete, indicating sites mentioned in text (drawn by Howard Mason).

houses in which they were found—Whitley in press). Its discovery, then, should prompt a reappraisal of finds in university museum collections, such as the decorated Archaic *pithos* from the Afrati workshop now on display in the Ashmolean (AN 1969.251).

Excavation almost invariably entails the reappraisal of older finds, often now in museums. This brings me to another practice that Osborne would like to revive—that of *partage*. He is not alone in this; Cuno (2008) is another enthusiastic supporter of this antiquated practice, where finds from a single place or excavation are now dispersed in a number of museums in several different countries. For one of the consequences of *partage* is that it makes a reappraisal of these finds much more difficult. It is much harder to re-evaluate the claims made by the excavators of *Artemis Orthia* (Dawkins 1929) near Sparta than it is those of Olympia, precisely because the finds from the former are dispersed between several museums in Greece and Britain, whereas the bulk of the finds from the latter are held in Olympia itself. That we now have a complete reappraisal not only of helmets but of the ‘depositional practices’ of the military trophy (Frielinghaus 2011) is due in large part to the retention in Olympia of the bulk of the finds from Olympia.

Both the problems created by *partage* and the importance of context can be illustrated from another set of examples from Praisos. These are the mould-made terracotta plaques, which form a distinct part of the coroplastic corpus (e.g. Higgins 1954). Such plaques show examples of both the male and female body, and would thus be as relevant to any general reappraisal of ‘the history written on the Classical Greek body’ (Osborne 2011) as the more usual (largely Athenian) examples from vase-painting and sculpture. These terracotta plaques first came to the attention of archaeologists through formal (and informal) excavations around Praisos between 1894–1901 (e.g. Halbherr 1901; Bosanquet 1902), and have ended up forming part of the major Classical collections in the Louvre

(Mollard-Besques 1954), the British Museum (Higgins 1954), the Genf Museum (Fehr 1970), the Heraklion Archaeological Museum, the Metropolitan Museum of Art (Dohan 1931) and both of the university museums of Oxford and Cambridge (Boardman 1961). This *partage* has made it very difficult to get a clear focus on their original context of deposition and so on their ritual significance. Their interpretation has been purely iconographic: through the lens of Greek myth a plaque of an archer (Boardman 1961: 112, no. 508) becomes ‘Herakles with bow’.

Fieldwork has changed that (Whitley 2015: 37–42). During the 1998 fieldwalking season we managed to relocate one of the original contexts of their deposition, the ‘spring at Vavello’ (site no. 68; for location see Figure 1, above). This relocation enabled us to reconstruct the likely provenance of many of the older finds. We were greatly helped by a comprehensive reappraisal of Archaic Cretan terracottas by Pilz (2011), of Cretan sanctuaries by Prent (2005: 302–305) and the application of principles of coroplastic study outlined by Nicholls (1952). Forster’s (1902; 1905) original typology remains useful. Full publication of the finds from another spring shrine at Anoixe (for location see Figure 1, above) within the territory of Praisos by Erickson (2009) allowed us to make both contextual and iconographic comparisons. Re-excavation of the Almond Tree House (*Andreion*) deposit at Praisos itself in 2007 (Whitley 2011) provided Classical versions of an iconography that can be traced back to the seventh century BC. Thus the recovery of 28 plaques (representing 13.6% of the total of the 206 known examples), first through survey and then through excavation, allowed us to place almost the whole corpus of plaques in its archaeological and ritual context. This re-contextualisation, combined with a reappraisal of their use and iconography, enabled us to relate them (and their distinctive iconography) to the workings of the ancient *polis* of Praisos (Perlman 2004: 1183–84, no. 984). There is not space here to give all the references (or

examples) necessary to reach these conclusions; their full publication must await the publication of the Praisos survey. The evidence for the principal types of Archaic date are set out in Table 2.

Several inferences can be drawn from this table. First, the majority (161 out of 170; i.e. 94.7%) of terracotta plaques were found within the territory of Praisos. Second, several distinctive Praisos types can be identified: the female with tympanum (Forster type 9), the warrior plaques (Forster types 10 and 11), the ‘warrior

abducting a youth’ (see Halbherr 1901: plate XII no. 4, New York Metropolitan Museum 53.5.19), the youth with a hand on hip (Forster type 12) and the ‘robed male figure’ (Forster type 8). All 95 specimens of these types were, with one exception (see Brun and Duploux 2014), found within the territory of Praisos, with distinct concentrations in the two spring shrines of Vavelloi and Roussa Ekklesia. By contrast, those plaques with a more generically orientalisising iconography (griffins, sphinxes, lions, centaurs

Table 2. Occurrence of types of Archaic terracotta found in Praisos (especially Vavelloi), in eastern Crete and their occurrence elsewhere in Crete. Information from the Praisos survey (see references in Whitley 2014; 2015), combined with a synthesis of older finds contained in Halbherr (1901), Bosanquet (1902), Boardman (1961), Brun and Duploux (2014), Forster (1902; 1905), Higgins (1954), Mollard-Besques (1954), Dohan (1931), Fehr (1970) and Pilz (2011).

	<i>Vavelloi</i>	<i>Praisos</i> (<i>elsewhere</i>)	<i>Sitia</i>	<i>Koukou to</i> <i>Kephali</i>	<i>Roussa</i> <i>Ekklesia</i> (<i>Anoixe</i>)	<i>Elsewhere</i> <i>in Praisos</i> <i>territory</i>	<i>Itanos</i> (<i>Vamies</i>)	<i>Elsewhere in</i> <i>Crete</i>	<i>Totals</i>
Astarte (generic)	4	1					1		6
Astarte (types 2 & 3)	14		1					1	16
Other female	15	2	1				2	1 (Lato)	21
Tympanum (type 9)	4				3				7
Griffin (type 33)	4				3	1 (Sklavi)		1 (Ayios Georgios, Papoura)	9
Sphinx (type 32) and related	5				1	1 (Sklavi)		2 (Lato)	9
Centaur	5	1							6
‘Archer’	3							Pi	3
Lion	1								1
Warrior (type 10)	10								10
Warrior (type 11)	4	1							5
Warrior abducting youth	1	1			5				7
Youth, hand on hip (type 12)	3	2							5
Robed male figure (type 8)	20			2	40		1		63
Others	2								2
Totals	95	8	2	2	52	2	4	5	170

and perhaps archers) are less clearly focused on Praisos itself: three out of the 28 known (11%) came from outside of Praisos's territory. The same applies to the vast majority of daedalic female plaques (including those often referred to as 'Astarte' figurines). This daedalic female iconography is widespread in Crete in Archaic times, and is certainly not restricted to Praisos. These patterns seem to remain much the same in the Classical period, the evidence for which is summarised in Table 3.

There appear to be far fewer Classical-Hellenistic terracottas from in and around Praisos than there were Archaic examples. They also appear to be more concentrated in and around the city itself: there are no known examples on the extremities of Praisos' territory, and no examples of distinctive Praisos types have been found elsewhere in Crete. While this effect may, in part, be caused by the relative neglect of the archaeology of Classical as compared to Archaic Crete, it does nonetheless reinforce the impression that

terracotta plaques with a distinctly east Cretan iconography are generally found only within the ancient territory of the *polis* of Praisos. Combining Archaic and Classical specimens, the vast bulk of these Praisian terracottas come from within Praisian territory (197 out of 206, i.e. 96%). Over half of the known terracottas seem to come from the spring shrine of Vavelloi (125 out of 206, i.e. 61%). Pilz (2014) has emphasised the political or ethnic dimension to this phenomenon: eastern Crete in general and the territory of Praisos in particular is far less austere in its material culture than central Crete.

What is even more striking is the longevity of these plaques. Type 8 (the robed male figure), for example, has at least two mould series (*sensu* Nicholls 1952)—some of the examples are markedly smaller, and therefore later; Pilz (2011: 334-35) divides them into types Pr IV/3 (earlier) and Pr IV/3² (later). Though the style of the original dates to ca. 600 BC, the fact that a second mould was made (almost certainly

Table 3. Classical/Hellenistic terracotta plaques from Praisos, Praisian territory and Crete. Information from the Praisos survey (see references in Whitley 2014; 2015) combined with a synthesis of older finds contained in Halbherr (1901), Bosanquet (1902), Boardman (1961), Forster (1902; 1905), Higgins (1954), Mollard-Besques (1954), Dohan (1931), Fehr (1970), Pilz (2011) and the results of excavation at Praisos (Whitley 2011).

	<i>Vavelloi</i>	<i>Praisos</i> (elsewhere)	<i>Totals</i>
Female type 13	3		3
Female type 14	1	1	2
Female type 17	2	1	1
Other female (types 20, 23, 28)	4		4
Dancing girl (type 31)	4		4
Warriors (ram's head shield)		2	2
Men with helmets	2		2
'Ephebus' type 24	1		1
Eros?	1		1
Men with rosettes	4		4
Male 'hand on hip' plaques (types 25, 26 and 27)	8 (or more)	2 (or more)	At least 10, possibly 16
'Egyptianizing'	1		1
Totals	30	6	36

from a terracotta plaque) suggests that this type was used well into Classical times. Iconographic continuity is more evident in other male plaques. The Classical plaque types 25–27 differ in size but not in iconography and must represent a similar mould series. The pose and iconography of these plaques derive from an earlier type (Forster type 12), a figure of a youth with a ‘hand on hip’ that goes back to 675 bc.

A similar degree of iconographic continuity can be detected in the ‘warrior’ plaques. These begin with a plaque of distinctly Geometric style (type 10). By the end of the Archaic period this type has been replaced by another (type 11) having the same iconography (helmet, shield and raised arm with weapon), but with addition of a ram’s head on the shield. Type 11 in turn seems to have been replaced with a Classical type, also with a ram’s head shield, the best example of which was recovered in excavation in 2007. The sequence is set out in Figure 2.

What is the significance of this distinct iconography? First, the iconography has a spatial dimension. The types of plaques found at Vavelloi are very much the same as those recovered from Anoixe (also known as Roussa Ekklesia; see Figure 1 for location) on the border of Praisian territory (Erickson 2009). This is one of the few sanctuaries in Archaic and Classical Crete that follows the mainland pattern outlined by de Polignac (1984), where sanctuaries help to define territory (see Whitley 2008). Secondly, the iconography may relate to ritual and to citizenship. Erickson (2009) argued that the male iconography, and especially the iconography of a ‘warrior abducting a youth’, might relate to a particular passage in Strabo (*Geography* 10.4.20–21), who quotes the fourth-century bc historian Ephoros (*FrGrHist* 70.149) on how young men were initiated into the citizen body. A boy (or young man) is not seduced, following the standard Greek relationship of *erastes* to



Figure 2. Sequence of warrior plaques from Late Geometric to Classical times, showing underlying iconographic continuity (prepared by Kirsty Harding). The figure should be read ‘retrograde’ (right to left), with earlier plaques to the left and later (Classical) ones to the right. Far right, a plaque in New York (Met Mus 35.5.9, after Halbherr 1901: 390, fig. 19); a composite picture of two fragments of the same type (10) in the British museum (BM 1907, 0119.64 above and BM 1907, 0119.60 below—Higgins 1954: nos. 575–76); a reconstructed drawing of another plaque (type 11), after Halbherr 1901: pl. XII, no. 3; and at the far left a type recovered from excavations at Praisos in 2007 (plaque A 205.7, no. 6; Whitley 2011: 18–19, fig. 14).

eromenos, but simply abducted by an older man, who takes the boy (chosen for his strength rather than his beauty) to his *andreion* (public dining hall). Time is spent hunting in the wilds, and love gifts (which may include large animals, both domestic and wild) seem to form part of the initiation ritual whereby young men are organised into 'herds', which eventually form a part of the citizen body. This ritual then is essential to the creation of citizens and thus to the maintenance of the Cretan 'citizen-state' (*polis*) and has been much discussed by historians (e.g. Seelentag 2015: 374-503). Lebesse (1985: 188-98) links this ritual to the iconography of Archaic bronze plaques from the Symi sanctuary, which show young men offering love gifts of hunted animals (hares or an *agrimi*, the Cretan wild goat). Such rituals, employing this distinctive male iconography, must have been central to the proper functioning of Cretan citizen states (including Praisos—Whitley 2011: 40-41; 2014).

That these kinds of iconography link bodies of men to the body of citizens, but do so in a way very different from that found in Classical Athens, should be something of interest to historians. Here the citizen body is far from invisible (*contra* Osborne 2011: 85-123). Just how these 'citizen bodies' were created is not a question that can be resolved by iconography alone. For if love gifts of hares or *agrimia* were consumed in the *andreion* (as Strabo/Ephoros implies), then there should be some kind of evidence for this in the archaeological record. This Cretan institution is thus not a matter just for ancient historians (trying to assess how much 'truth' is to be found in Ephoros), but for any scholar (historian, archaeologist or anthropologist) concerned with the general problem of commensality, and its relationship to power structures within the 'citizen state' (Whitley 2014).

The relevant evidence must take the form of faunal remains from well-excavated contexts. A preliminary examination of these from Praisos (Madgwick and Whitley in press) suggests that there was an unusual bias towards the consump-

tion of wild and feral (and so hunted) species, as against purely domestic ones such as from deposits that may be related to the *Andreion* (Almond Tree House) excavated by Bosanquet (1902: 259-70; Whitley 2011). A similar bias towards hunted species (hare, boar and deer) also seems to be evident in Dreros (Zographaki and Farnoux 2011: 642-43). Faunal analyses, and indeed any other kind of scientific analysis, from strontium-isotope to petrography to radiocarbon dates, requires evidence from well-stratified, well-recorded and well-studied contexts from excavations conducted to modern standards. They are rarely found in museums.

Some Conclusions

My conclusion then is exactly the opposite of Osborne's. Addressing the research questions of the twenty-first century in Classical antiquity requires new evidence from new fieldwork (both survey and excavation). This evidence must take the form of things previously neglected by traditional, object-focused Classical archaeology (such as animal bones) if we are systematically to investigate questions of fundamental importance to our understanding of Classical antiquity such as ritualised commensality and its relationship both to religious practice (Parker 2011) and to political structures. We do not need fewer classically-focused field projects; we need more.

Osborne is also quite wrong in what he says about 'dirt' archaeologists. Excavators are in general fully cognisant of the importance of 'object biographies', and incorporate these concepts into their interpretations (as in Whitley in press); museum finds need to be re-evaluated in the light of discoveries made through excavation, where those excavations are fully contextual and conducted to modern standards. Osborne's enthusiasm for the old practice of *partage* seems misplaced. Museums and excavations ought to exist in a symbiotic relationship, not an antagonistic one. So, if one could establish, whether through further excavation or

through a contextual reappraisal of tomb finds in Etruria, that not only one but most examples of red figure Attic *pelikai* came from Chiusi (ancient Clusium), one would have made a great advance in our knowledge of the highly connected Mediterranean world of the sixth and fifth centuries BC. Instead, Osborne is left to guess as to who exactly is viewing these objects.

So, Osborne's argument is, to me, rather baffling, particularly for someone who has, elsewhere in his work, made excellent use of Greek material culture and (highly contextual) archaeological scholarship, and whose ambition it is to get away from the dominance of 'the text' in the study of Classical antiquity (e.g. Osborne 2011). How can Osborne have arrived at such an odd idea of what actually goes on in excavations? And why does he seem to have such a visceral aversion to 'dirt' archaeology?

Osborne's view, I think, reflects a *communis opinio* within Classics, and results from other recent developments in that field. Chief amongst these is the vogue for reception, a field that depends absolutely on there being a defined canon of texts (literature) and objects (art) whose reception can be assessed. Classics is a subject defined, in part, by what it has chosen to canonise, whether these be art or texts. It is perhaps in this defence of the canon (both of objects and of texts) that we can locate the true reason for Osborne's objection to excavation. Archaeological research is inherently dirty; most of the material evidence for the ancient world, from the papyri of Oxyrhynchus to the painted pottery from Cerveteri, comes from incredibly filthy tombs, mounds, middens and pits. Classics requires a defined and definitive canon of art and literature whose origins in archaeological filth must remain hidden. Archaeology, especially when combined with anthropological theorising, has always tended to undermine the Classical canon of established textual and aesthetic truths,

as some of his colleagues have (reluctantly) come to recognise (Beard 2001; Henderson 2001). 'Dirt' archaeology (and dirty archaeologists) are an uncomfortable reminder that no-one in the study of Antiquity has ever had clean hands.

Note

1. I am not saying that this is, in some ways, not equally problematic—and some of the 1950s dioramas I visited in the 1980s did make me wince. But that is an entirely separate debate, which cannot be pursued here.

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About the Author

James Whitley has been engaged in fieldwork in and around Praisos since 1992. His major works include *The Archaeology of Ancient Greece* (Cambridge: Cambridge University Press, 2001). Between 2002 and 2007 he was Director of the British School at Athens.

Response to James Whitley

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Since I wrote ‘De-contextualising and re-contextualising: why Mediterranean archaeology needs to get out of the trench and back into the Museum’ in order to provoke debate, it is a pleasure to have James Whitley take up the challenge. James’s long list of where he agrees with me is heartening; his disagreements stand in rather oblique relation to what I wrote.

Some of the issues are trivial or distort what I said. I never expressed a desire to revive the practice of *partage*, nor did I express an enthusiasm for it; I merely noted that *partage* had advantages to which Kersel (2015) had drawn attention. I have no ‘visceral aversion’ to ‘dirt archaeology’ (Whitley’s phrase); my aversion is to the assumption of the moral high ground by those who privilege evidence from the site of excavation over other contexts of understanding an object—or as I put it, my paper offered some ‘resistance to privileging the field archaeologist’s priorities’. I see no point in arguing about ‘Mediterranean’ vs ‘Classical’ archaeology; ‘Classical’ is a problematic term because it has alternative senses (one of which, the one privileged by Whitley, refers to a specific period; another has it refer to the whole of ancient Greek and Roman archaeology from the end of the Bronze Age to late antiquity). Nor will I rise to the bait over ‘Athenocentrism’: Athens is a useful example because the quantity and range of material (textual as well as archaeological) is so great, but my paper involved neither claiming that what was true of Athens was true of the whole Greek world (but Athenian Praxiteles’ statues were set up in Knidos and Olympia), nor asserting that Athenian material was in some way better.

Two matters of substance deserve discussion. I clear away some misunderstandings and then turn to them.

Misunderstandings and Misrepresentations

Whitley claims (p. 254) ‘Osborne has placed so much emphasis’ on object biography. I did not use the term. I talked about the ‘social life of things’. Appadurai (1986a), not Kopytoff, is my man. As the first paragraph of Appadurai’s (1986a: 3) famous paper lays out, to focus upon the social lives of commodities is to make things a source of knowledge of politics, construed broadly, since it is politics that creates the link between exchange and value.

The claim that I am at odds with Haggis and Antonaccio depends on a reductive reading of my claims and theirs, which are essentially complementary. My paper is not arguing that there should be no more excavation or no study of field data: I am strongly in favour of both. My point is that some questions emerge better from looking at artefacts in museum contexts, not excavation. I never argue that ‘all contexts are equal’, simply that the assumption that excavation context trumps all other contexts needs questioning.

I was myself heavily involved with Lucilla Burn, Kate Cooper, Mary Beard and Caroline Vout in redisplaying the Greek and Roman Galleries at the Fitzwilliam, which I am accused of ignoring. But the redispays in the Fitzwilliam and Ashmolean Museums are beside the point. Those displays were concerned to make the collections more accessible to the public,

and to showcase the results of *past* research. The Fitzwilliam redisplay yielded new research, but primarily on the history of the collection (see the papers in the *Journal of the History of Collections* 24.3 [2002]). My point, that research in university museums on the objects themselves by students and faculty members is not now common, stands.

Whitley's opposition to the term 'theology' is a traditional one. The uncritical acceptance of claims by advocates of 'religions of the book' that without scripture there can be no theology has massively distorted the study of Greek religion and led to the notion that one can have orthopraxy without theology. Every act of worship implies a set of beliefs about those worshipped, and to rule out studying those beliefs, for all the difficulties involved in doing so, is to condemn oneself never to be able to understand great swathes of ancient culture, both material and textual (for fuller exposition of theologies of Greek religion, see Eidinow *et al.* 2016).

Whitley's accusations that museums 'aestheticise' the classical are out-dated. My paper discussed a number of museum contexts in which that was not true, and it is not true of the new displays in the Ashmolean and Fitzwilliam Museums to which Whitley refers. Of course museums are interested, and interest their visitors, in the appearance of their objects, including their ethnographic collections, but that is because their appearance both was and is a vital affordance of these objects; by putting objects with other objects to which they are visually related, one of their important contexts is recreated and their social life, their politics, value and exchange, in the ancient as well as the modern world, is understood.

As to Euphronios's krater, we think we know where it was found, but we do not know what was found with it. While the broad pattern of Attic pottery with which Euphronios's krater was imported is indeed more apparent in Cerveteri, the broad pattern of Attic pottery with which the krater was produced was more apparent in the

Metropolitan Museum of Art (because of local importation patterns in Etruria). Understanding the social life of this krater does not depend simply on understanding of its context of discovery (i.e. the context of its momentary deposition), but on our broader understanding of its relation to the material culture of its time.

I am accused of 'insistence on talking about the "context of the archaeological trench"' (p. 250). The phrase 'context of the archaeological trench' is not used even once by me. 'Trench' occurs twice, in titles (for the article as a whole and in a subtitle 'Locking up knowledge in the excavation trench'), where its symbolic value must be apparent ('polemical exaggeration' if you like). I cited the objects of my polemic in my paper, and shall not further justify what I said in that section, to which Whitley's lesson in egg-sucking has no relevance. But Whitley's discussion allows me to engage with two issues that were not on my agenda in that paper. These are best addressed with reference to the studies that he cites.

The Symposium in Context

Kathleen Lynch's fine study of the pottery from Well J 2:4 (Lynch 2011; cf. Lynch 2014) is used by Whitley to stress the virtues of contextual archaeology, and rightly so. I have no desire to play down the virtues of that study, with its careful arguments for why we should think of the well deposit as coming from a single household and its careful comparison of that deposit with other domestic deposits, contemporary and more distant in time, in the vicinity and more generally in Attica. However, the extraordinary value of having a deposit made at one point in time from a single household is accompanied by severe limitations.

The first limitation is that while we can assemble good arguments for the pottery in the well coming from a single household, it is not possible to argue that *all* the pottery from that household, let alone all the sympotic vessels, ended up in the well. The total absence of

a krater, the mixing bowl without which there could be no symposium, is the most signal indication of this. Lynch (2011: 130–31, 170) conjectures that this is because the krater or kraters used in the household were made of metal, but this is only one possible explanation. It is close to certain (Lynch 2011: 131, for how one might survive without a krater) that an important item is missing from the deposit. Once that is acknowledged, the confident statement with regard to other absences that ‘the house did not own set-specific serving utensils’ (Lynch 2011: 170) becomes very fragile (she is rather more careful elsewhere in the same work; cf. Lynch 2011: 80). What we have in this well is a subset, whose relation to the whole set can be conjectured only on the basis of comparisons with material elsewhere.

And this is the second limitation. We have no reason to believe that every household had the same set of symposium pottery. Actually, we can make a stronger point. The very coherence of the set of cups found in this deposit makes it certain that other households had different sets of sympotic pottery. When Lynch (2011: 173–75) moves, therefore, from discussion of this set of pots and their iconography to claims about the pattern of iconography on pots used in Athenian households more generally, the move is problematic. Lynch takes as her evidence the iconography of vessels of all sorts from her particular well, but only cups (apparently) from other Athenian deposits, and indeed only deposits that have material from the clean-up after the Persian destruction. This is not an unreasonable thing to do, but the conclusions that follow must be conclusions about this evidence. That is, the conclusions relate to cups in use in households in central Athens at the time of the Persian wars. If there is to be a comparison between pottery from Athens and Athenian pottery found elsewhere, it needs to be a comparison with cups in use elsewhere at the same time. And the possibility that various peculiarities of the evidence from Athens

might be related to each other needs to be borne in mind.

The cups that Lynch takes as her sample turn out mainly to be decorated only on the interior, to have few scenes of mythological narrative, ‘no elaborate symposium scenes depicting the entire room’ and no ‘graphic sexual images’ (Lynch 2011: 173, 175). They include few fragments that can be ascribed to ‘prominent, innovative red-figure painters’ (Lynch 2011: 175) such as the Brygos Painter, Makron, Douris and the Berlin Painter. But both mythological narratives and sympotic scenes depicting the entire room are primarily found on the exterior of cups, and their absence from cups decorated only on the inside is completely unsurprising. Fewer than a sixth of the cups attributed to Douris, and only about a tenth of cups attributed to Makron, were not decorated on the outside. The Berlin Painter was not a cup painter. ‘Graphic sexual images’ are not common on Athenian red-figure pottery, and those that there are have a very particular chronological distribution and are produced by a relatively small number of painters. The pottery in the Persian destruction deposits is not a random sample of the pottery made at the time, and the factors that have skewed the sample need careful consideration. Whitley’s claim—that Lynch’s material challenges the case, made not simply by me (Osborne 2001) but in more detail by Reusser (2002), that the vast majority of Athenian pottery iconography can be found in Athens as well as in Etruria, and that production for the Etruscan market remains to be proved for all but a few special cases—is at least premature (cf. Osborne 2014).

Even in Lynch’s study we can see the temptation to privilege the particular assemblage and to underestimate the limited light that that assemblage sheds on the social life of the objects in it. Understanding an assemblage demands understanding how the material in it might relate to wider material—something with which study of material in museums can significantly assist.

Invisible Citizens

Although Whitley's main target is my *JMA* provocation, he cannot resist casting aspersions elsewhere. I respond to one challenge. The plaques showing men abducting boys that Erickson (2009) published from Roussa Ekklesia are invoked with the claim that they 'link the bodies of men to the bodies of citizens [...] in a way very different from that found in Classical Athens [...] Here the citizen body is far from invisible (*contra* Osborne 2011: 85-123)' (Whitley p. 260). Whitley invokes a passage of Ephoros quoted by Strabo to show that citizen bodies were created by this abduction ritual; he claims that selection for abduction was on the basis of strength not beauty. Ephoros in fact opposes beauty to bravery and orderliness (ἀνδρεία καὶ κοσμιότητα), later saying that if a handsome boy was not chosen it would be reckoned that this related to his 'character' (διὰ τὸν τρόπον). That citizenship is involved here is something that Whitley imposes both on Ephoros and on Erickson. The passage of Ephoros never mentions citizens, and indeed makes it plain, as Erickson correctly recorded, that abduction was not the only way of coming of age. Ephoros does indeed note that those who have been abducted receive honours, but the

honours he records are privileged places at dances and races, and better clothes that they continue to wear even after they have grown to manhood (*FGrH* 70.149 = Strabo 10.4.21). The citizen body is as invisible here as it is on Athenian pots. Whitley's reading of Ephoros turns out to be as careless as his reading of my own text.

Conclusion

Whitley claims to be baffled by my argument and by how I could 'have arrived at such an odd idea of what actually goes on in excavations'. But I made no claim about what goes on in excavations: my observation was only that some archaeologists so privileged the excavation context that they treated museums as places in which objects were de-contextualised. My paper was an argument for richer and fuller re-contextualising of archaeological material. Both the examples that Whitley throws at me suffer from skimpy contextualising—from thinking that a particular time- and space-bound deposit / set of deposits offers a complete picture, on the one hand, and from thinking that the only issue for men in a Cretan city was becoming a citizen, on the other. On my reckoning, Whitley scores two own goals.

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