

**Of princes and peasants? A comparative
approach to an understanding of social
development, identity and dynamics in mainland
Greece, c.1300 – 900 B.C.**

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Abstract

With the destruction of the Mycenaean palaces, Aegean Bronze Age society underwent dramatic transformations. The palaces, along with much of their associated material expressions, disappeared. In this comparative study I examine why the Mycenaean political institutions were never reinstated and the nature of the social dynamics that subsequently created a situation traditionally characterized as the 'Dark Ages'. To this end, analyses of the Linear B documents from Pylos are used to examine palatial and 'extra-palatial' social identities, relationships and the dynamics of socio-political change. Examining firstly the concept of an administrative archive, I propose a fundamental revision to our understanding of what these documents represent, how they were used and, for Pylos, where their primary context of expression lay. Specifically, I argue that the tablets were not a passive administrative tool, but were active devices in the manipulation of social relationships and identities within and beyond the Palace of Nestor. As mnemonic aids to the establishment of relationships of patronage, debt and obligation within an oral/aural arena of negotiation, they reflected clear divisions within Mycenaean society; divisions that laid the foundations for a rejection of that socio-political system. From this, a model is suggested whereby the dynamics of Early Iron Age society were driven by factions and factional competition, initially focussed upon authority figures such as the *basileis*. It is further proposed that the archaeological variability characteristic of this period is a direct reflection of competing factional identities whose ideologies can be distinguished by varying degrees of affinity to the preceding palatial system. Finally, the hiatus in the use of writing between the 12th and 8th centuries B.C is suggested to be a direct result of the connotations arising from the use to which Linear B was put and the concomitant antipathy towards the accoutrements of Mycenaean palatial identity.

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Pursuing research into past social identities inevitably generates a degree of introspection, of asking the questions: who am I and where do I fit in this world? Undertaking this PhD has, for me, been a profoundly life-changing experience. It has constantly challenged and tested me in ways that I could never have foreseen and not unsurprisingly this journey has demonstrated beyond any doubt that our identities, however personal we believe them to be, are neither static nor developed in isolation. The journey that I undertook was aided along the way by the generous spirit of many friends and colleagues but first, and foremost, by my supervisor, John Bennet, to whom I owe an enormous debt of gratitude. From the original conception of this project John has been a constant source of inspiration, encouragement and steadfast support. From the intricacies of Linear B scholarship, through to the various personal troubles that arose, his steadying influence, experience and analytical skill has guided me through. Following each and every meeting or discussion my enthusiasm and motivation was given fresh impetus and although, I am sure, my sometimes over-enthusiastic approach occasionally required tempering, I count myself extremely fortunate to have been able to give free rein to my ideas without fear of condemnation. As well as reading and commenting on several versions of this thesis, generously allowing me to view work prior to publication and providing me with many offprints and references, it is his patient motivation and genuine interest in this research that is deserving of my utmost respect and thanks.

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Chapter I

Introduction: 'The writing's on the wall...'

There is a curse.
They say:
May you live in interesting times.
Terry Pratchett, *Interesting Times*.

This popular rendition of an alleged ancient Chinese curse succinctly encapsulates many of the historical views surrounding the collapse of the Mycenaean palaces and the end of the Aegean Bronze Age. Since the dramatic discoveries of Heinrich Schliemann that brought the Mycenaean world to light, the sudden disappearance of the palaces has long been the subject of intense debate. Often heralded as the highpoint of Bronze Age society, Mycenaean culture exhibited many characteristics that today are regarded as the marks of civilization, but the events surrounding its collapse remain something of an enigma. Yet the archaeological record is clear. Towards the end of the 13th century BC, the Mediterranean world of the Late Bronze Age (LBA) was subjected to a series of destructive episodes culminating in a final wave around 1200 BC. Many of the major centres in Greece and across the Eastern Mediterranean were destroyed. In neighbouring Anatolia, the once great empire of the Hittites fell and their capital at Hattusa was destroyed. Through the Cypriot and Levantine region, significant centres of trade such as Enkomi, Kition, Ugarit, Ashdod and Ashkelon met similar fates and the great power of Egypt faced its own threats from the infamous Sea Peoples. In Mycenaean Greece, widespread destructions encompassed all of the major centres of political power, resulting in abandonment for many and, ultimately, the disappearance of the Mycenaean political system. Radical social changes occurred from an apparently dramatic decline in population to the severe interruption of 'international' trade. In the case of Pylos, this abandonment was apparently complete with the site never again re-occupied in any sustained form. Although archaeological evidence attests to a brief renaissance of Mycenaean cultural activity in the period known as LH IIIC, no attempt to reinstate the palatial system, or to reconstruct or reoccupy the central palace buildings was made. By the 11th century B.C, mainland Greece was seemingly entering a period of decline that traditionally has been referred to as the 'Dark Ages'. To judge by these disasters, whether natural or human in origin, these certainly were 'interesting times'.

However, seldom is this picture given the perspective it deserves. The palatial destructions notwithstanding, the assumption has been made that the events of this time were held to be

disastrous for the people involved. The removal of the Mycenaean cultural and political 'package' is viewed as something of a retrograde step (or, indeed, as the 'curse' of condemnation to times of change and instability). From the emergence of palatial civilization to the development of a fully-fledged Mycenaean culture, contemporary thought imposes an evolutionary scheme of development – each step progressively improving the social environment. Not surprisingly, when the archaeological record unequivocally demonstrates an end to this pattern, the logical conclusion is that Mycenaean civilization gave way to social disorder, cultural decline and a 'Dark Age'. This latter notion has more recently been downplayed, particularly with regard to the apparent continuation and even renaissance of elements of the Mycenaean 'cultural package' in the post-palatial period, with the suggestion that it was only by the 11th century B.C that Mycenaean culture was fully extinguished (Rutter 1992: 70; also, Osborne 1996: 19-51; Whitley 2001: 78-80; Dickinson 2006a: 58-77). The symbols of Mycenaean political dominance, however, did not reappear. Linear B, elaborate decorative elements such as wall-paintings, monumental constructions and palatial architectural forms all disappear. Furthermore, although evidence from Tiryns in particular suggests the desire for some form of connection with previously important sites, the palaces themselves are no longer the foci of elaborate settlement or statements of power. Building T, although thought to be a 'ruler's dwelling' clearly demonstrates, architecturally, a changed ideology (Maran 2001: 117-18; 2006). This 'Mycenaean renaissance' cannot therefore be described as anything more than selective and restricted (cf. Morris 2000: 195-256), strongly suggesting that in terms of the elements that might be described as defining Mycenaean 'civilization', no attempt appears to have been made to reintroduce them.

From this point on, the Early Iron Age (EIA) is characterized by a degree of cultural poverty. Marked changes occur in the settlement, funerary and subsistence evidence, alongside the technological innovation of iron-working. But the socio-political framework in which these changes are situated appears to devolve into one that is reminiscent of pre-Mycenaean times (cf. Snodgrass 1971: 383-85; 2000: xxvi), in one model characterized as a 'big-man society' (Whitley 1991). From our perspective then, the changes appear not simply unwelcome but calamitous. Yet if we are to suppose that the destruction of the palaces was such a catastrophe, why were they not reoccupied, rebuilt and the political system reinstated as many centres across the Near East were? In this at least, the archaeological/historical record is clear, such actions did not take place and hypotheses based upon population and economic decline in the wake of the destructions cannot, in my opinion, justifiably be used to explain this. As some have suggested, this and subsequent social changes must reflect a social *rejection* of the Mycenaean way-of-life (Morris 2000: 201; Sherratt 2001: 214-15; Bennet 2004: 101), not simply the logical conclusion of a socio-economic disaster. This thesis

attempts to understand how such a situation may have occurred and why the separation between Mycenaean and EIA societies is so marked.

Reviewing 'Mycenaean collapse' hypotheses

Inevitably, these widespread and dramatic events have prompted an intensive and ongoing debate as to the likely cause – one that shows no signs of abatement or resolution (cf. Desborough 1964; Mylonas 1966: 213-33; Betancourt 1976; Hooker 1976: 140-82; Sandars 1978; 1983; Drews 1993; Popham 1994; Shelmerdine 1997b: 580-84; Dickinson 2006a: 24-57; Tartaron 2008: 132-34). Much of the earliest attention focussed upon the 'pseudo historical' accounts of Classical writers that maintained a tradition of invasions and migrations. The legendary Dorian invasion in particular has been the subject of intense debate, with archaeologists and historians attempting to discover in the material record some physical evidence for the influx of new peoples. A variety of indicators has been suggested, from the appearance of material culture apparently alien to the preceding Mycenaean types, to the adoption of new forms of settlement and burial. Perhaps one of the most controversial lines of evidence is the so-called 'Barbarian' or 'Handmade Burnished Ware' (Rutter 1975, 1990; Sandars 1978: 191-95; 1983: 60-63; Small 1990; Bankoff *et al.* 1996; Morris 2000: 199). Apparently completely atypical of Mycenaean pottery forms, in that it appears crudely made and stylistically inept in comparison, this has become a central icon in the arguments for foreign intruders. Yet even without considering the exacting and contentious issue of relating material culture to specific peoples, examples have now been discovered in several LH IIIB contexts at Mycenae, Tiryns and possibly Athens, thus predating the palatial destructions (Small 1990: 5; Rutter 1990: 35; 1992: 66, 70; Hall 1997: 120). So, those literary sources that talk of invaders to Greece, be that the 'Dorian invasion', the 'return of the Herakleidai', or an Illyrian migration ultimately suffer from a lack of supporting evidence in the archaeological record, and the virtual invisibility of these groups (cf. Mylonas 1966: 218-27; Hall 1997: 56-65, 114-28).

Nevertheless, the notion of external invasion has remained a central thesis in LBA studies and formed the core argument of the now classic synthesis by Desborough (1964). Although later revised (Desborough 1972: 18-25) to take into account further archaeological discoveries, in essence the argument remained the same: that an incursion from the north, by peoples bringing new material culture with them, including new forms of settlement and burial, brought about the destructions of the palaces. The invaders hypothesis, whilst popular, has faced criticisms from the perspective of attaching ethnic origins to particular forms of material culture, but also from the problem of equating several independent

destruction phases at the major citadels to invading hordes. In the same way that those in favour of invaders argue against natural phenomena by asking how it is possible to positively attribute the cause of burning to natural disasters, the reverse similarly applies. How is it possible to positively identify the hand of man in these events? Strong arguments against such notions were forwarded by George Mylonas (1966: 213-33), who preferred to see these events in terms of the 'legends', myths and 'histories' recorded by ancient authors. So, for example, the destructions at Mycenae were seen in relation to the legends of Agamemnon and Clytemnestra and possible power struggles within the Mycenaean hierarchy following the death of a 'great king'. In pursuing this line, however, the notion of an internal 'social revolution' from the lowest classes of Mycenaean society was rejected (*ibid*: 231-32; also Desborough 1972: 21-22).

Alongside manmade disasters several natural explanations have also been explored. In dismissing the case for invasion, Carpenter (1966) suggested that severe and extended drought, brought about through climatic changes, severely disrupted the agricultural economy. Based upon the premise of shifting trade winds leading to drier winters over the Aegean, the model was given plausibility but no contemporary climatic data offered to indicate that this indeed occurred. Subsequent attempts by climatologists to re-evaluate this thesis and establish whether evidence existed for a real climate change around 1200 BC in the Aegean was largely inconclusive (Bryson *et al.* 1974; Weiss 1982). Whilst the feasibility of climate changes affecting agricultural production was convincingly argued using a similar pattern of events observed in 1955, the reality of a 1200 BC climatic change directly affecting mainland Greece was less convincing. Elsewhere, indications, both from contemporary written accounts suggesting food shortages and recent analyses of wood samples from Gordion, suggest that at least parts of Anatolia suffered from drought conditions around this time (Drews 1993: 79). Yet even here, the case is far from clear. It has also been argued that the problems of agricultural production may have had as much to do with the redeployment of agricultural labour and to the disruption of trade as to the prevailing climate (Bryce 2002: 255-56). Whether climatic factors can be shown to be central to the problems of the Hittite empire, it remains uncertain how widespread this may have been or how long such conditions may have lasted. We cannot therefore, with any degree of certainty, extrapolate from the Anatolian case to the Mycenaean heartland. Although recent analyses do suggest that such a deterioration may have occurred and had some impact in Greece (Moody 2005), ultimately, a simple lack of evidence leaves this issue unresolved (French 2002: 135).

Other explanations, such as plague or disease, whilst being inherently attractive have lacked the supporting evidence. We cannot, for example, point to any aspect of the funerary record for this period that indicates a sudden increase in interments brought about by the sudden onset of a virulent disease, or the mass burials that are often a feature of times of plague, such as have been recently discovered in 5th century contexts in the Athenian Kerameikos cemetery (Baziotopoulou-Valavani 2002; Papagrigorakis *et al.* 2006). Nor is it, at first sight, possible to attribute such widespread phenomena to another popular cause – earthquakes. Indeed, despite the relatively early suggestion that many of the destructions at Mycenae could be attributed to earthquakes rather than invasions (Mylonas 1966: 222), such interpretations remained unpopular or were simply dismissed on the basis that such widespread and catastrophic damage did not seem possible from such an event or that no definitive evidence for seismic activity existed (Betancourt 1976: 41; Zangger 1994: 210).

The debate was given renewed vigour with the excavations at Tiryns in the late 1970s. Several destruction phases attributed to seismic activity were identified, beginning with widespread damage at the end of the ceramic phase LH IIIB early. A final destruction in the area of the citadel at the end of LH IIIB2 suggested to the excavator that earthquakes had provided the catalyst for the ultimate collapse of the palatial system (Kilian 1988a: 134). Since then, the geological and archaeological evidence for widespread earthquake damage has significantly increased and the reanalysis of earlier excavations has suggested that earthquake damage may be more widespread. Indeed, recent studies in the Aegean and East Mediterranean have radically changed the traditional picture, particularly with the derivation of probability models for determining earthquake damage in the archaeological record (Stiros 1996; Nur & Cline 2002: 48, 52, fig. 9). Using such methods, the positive identification of earthquake damage has been made possible with the result that many firmly entrenched views have been shifting; at least to the likely occurrence of earthquakes in the period around 1200 BC, if not to the cause of Mycenaean collapse (Nur & Cline 2002: 49; also, French 1996: 51; 2002: 135). Where before, for example, the destruction of the palace of Pylos had been attributed to human intervention, the evidence of distorted foundation walls in some areas prompted the suggestion that Pylos too was the recipient of earthquake damage (Kilian 1996: 65). Moreover, the evidence from Tiryns, Mycenae and Midea in the Argolid in particular, but also from other sites in Greece and across the Mediterranean, recently led to the proposal that the East Mediterranean region between 1225 and 1175 BC may have been subjected to an ‘earthquake storm’; a whole series of temporally and geographically spaced earthquakes occurring across a common fault line (Nur & Cline 2002). Such a proposal certainly merits consideration given that many of the criticisms of the basic hypothesis point to apparent chronological incongruities (e.g. Zangger 1994: 209-10),

to differing levels of visible destruction across sites (French 1996: 54) and in the notion that these could have resulted in the destruction by fire at each and every centre (Drews 1993: 39; Dickinson 2006a: 50).

Certainly, the criticism that there is a lack of human remains associated with a supposed earthquake, particularly at Tiryns, Mycenae and Midea cannot be maintained (Drews 1993: 39-40). Human remains were found beneath the debris of the Southwest House of the Citadel, in the Plakes house north of the citadel and in Panagia House I. At Tiryns, the fallen walls of Building X buried the skeletons of a woman and child, whilst others were found beneath the debris from the fortification wall and an early LH IIIB house. Lastly at Midea, in the area of the East Gate the skeleton of a young girl was found, buried by the falling masonry (Shear 1987: 154-55, plate 5b; Åström & Demakopoulou 1996: 39; Kilian 1996: 65, fig. 6; Nur & Cline 2002: 48-54). The incidences of fatalities are few, but given the evidence for the clearing and rebuilding of these sites, the argument for the deliberate recovery and formalized disposal of any victims is not implausible. Moreover, we need to consider whether the population was given any warning of impending earthquakes, particularly if an 'earthquake storm' was involved. After all, in the case of the earlier Thera eruption, substantial excavations in the Bronze Age town of Akrotiri have failed to find direct evidence that the disaster led to any deaths. The suggestion has therefore been made that the inhabitants had prior warning from seismic activity preceding the catastrophic eruption (e.g. Page 1970: 32-3), a scenario that should at least be considered at Pylos where no evidence of human remains was uncovered, but where the contents of the palace do give the impression of a Mycenaean 'Marie Celeste'.

Nonetheless, seismic activity cannot alone explain the tumultuous changes in society over the period 1300 – 900 BC (Nur and Cline 2002: 61). The evidence for rebuilding and reoccupation in the palaces following earlier disasters, and in areas such as the Panagia houses, show that whilst earthquakes could have a devastating effect, Mycenaean society was capable of adapting and recovering in their wake. So, while they remain a plausible trigger, alone they cannot explain the radical social changes that occurred. Indeed one of the chief proponents of earthquake activity, Klaus Kilian (1988a: 134), viewed them as only a catalyst to a wider economic problem. However, as with many of the individual hypotheses, arguments have tended to progress from a single, causal explanation to a realization that each, alone, either lacks definitive supporting evidence or is too simplistic to account directly for all of the complexities of the archaeological record.

Inevitably, discussions tend to develop into amalgamations – models incorporating one or

more elements from competing hypotheses. Thus the suggestion that the 'catastrophe' could be intimately associated with changes in armaments and the tactics of warfare was supplemented, in the specific case of Greece, with the notion of invaders from the sea (Drews 1993: 221-22), or from the north (Sandars 1983). Similarly, it has been suggested that climatic deterioration led directly to an alteration in the balance of power between Mycenaean Greece and its northern neighbours – a change that led to invasion from the north (Bouzek 1994: 232-33). Most recently, this approach has been taken to an extreme by Dickinson (2006: 54-56, 242-45) who, although suggesting that as yet unproven natural events may have played a role, favoured a prolonged period of increasing internal strife, integrated with failing situations in the Near East and a pan-Mediterranean economic uncertainty as aggravating factors in this process. The economic model, whether predicated upon over-centralization around the palaces as administrative/bureaucratic centres (cf. Betancourt 1976; Deger-Jalkotzy 1996: 718, 726), or upon the interruption or re-alignment of external trade routes upon which the palaces relied (cf. Sherratt 2001), whilst seductive, again fails to explain the rejection of the Mycenaean political system and the diverse nature of the post-palatial period in which trade has clearly not ceased. In this they closely resemble the philosophy of systems collapse. Systems theorists, in attempting to incorporate and account for the various and often disparate forms of evidence, advocated a cascade effect of failing elements in the social system (cf. Renfrew 1978; Tainter 1988; Drews 1993: 85-90). Yet whether driven by over-specialization, centralization or any other inherent societal weakness, such hypotheses essentially reduce to arguments over the precise trigger. No one model satisfactorily addresses why such seemingly radical social changes occurred in the transition period of the LBA to EIA as a result of this collapse, or why the collapse was never followed by recovery and reinstatement.

A Mycenaean interlude?

Teasing apart the relationships between all of these hypotheses is extremely problematic. Whether or not the traditions maintained by later sources can ever be demonstrated to have veracity, without new evidence the fundamental problem of distinguishing cause from effect will remain. Conflict, invasions, migrations or internal uprisings could all have been the result of natural catastrophes, or could have triggered the collapse themselves. And so the debate remains at an impasse that is likely to continue for the foreseeable future. Yet in many ways, this could be argued to be the least significant element of LBA history. Extrapolating precisely the relationships of cause and effect in the dissolution of the palaces does not alter the one basic 'fact' – the palaces all, eventually, succumbed to destruction and the Mycenaean *palatial* organization disappeared. Whether this can ever be reduced to a single

cause, or established that a combination of events culminating in a particular trigger brought this about, the crucial question must be: why did the Mycenaean palatial system never recover? Archaeology, after all, presents the era of the Mycenaean palaces as the heyday of the LBA Aegean. It would seem invidious to claim that the Mycenaean political system was anything other than successful, whether or not the question of the level of international prominence is resolved. Neither can we simplify the problem with recourse to situating the Mycenaean case within an 'international' catastrophe. The events of the LBA consumed political centres across the Mediterranean, but whereas some, like Ugarit, remained devastated, others such as Enkomi and Kition on Cyprus were rebuilt and reoccupied. Indeed, whether or not the Eastern Mediterranean collectively experienced an economic decline, many previously important centres recovered. The same cannot be said for Mycenaean Greece.

Perhaps the greatest problem is presented by the unspoken assumption that the 'great age' of the Mycenaean palaces represents a cultural high-point of the Aegean Bronze Age. Although the relative chronology of the LBA is well understood (if, perhaps, not sufficiently criticized) the same cannot be said for the absolute chronology of the region. Inevitably the debate surrounding the respective merits of the 'high' or 'low' chronologies has a definite impact, particularly in terms of lengthening the spans of LH I and II by around 100 years for the 'high' chronology (cf. Betancourt 1987; Betancourt & Michael 1987; Warren 1987; Warren & Hankey 1989; Manning & Weninger 1992; Shelmerdine 1997b: 539-41; Manning 1999; Manning *et al.* 2006; Tartaron 2008: 86-89). Beyond this, the absolute dating evidence and traditional chronologies are broadly in agreement (table I-1). Although it must be acknowledged that it remains to be seen how the latest evidence from Assiros concerning the substantial redating of the beginning of the Protogeometric period and the consequent impact on the preceding LH IIIC period will affect this sequence (Newton *et al.* 2005; Wardle 2005). This is undoubtedly a highly controversial issue (Tartaron 2008: 87-88), but in relation to this discussion would only serve to reinforce my argument, which is simply that this 'great age', in absolute terms, represents an extremely short period relative to the Bronze Age as a whole, and to the subsequent post-palatial and EIA periods.

Ceramic Phase	Absolute Dates (B.C)
MH	2000-1600
LH I	1600-1500
LH IIA	1500-1440
LH IIB	1440-1390
LH IIIA1	1390-1370
LH IIIA2	1370-1300
LH IIIB	1300-1190
LH IIIC	1190-1070
Submycenaean	1070-1015

Table I-1: Aegean Middle - Late Bronze Age chronology (after Bennet 2007a: 178, table 7.1).

Here we can see that the period generally viewed as representing the emergence of palatial society (LH I-II) spans approximately 210 years, whilst the ‘heyday’ of the palaces (broadly LH IIIA-IIIB) spans 200 years. Combined they span a period of around 410 years. However, between the collapse of the Mycenaean palaces and the widely recognized turning point of the 8th century B.C (e.g. Snodgrass 1971: 416-36; Morris 1987; Whitley 2001: 98; Dickinson 2006a: 256), lies 4-500 years of Greek history. In other words, between development and collapse, the Mycenaean ‘epoch’ lasted no longer than the so-called ‘Dark Ages’. Indeed, the preceding Middle Bronze Age itself endured for broadly the same time. It would be too extreme to characterize the palatial period as an aberration, but there is nonetheless a case for critically reassessing the historical primacy given to this period. Most discussions of Mycenaean palace society focus upon the emergence and development of a cultural highpoint for Aegean society, and the subsequent collapse, decline or devolution into a culturally bereft situation. But it may be more profitable to examine the palatial period as an interlude. In other words, the primacy given to the Mycenaean period imposes a false perspective upon the preceding Middle Bronze Age and subsequent developments in the EIA.¹

¹ A point recently made by Snodgrass (2000: xxxii) who, in respect of the period of the ‘Dark Ages’, rightly pointed out that: ‘for those archaeologists who believe in its current interpretation, one urgent task ... is to explain its long duration: to throw light on the process – often repeated in history – whereby a culture and a people with major attainments behind them, of a highly visible kind archaeologically, and with even more obvious ones ahead of them, were content for some centuries to pass during which there would not be, materially speaking, anything comparable to show.’

Investigating 'Mycenaean identities' and social dynamics

If there are problems with the established views of Mycenaean palatial collapse and the subsequent 'Dark Ages' how then are we to proceed? Even accepting that a social rejection of Mycenaean values occurred this does not necessarily translate into 'social revolution', internecine warfare or any other form of direct conflict. Social rejection suggests instead a situation in which an opportunity arose for previously subjugated, disassociated or disenfranchised individuals and groups to take advantage of changed conditions. This does not, however, require that all reacted in the same way. Rejection can be absolute, but it can also be by degree. As a basic model then, social rejection could potentially account for the regional differences in the post-palatial archaeological record. In other words, rather than forcing a homogenous explanation or scenario upon the entirety of mainland Greece, we should be open to the possibility of differing expressions or reactions to events regionally or locally. Indeed, the numerous issues surrounding relative chronology and chronological resolution make any statement concerning the totality of the Mycenaean world virtually impossible. We cannot be sure when precisely each and every palace, territory or state succumbed to the dissolution of Mycenaean power relative to each other. In this Mylonas (1966: 228) was undoubtedly correct in his assertion that this period must be studied at a regional or individual level. Nevertheless, to understand how such a rejection may have occurred we need to establish not only how the Mycenaean palaces operated and how the political system worked, but more importantly, how this was understood and viewed by the mass of the population.

Indeed, I would argue that the whole approach to such questions thus far has been too simplistic and that to understand the historical processes at work, we have to examine the driving force of human history: the historical figures themselves. In other words the focus of this study should start with the individuals and groups whose actions combined to create particular social conditions (cf. Barrett 2000; 2001). Essentially this means establishing how individuals contextualized their actions within their everyday local situation and the wider setting of regional socio-political groupings centred on the palatial infrastructure. To attempt such an understanding we have to look specifically at the nature of social identity at both individual and communal levels and how the construction, maintenance and mutability of these identities affected the relationships between the palatial institution and the general populace. This is undoubtedly a vast and complex topic in itself, but the fundamental question within the context of the collapse of the Mycenaean palatial system relates to the possible tensions and conflict arising from differing views of the world and the priorities and

understandings between a 'palatial society' on the one hand and wider, 'extra-palatial', social groups on the other. Identities, to a lesser or greater extent, are, after all, modified and reshaped through a continuous process of active participation in a social and physical world. As I hope to demonstrate, the ongoing social dialectic actively encourages and facilitates identity modifications at individual and communal levels. Inevitably this constant process of re-alignment and modification creates ontological differences in social perceptions which can lead to conflict and tension at one extreme, and feelings of disenfranchisement and irrelevancy at the other.

In effect, I shall be posing the question as to whether dichotomous and irreconcilable social identities and beliefs provided the conditions, if not the impetus, for the ultimate demise of the Mycenaean palaces and their attendant socio-political system. For I would contend that in rejecting this particular social form, individuals and groups were essentially restructuring their identity to exclude the 'Mycenaean elite identity' that once held sway. We might, for example, hypothesize that this is reflected in the alteration of burial forms from the collective interments characteristic of the Mycenaean period to the post-palatial and EIA characteristic of individual interment in cists and the previously rare practice of cremation becoming more widespread. Individualism, possibly a deliberate counteraction to the collectivity and sublimation of identity in the palatial period, becomes a more acceptable expression of social identity and 'place' the medium or mediator of group interaction and identity. In this way, the single, communal structures housing the bones of a group where individual identity is subsumed within the group is, over time, gradually replaced by cemeteries, such as at Perati or the Kerameikos, in which the group is identified by the individuals that formed it.

The notion that identity theory can help to unravel this confused tangle of evidence and events is not as obscure as it may at first seem. Archaeological and textual evidence strongly suggests that Mycenaean society was highly stratified and that during the long period of stability prior to the destructions was relatively secure politically, prosperous and achieving. The homogeneity of Mycenaean culture across regions is amply illustrated by the lack of regionalism in the pottery styles of the LH IIIB period, the seemingly common use of the Linear B script and administrative practice and the same imagery in artistic expression, for example. With the demise of the palaces, these elements start to change and regionalism, once hard to identify, becomes a significant and visible motif within certain areas such as ceramic production, whilst others disappear entirely. The hierarchical structure that was once so apparent becomes much harder to detect and over time the whole nature of society apparently undergoes substantial changes. Whatever the initial cause of the upheavals in the society, recent studies in the field of psychology indicate that such changes or instabilities in

the social *status quo* have a fundamental effect on how individuals and groups identify and interact:

For low status groups, social identity and group identification contain the seeds of action aimed at changing the *status quo*. According to social identity theory, social identity and identification should become particularly evident when the intergroup relationship is unstable or 'insecure'. In intergroup contexts where the status hierarchy becomes unstable, the social identity of members of both the low status and the high status group should become particularly relevant and salient, as 'cognitive alternatives' to the *status quo* become available, or are actually made available in the course of collective struggle. This salience should manifest itself in group action designed to realize or resist social change. The low status group should try to assert its own group in order to challenge the higher status group and the higher status group should try to fight the impending changes. Both of these strategies imply co-ordinated group action. (Doosje *et al.* 2002: 60).

This relationship appears to offer a potential solution to the conundrum presented by the archaeological record and to correlate with the dynamics of the LBA – EIA transition, if we understand the 'high status' groups in this model as referring essentially to the palatial elite and the 'low status' groups to the dispersed communities beyond the palaces. Naturally, the palatial community would have acted in a way to resist social change and with the extreme power differential between the palatial and extra-palatial communities, under normal circumstances, would have constituted the *status quo*. If, however, we introduce the destabilizing factor(s) of the destructive episodes then any inherent instability in the socio-political organization, however slight, could have been exploited. The precise nature of the destabilizing factor, whether it is was natural, economic, exogenous interference, internal unrest and conflict or any combination is less important than the social environment in which such events occurred. Furthermore, this model, by its very nature contains the seeds of variability. The reaction to change in the *status quo* need not be identical regionally or, indeed, locally. In 'realizing social change' groups will attempt to assert those elements that define their own identities, which may, or may not, bear some relationship to the previously dominant group. Whilst this can only be viewed as a working hypothesis, such observations do suggest that a study of social identities is crucial to our understanding of this transitional phase in early Greek society.

To achieve this, I shall focus upon the region of Messenia and the so-called 'Palace of Nestor' at Pylos. Whereas the archaeological record for this period, across all regions, is biased towards the palaces, with relatively few intensively investigated satellite sites available for comparison, the excavations at Pylos uncovered an invaluable source of comparative data in the form of the Linear B tablets. This documentary evidence makes it

possible to redress the balance to a degree, and to provide an almost unparalleled opportunity to examine socio-political relationships between, and the cohesiveness of, palatial society and outlying communities. From a methodological perspective, the Pylian corpus is the most suitable for such an investigation as not only does it represent one of the largest single groups of documents, but unlike the Knossos corpus, for example, it forms a chronologically and spatially coherent group. The majority, indeed, were recovered from a single location within the palace, commonly referred to as the Archives Complex (hereafter abbreviated as AC) – an area interpreted as scribal offices and the administrative heart of the palace. The evidence these texts provide is, however, far from straightforward. Despite the largest class of words in the Mycenaean lexicon being personal names (Chadwick 1973: 92; Nakassis 2006: 132), the documents themselves are conventionally regarded simply as bland administrative accounts whose primary historical interest lies in elucidating the economic concerns of the palace.

The strength and longevity of this interpretive model seemingly precludes any substantial discussion of social or political concerns, but it cannot be denied that the texts' content includes a wealth of data directed at individuals, communities, organizations and locations within the polity. As such, they appear to address the basic elements of individual, communal and corporate identities within, and beyond, the palace environment. Therefore, before attempting to interrogate the documents themselves, it is first necessary to assess the validity of the administrative model and understand the nature and role of these documents within the palace environment. The approach that I will adopt may be considered controversial, but seeks to address not only the broader questions outlined previously, but also to re-examine the contextual framework in which documentary analyses are conducted and consequently the possible purpose behind their composition. In the following chapter, therefore, I subject the AC to a critical re-analysis from an archaeological perspective, before proceeding to an examination of the documents themselves. This will take the form of a series of case studies examining the motivation and context behind their production, their socio-political role and the nature of the documented personnel.

Chapter II

Nestor's 'Archives Complex': Fact or Fiction?

The students nodded, emphatically agreeing with a statement which upwards of sixty-two thousand repetitions in the dark had made them accept, not merely as true, but as axiomatic, self-evident, utterly indisputable.

Aldous Huxley, *Brave New World*.

Before attempting to analyse the Pylos documentary corpus itself, a full appreciation and understanding of the archaeological context in which these documents existed must be considered essential (e.g. Bennet 1984; 1988; Palaima & Shelmerdine 1984; Palaima 2003). At Pylos, documents were discovered in several contexts, but far-and-away the most significant were Rooms 7 and 8 of the main palace together known as the Archives Complex (AC). With the recovery of approximately 80% of the total Pylian corpus from this location, it seemed entirely logical to the excavators and to the wider scholarly community to conclude that this architectural complex represented a documentary archive comparable with discoveries in Mesopotamia or Hittite Anatolia.² This interpretation has remained a constant in Mycenaean studies from the initial exploratory excavations at Pylos, through the completion of the excavation and publication programme, up to the present day with the ongoing detailed analysis of the documents themselves (e.g. Kourouniotis & Blegen 1939; Blegen 1953; Blegen & Rawson 1966; Chadwick 1976: 18-20; Palaima & Shelmerdine 1984; Palaima & Wright 1985; Palaima 1988: 171-89; 2003; Kyriakidis 1996-1997 [1998]; Pluta 1996-1997 [1998]; Shelmerdine 1998; Bennet 2001; Firth 2006). To all 'students' of Mycenaean Greece, this has become an 'indisputable' fact, forever unquestioned, unarguable and absolute.

The implications of this are profound for it provides the foundation upon which not only the current understanding of the role and purpose of the Linear B tablets is based, but also many wider issues such as, for example, the development of writing, the nature of scribal craft, and the development of models of social, political, economic and administrative operation and structure within Mycenaean society. Yet despite playing such a crucial role in Mycenaean

² The figure of 80% is an approximation based upon the figures quoted by Palaima (1988; 2003: 181) but for the purposes of this analysis it is the proportion that is significant rather than the precise figures of distribution. As has previously been noted (e.g. Bennet 2001: 27-28, n. 18) the precise figures are, to a degree, potentially in flux since the overall figures for the number of tablets in various areas can change with the recognition of new joins. Such changes are likely, however, to be small and largely insignificant with respect to the overall pattern of distribution.

studies, and although various refinements to the model of structure and operation of the complex have been made (e.g. Wright 1984; Palaima & Wright 1985; Palaima 1988; 2003; Pluta 1996-1997 [1998]), the basic premise has never been subjected to the critical questioning or examination necessary for providing an accurate context or framework in which to situate textual analyses. Yet there are numerous interpretive problems presented by the accepted model that undoubtedly have an impact on how the documents may be interrogated and the kinds of information that we may expect them to provide. These not only involve issues surrounding the physical reconstruction of the architecture of the AC but also questions of how the rooms operated, the movement of documents to and from these rooms, the nature of any potential archival storage of tablets, the relationship of the tablets to other artefacts found at this location and ultimately what the purpose of these documents was.

The Archives Complex at Pylos

The existence of this complex first came to light during exploratory excavations in 1939 when a partial excavation of the room later designated Room 8 was conducted (Kourouniotis & Blegen 1939). These excavations uncovered some 600 tablets scattered on and around a clay bench that ran around the three sides of the south-western half of the room which, despite the excavations failing to uncover its entire extent, led to the crucial interpretation and labelling of it by the excavators as an 'archives-room' (*ibid*: 564, fig. 7, 569). Even from this early stage then the investigation of this area was influenced and conditioned by preconceived notions of function and operation and, by implication, the expectation of discoveries to follow. How much future investigations of the area were directly affected by this interpretive label, though, is uncertain, but there can be little doubt that to some extent all subsequent reconstructions and interpretations were biased by these statements.

With the resumption of excavations in 1952, the remainder of the AC was uncovered along with the central Megaron, vestibule and much of the Portico of the Main Building (Blegen 1953; Blegen & Rawson 1966: 7-8). Excavations of the AC revealed a largely incomplete architectural plan with only a section of the dividing wall between the rooms preserved; the remainder having been robbed of their stone, possibly in the Venetian period.³ The resultant trench, although creating considerable difficulties in architectural reconstruction did, however, provide an outline from which it was evident that, whilst appearing to be structurally very similar, the outermost room (designated Room 7) at approximately 4 m²

³ This is based upon the discovery in the backfill of the Chasm of a gold ducat identified as being minted by the last Doge of Venice during the 18th century, suggestive of a date around 1797 (Blegen & Rawson 1966: 96).

was somewhat larger in area than the adjoining Room 8. Fortunately, despite the destructive effects of the fire that engulfed the palace and this later quarrying, the contents of the rooms were remarkably well preserved and undisturbed. Although Room 7 produced what can only be described as an eclectic mix of artefacts, the most significant discovery was the substantial additional collection of tablets from both rooms. It seemed clear then to the excavators, and indeed to the scholarly community as a whole, that the initial interpretation of these rooms forming an AC was correct. Superficially, it appears impossible that the sheer quantity of tablets in this place could be interpreted otherwise.

During subsequent excavation seasons several smaller caches of tablets were recovered from various rooms within the palace and areas outside the Main Building, but none were remotely as substantial as that from the AC. With the completion of the excavation programme, the discoveries surrounding these rooms could be placed within a wider palatial context and the AC, as it came to be known, was seen to be located at a significant point of access within the palace complex (figure II-1), proving the fundamental hypothesis that a central administrative archive occupied a core role in the day-to-day operation of the palace. Nevertheless, the seemingly coherent and persuasive picture that emerged was ultimately only ever founded upon the basic assumption that the presence of so many documents in one place was indicative of a functioning palace archive. The undoubted influence of the tablets in defining architectural function was later duplicated following the excavation of the North-East Building, where again it was the documents themselves that were instrumental not only in the initial interpretation of the role of this building, but also in the subsequent modifications which saw it labelled as a workshop (Bendall 2003: 182-84). As discussed in Chapter IV, I suggest that undue emphasis has been placed on the documents in understanding the archaeological evidence in the AC and that as a result a critical, objective analysis of the reconstruction, whether architectural or operational, has been neglected. Before examining the surviving architecture and the reconstruction of the AC itself though, due consideration must be given first to the feature that has removed so much of the direct evidence for how the two rooms were originally constructed – the so-called ‘Chasm’.

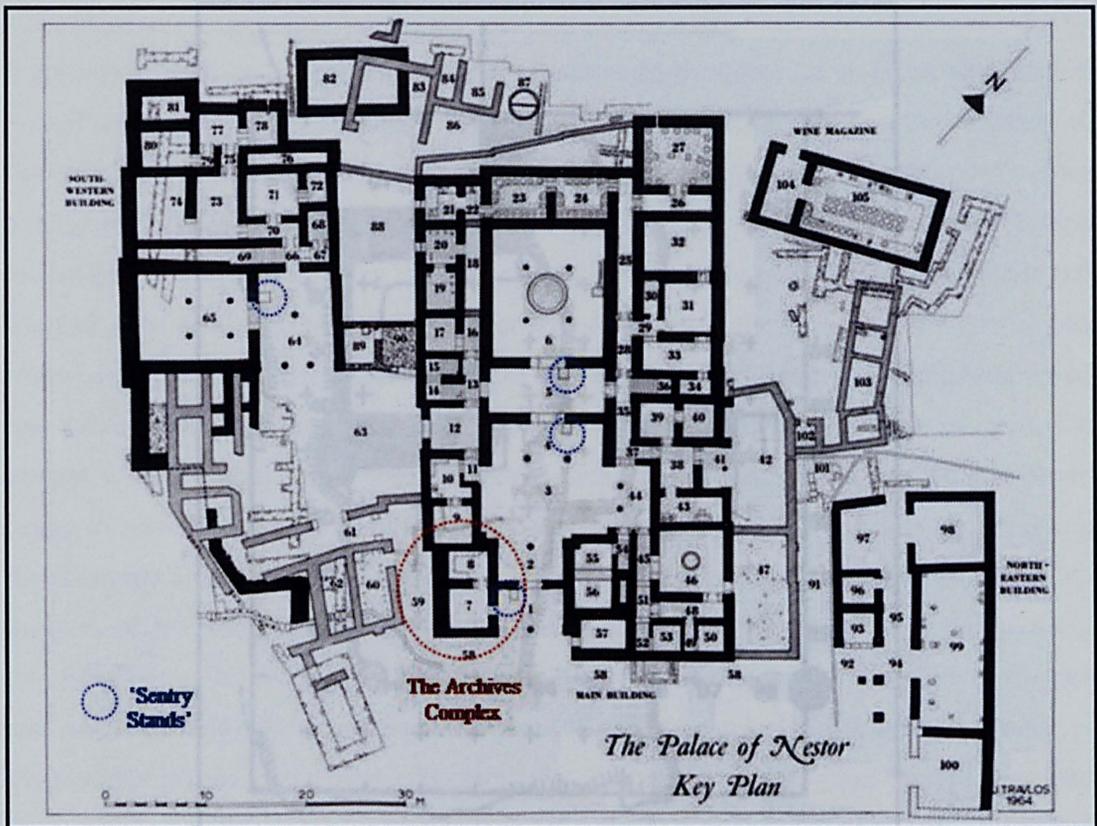


Figure II-1: Key plan of the Palace of Pylos (after Blegen & Rawson 1966).

The 'Chasm'

The surviving architecture clearly militates against a full understanding of the function of the rooms, for with the exception of a section of the partition wall the remainder were destroyed to their foundations. The primary reason for this lies not in the destructive effects of the fire that engulfed the palace, but in the activities of later stone robbers retrieving substantial quantities of architectural stone from the site as a whole. Around the AC these activities are manifest in the dramatically dubbed 'Chasm'; an irregular trench circumnavigating the limits of the two rooms (figure II-2). However, as with many of the labels attached to features in and around the palace, the Chasm is a somewhat misleading name applied to only one section of a much larger trench that tracks the south-western façade of the Main Building. Conventionally, only the section relating to the boundaries of Rooms 7 and 8, the adjoining vestibule and Inner Propylon is commonly referred to by this name (Bennett 1964: 247).

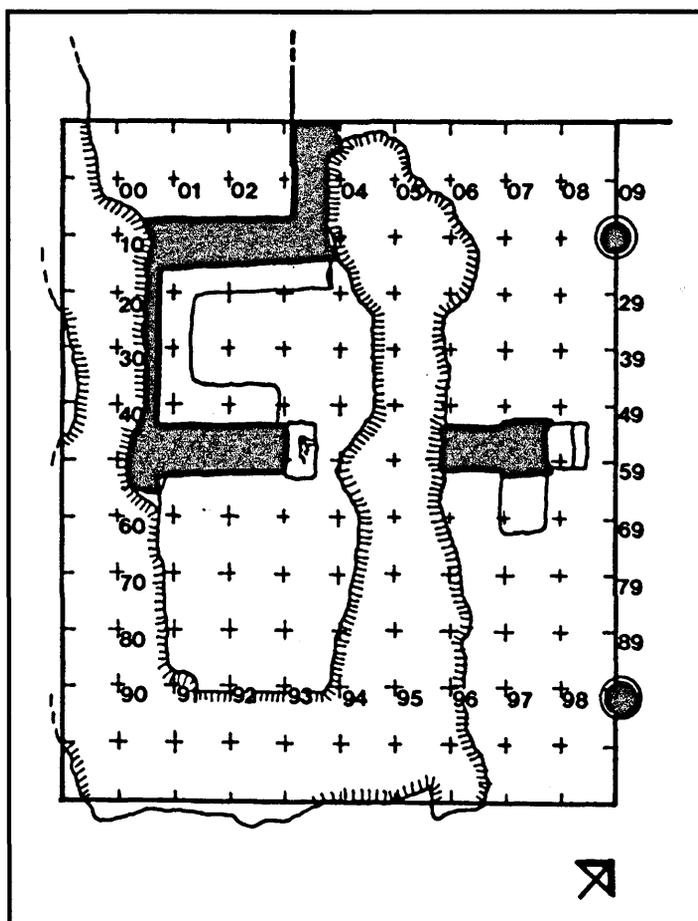


Figure II-2: Plan of the Archives Complex and the Chasm (Palaima & Wright 1985: 256, Ill. 6).

In order to fully comprehend what was removed, we need to understand the philosophy behind the creation of this trench, which in turn entails a brief consideration of the entire process of stone removal. More specifically we need to establish, as far as is possible, the direction in which stone extraction occurred and what this can tell us in terms of the original architecture and the distribution of artefacts within the Chasm. Unfortunately, little reference is made to the nature of this trench in the final report beyond its general effect upon the architectural remains, with only the Chasm itself subjected to any level of discussion. Nevertheless, the trench continues for a substantial distance northwest beyond the AC affecting, to a greater or lesser extent, the south-western walls of Rooms 9, 10, 12, 14, 15 and 17, and it is here that in all likelihood stone removal originated (Blegen & Rawson 1966: 45). Topographically this would be the most logical scenario as this location represents the highest point the trench reaches on the hill of Ano Englianos. If the cautious dating of this action is correct, then the precise nature of the site would have been unknown and the recognition of useful building material only made through the effects of erosion exposing surviving architecture that was preserved to a reasonable height. Such preservation is not apparent towards the south-eastern part of the site.

It appears likely then that the quarrying was initiated northwest of Room 8 and continued in a south-easterly direction, tracking the western side until reaching the southwest corner of Room 7. From here, turning east, quarrying continued along the outer wall of Room 7 before turning northwards at the south-eastern corner. It is here that the trench morphology begins to change noticeably, suggesting that the 'robbers' were unsure of the width of the walls and whether they continued in multiple directions. As a result the corner of the trench was widened to expose the full width and nature of the east wall of Room 7. Having established that the wall did not continue to the north-east but turned instead to the north-west, the eastern wall of Room 7 was followed northwards, with the natural effect that the trench began to narrow to the width of the wall before arriving at a point opposite the surviving threshold block of the doorway between the rooms. Here, there is a perceptible widening into the area of Rooms 7 and 8 suggesting that this allowed for the removal of an opposing threshold block. Beyond this, the trench immediately narrows to the known width of the east wall and continues until the northeast corner of Room 8 is reached. Once again, the trench is irregularly widened to expose the corner blocks. Presumably, at this point enough stone had been removed as the wall beyond Room 8 (the east wall of Room 9) was only marginally attacked.

Understanding and appreciating this sequence of events is crucial as it has a direct bearing on the restoration of the external doorways and, in particular, the commonly accepted doorway connecting Room 7 with the Outer Portico. This reconstruction, which has remained essentially unchallenged to this day, was argued for primarily upon the apparent widening of the Chasm immediately south of the threshold between Room 7 and 8 and the relative positioning of a 'sentry stand' adjacent to Room 7 (*ibid*: 93). Yet the extraction sequence demonstrates that this widening is in reality a narrowing and as such no evidence for a doorway can be discerned in the features of the Chasm. Indeed, if we were to understand this as a widening, as Blegen proposed, then an explanation would have to be found for the extreme widening of the Chasm at the south-eastern corner of Room 7, which appears excessive even for the removal of the corner blocks of the structure. As there is no evidence for any other structural components that might have been of use or that any attempt was made to extract stone from beneath the bedding layer, we are forced to conclude that widening occurred simply to expose the direction of construction. Moreover, the evidence of the ashlar blocks belonging to an earlier phase of construction remaining untouched and in-situ beneath the floor of Room 7, shows that the philosophy behind the quarrying was based not only upon the quality of the building stone, but also upon ease of access.

Even considering the process of stone extraction in this simple fashion, the irregularities in the trench morphology become meaningful, and it is immediately evident that very little can be inferred from relatively minor changes. This process was clearly methodical and progressive, chasing the line of the exposed wall opposite Room 17 south-east until the limit of Room 7 was reached, before changing course to follow the architectural plan. Changes in the trench morphology therefore have to be understood in relation to this process rather than to isolated features. Indeed, whilst much has often been made of areas where such changes are discernible, the overall plan and photographic record of the full extent of this 'robber trench' is revealing (*ibid*: figs 20, 21, 407). The stretch northwest of Room 8, particularly around Room 10 and 12, does not possess a regular profile but is extremely ragged, most noticeably on the exterior sides of the walls being removed. This is, of course, entirely logical given that the only interest in these operations was to remove building stone and so the only regular face to this trench that we should expect to see is that exposed when the walls were removed. It is also worth noting here that the 'robbers' would, in many areas, have been digging through thick stucco surfaces, which undoubtedly would have fractured and broken in an irregular plate-like fashion exacerbating the ragged nature of the trench-edge opposing the walls.

Some confirmation of this sequence is provided by the pattern of artefact recovery from the Chasm, particularly the pattern produced by the find-spots of adjoining tablet fragments. Crucially, the vast majority of tablets and tablet fragments were recovered from the north-western extent, with the distribution density diminishing considerably towards the southern end of Room 7 (figure II-3 below; also figures III-2 and III-6 in chapter III). Yet an examination of these indicates that a great many almost certainly originated from the discrete clusters of tablets found on the floor adjacent to the south-east wall of Room 7. In addition, the tablet distribution within the Chasm stops short of the point corresponding to the southernmost corner of Room 7 – a pattern that is unlikely to have been produced if the quarrying process had occurred from north to south. This suggests quarrying from south to north, with the tablets formerly resting on the floor of Room 7 having been pushed northwards as the removal of stone continued.

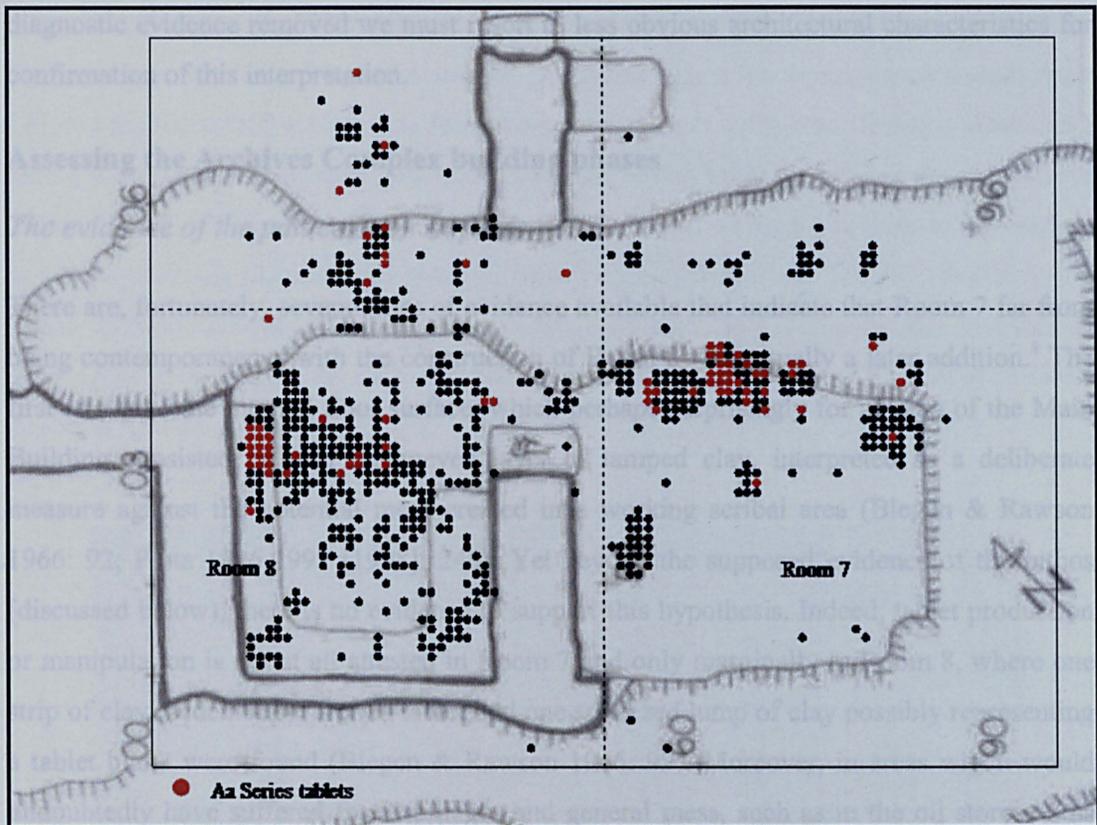


Figure II-3: The overall tablet distribution within the Archives Complex.

The importance of these observations lies not only in their impact upon the architectural reconstruction initially proposed, but also upon subsequent analyses of the architecture and operation of the rooms. With only a short length of the partition wall surviving, the majority of the reconstruction was almost exclusively based upon the negative evidence provided by the Chasm (*ibid*: 92). When combined with the function implied by the artefacts recovered internally, this inevitably led to a series of assumptions being made as to the nature of the original construction. The first of these is simply that the Chasm was evidence for the construction of fully enclosing walls, an entirely understandable conclusion given that the Chasm would seem to indicate substantial foundations to the enclosing structures, and the bench clearly backed on to a continuous wall (of unknown height). Nevertheless, the existence of such foundations does not necessarily demonstrate that the superstructure would have been uniform in design or all-enclosing. The second, and more important, assumption was that both rooms were constructed concurrently, again entirely logical given the discovery of the tablet caches and the likelihood that both rooms functioned in tandem. However, the loss of any direct evidence of differential phasing with the removal of the foundation courses does not, in and of itself, preclude the possibility of chronological differences in the construction of the rooms or changes in functional requirements. This issue is undoubtedly crucial to any understanding of the functioning of the AC, but with the most

diagnostic evidence removed we must resort to less obvious architectural characteristics for confirmation of this interpretation.

Assessing the Archives Complex building phases

The evidence of the palace floor surfaces

There are, fortunately, several lines of evidence available that indicate that Room 7 far from being contemporaneous with the construction of Room 8 was actually a later addition.⁴ The first relates to the internal floor surface, which perhaps surprisingly for an area of the Main Building consisted only of an uneven layer of tamped clay, interpreted as a deliberate measure against the potential mess created in a working scribal area (Blegen & Rawson 1966: 92; Pluta 1996-1997 [1998]: 246). Yet beyond the supposed evidence of the pithos (discussed below), there is no evidence to support this hypothesis. Indeed, tablet production or manipulation is not at all attested in Room 7 and only marginally in Room 8, where one strip of clay evidently cut from a tablet and one squeezed lump of clay possibly representing a tablet blank were found (Blegen & Rawson 1966: 99). Moreover, in areas which would undoubtedly have suffered from spillages and general mess, such as in the oil store-rooms (Rooms 23 and 24), floors were surfaced with stucco; a feature that was contrastingly described as facilitating the cleansing and maintenance of the rooms (*ibid*: 136). Pluta's (1996-1997 [1998]: 246) assertion that the floor in an area continuously being affected by water spillages would not be stuccoed is refuted not only by the use of stucco in the courtyards, where it would presumably have been constantly exposed to the elements, but also by Room 43. Here, where a bath was installed alongside a clay bench containing two large jars possibly for containing oil and water, stucco was liberally used and evidently subjected to several re-applications (Blegen & Rawson 1966: 185-89). There is, therefore, no obvious functional reason why the floor of Room 7 should have been kept as a simple tamped clay surface, which is, besides, relatively rare throughout the palatial complex.

This line of enquiry has never been fully explored but as I will show, the pattern of flooring is extremely revealing with respect to the palatial building phases. In the following plan (figure II-4), the floor surfaces across the rooms of the palace, associated buildings and courtyards have been illustrated, recognizing three basic surface types: areas where no floor was preserved, clay or earth floors and stuccoed surfaces. No distinction is made as to the details of each type. So, for example, decorated stucco is not distinguished from plain stucco and the diagram does not portray the true state of preservation as revealed by excavation;

⁴ This question does not appear to have been asked by Nelson (2001) in his reanalysis of the palace architecture and phasing.

rather it shows what may be inferred from the surviving remains.⁵ One instance of this is in the vicinity of the Wine Magazine and the North-East Building, concentrated around Area 103, where the entire surface has been extrapolated from only trace remains discovered during excavation. In this case, the incomplete intervening building remains make a general understanding of individual floor surfaces problematic and so here, the floor is represented as stucco although various areas were in reality unpreserved (*ibid*: 336-40). Similarly, no details of earlier phases of flooring are included, for which there is considerable evidence in many areas of the palace. As such, this plan represents the situation immediately prior to the destruction of the palace.

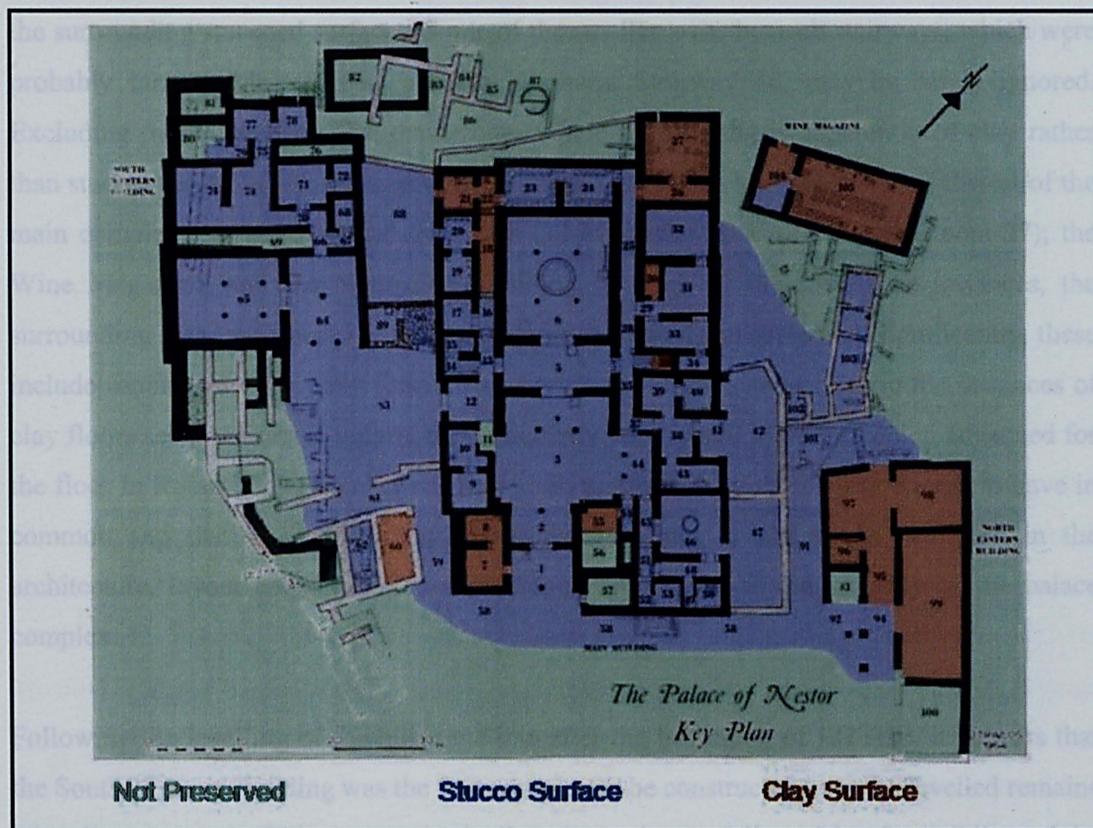


Figure II-4: Floor plan of the Palace of Pylos.

The immediate impact of this analysis is to reveal how rare variations in the type of flooring are. Ignoring those areas within the complex for which no evidence was preserved, the vast majority of floors, both internally and externally, are paved in stucco. The extent of this flooring is in some outlying areas undetermined through erosion, disturbance and other natural or anthropogenic post-depositional effects, such as to the south and south-east of Courtyard 58 where the natural slope of the hill led to increased erosion, and in the area

⁵ This data has been generated from a close study of the Blegen and Rawson's (1966) report. Many of the areas contain only trace evidence that may be indicative of incomplete renovations. Nevertheless, where traces were noted it seems likely that we can reliably infer either that the surfaces were finished in stucco, or that the intent was to create such surfaces.

between the North-western and South-western Buildings and Court 88. For these the floor surface has been shown as not preserved, but certainly in the former case the extent of the stucco surface was, in all likelihood, considerably greater, undoubtedly extending to the conjectured colonnade that surrounded the entrance to the south and south-east of the complex (*ibid*: 228). Indeed, given the overall pattern, it is not unreasonable to suppose that the various locales within the complex that are shown as unpreserved were, in all probability, paved with stucco.

The importance of this plan lies in a comparative examination of those areas which appear to have been deliberately provided with tamped clay or earth floors and their relationship with the surrounding stuccoed surfaces. Some of the smaller areas beneath stairways, which were probably inaccessible, such as beneath Northeast Stairway 36, may be safely ignored. Excluding these, there are five main zones where the flooring is apparently of clay rather than stucco: the AC itself; the area of the pantries (Rooms 21 and 22) to the northwest of the main complex; the areas around the North Oil Magazine (Corridor 26 and Room 27); the Wine Magazine; and the North-East Building. In each of the first three instances, the surrounding areas are floored with stucco, some decorated and some not. Significantly, these include rooms that apparently function as working or storage areas making the instances of clay floors seem entirely incongruous, particularly with regard to the reasoning advanced for the floor in Room 7. Yet there is one feature in particular that these areas appear to have in common and that is that they encompass late changes or alterations identified in the architecture, layout and operation of the rooms in relation to the majority of the palace complex.

Following the levelling of the hill sometime after the beginning of LH IIIB, it appears that the South-Western building was the first complex to be constructed over the levelled remains of earlier structures. This, it was originally proposed, was followed by the building of the main palace complex and then by the Wine Magazine and the North-East Building. Although presenting a basic sequence of unit construction, Blegen essentially viewed the Wine Magazine and North-East Building as contemporaneous with the Main Building solely by virtue of what he considered to be an operational necessity (*ibid*: 32-33, 423). In this scheme, later buildings were also constructed to the south-west, enclosing Court 63, to the north-west and on the south-east of the palace buildings creating the enclosed Courts 42 and 47 as well as the complex between the Wine Magazine and the North-East Building. Here at least there is stratigraphic evidence to show that both the South-Western Building and the Main Building were in place prior to the construction of the complex to the extreme south-west and the creation of Courts 42 and 47.

More recently though, a number of studies have shown this basic sequence to be incorrect, particularly with regard to the Wine Magazine and North-East Building (Wright 1984; Shelmerdine 1997a: 394-95; 1997b: 545; 1998; Hofstra 2000: 72-73, 281-83, 295-97; Nelson 2001: 29-44, 212-16; Bendall 2003; 2004: 116; Westerburg 2003). It now appears that the North-East Building is a late construction, post-dating the creation of Courts 42 and 47, and was itself subject to at least two phases of building works, with the last dating to the period immediately prior to the destruction of the palace (Westerburg 2003: 9-10). In the case of the Wine Magazine, the most telling feature of the construction is the overall alignment of the building, which mirrors only that of the wall enclosing the north-western extent of Court 88 and the complex of Rooms 83-87. In the latter case, stratigraphic evidence with the underlying Room 82, an independent unit apparently closely associated with the early phases of use of the South-Western Building (Blegen & Rawson 1966: 289), shows these to be much later additions. The construction of the wall enclosing Court 88 can then be seen as a measure preventing direct access through from these rooms as well as from the Wine Magazine.

To complicate the understanding of this sequence, several areas were evidently subjected to repairs, modifications and additions, attesting to an ongoing process of architectural adaptation. The most well-known of these concerns the pantries (Rooms 21 and 22) and the adjoining Room 18, which were clearly remodelled not only providing fixed storage facilities for the ceramic assemblages found there, but also altering routes of passage by closing off access along what would have been a long corridor that combines Rooms 13, 16, 18 and 22 and, with the inclusion of a new external doorway to Room 21, granting access only via Court 88 (Blegen & Rawson 1966: 116, 119, 129-30; Wright 1984: 20-21). Modifications to the internal form of rooms are also evident in Rooms 55 and 56 at the south-eastern extent of the Main Building, although here substantial later disturbance makes an assessment of these changes problematic. Whilst this, and similar changes to the external façade of Rooms 17, 19 and 20 and the compartmentalization of the internal corridor that had flanked these rooms involved modifications, new additions to the Main Building are also readily apparent, such as the North Oil Magazine (Room 27) and the adjoining Corridor 26.

Globally, these changes have been interpreted as pre-empting some form of external threat that ultimately heralded the destruction of the palace (Wright 1984; Shelmerdine 1997a: 394). Yet despite the recent discovery of a possible circuit wall surrounding the hilltop of Ano Englianios (Shelmerdine 1997b: 545-47; Zangger *et al.* 1997: 610-13), there is no evidence to support the notion that these changes were undertaken as a defensive measure.

Indeed, many of the modifications and new constructions appear to be focussed upon the external courtyards and were seemingly concerned with the control of access and the altering of routes in and around the palace. Moreover, the majority of the changes to the internal form of rooms and their associated function relate to provisioning issues – the increase in pantry space as well as the apparent division of pottery sets according to quality (Bendall 2004), the increased provisioning of wine and oil and the augmentation of areas related to industrial activities of procurement and storage (e.g. Hofstra 2000: 280). As such, I suggest that the motivation was entirely social in foundation and served to alter not simply the perception of the palace but the physical access to particular groups of people.

Comparing the proposed building sequence with the flooring evidence is extremely revealing. Every instance of a clay floor is associated with a late phase of construction or of architectural modification. Thus the clay floors in Rooms 21 and 22 and the associated Corridor 18 correspond with the remodelling undertaken to equip the rooms for pottery storage and to allow access from the exterior Court 88. To the north-east, structural evidence shows that Corridor 26 and Storeroom 27 were late additions to the structure of the Main Building (Wright 1984: 21). In both cases, the process of construction would have necessitated the provision of new flooring: for Corridor 26 and Storeroom 27 simply because they were new rooms, while the substantial structural modifications within Rooms 21 and 22 and the provision of new thresholds would have destroyed the existing surfaces. I suggest, therefore, that the clay floors are indicative of an incomplete process of conversion with this element naturally being the last to be completed. An indication that a tamped clay surface was simply a preparation for the laying of a stucco floor is provided by the Wine Magazine, a further late addition. Here, in the vestibule Room 104, an incomplete (through preservation as opposed necessarily to incomplete construction) floor surface was discovered. In areas where the stucco had not survived, patches of a clay floor underlying the stucco were observed, which Blegen was most likely correct to suggest was a bedding layer (Blegen & Rawson 1966: 343). So it is possible that here we may conjecture that the final phase of construction was underway and the Wine Magazine was nearing completion. Similarly, the existence of a clay floor in Room 55 can be associated with significant internal modifications, although the reasoning remains unclear (*ibid*: 222-26, fig. 424).

Outside the Main Building, the complex surrounding Room 60 is demonstrably a late construction mirroring the modifications made to Rooms 20 and 21, and is intimately associated with the changes in access to the outer courts and to increasing storage facilities necessary for accommodating the banqueting and feasting activities occurring nearby (Wright 1984, Bendall 2004). Inside Room 60, provision had already been made for shelving

and the ordered nature of the ceramic assemblage shows that it had been partially outfitted, but I also suggest that the work was incomplete. Whilst it may be imprudent to place much emphasis on the missing threshold block at the entrance, amongst the debris filling the room decorated plaster fragments were recovered indicating not only that there was an upper storey to this building, but that it was finished with painted stucco (Blegen & Rawson 1966: 237-40). It seems highly unlikely that Room 60 would have remained undecorated, particularly since the entrance fronts the access to the inner courtyards via Ramp 59 and presumably, at times, would have been visible to those attending the palace. A similar situation exists with the North-East Building where rooms appear to have been outfitted with the necessary fixed furniture, but no stucco floors have been laid. This appears to be simply the result of incomplete modifications to the architecture rather than a deliberate policy since Colonnade 94 approaching the entrances is itself provided with a stucco floor, but Corridor 95 leading directly off this approach is not, yet there is no threshold or doorway between the two. In Room 98 however, the floor was found to resemble closely the floors of the AC (*ibid*: 315) and, unlike the neighbouring Room 97 where the walls had nothing applied to them, here the walls did have clay applied. Similar patches were also found applied to the walls of Room 99 suggesting that the North-East Building, despite being in use, was the subject of an ongoing process of construction when the palace was destroyed.

The only areas within the Main Building where clay floors truly exist are beneath stairwells, such as Northeast Stairway 36 and Southwest Stairway 14-15, and seemingly also the single, narrow Room 30.⁶ Despite this one problematic area, it is evident from figure II-4 that the clay floors in accessible parts of the palace are connected with late modifications to the architecture. Given this pattern of associations, the only unexplained areas within the palace complex are the two 'archive rooms'. I suggest contrary to the established view, that the clay floors are direct evidence for a process of structural change focussed on the AC. More precisely, I suggest that Room 7 was a late construction and addition to Room 8, which would have involved at some stage the installation of an interconnecting doorway. Such works would of course necessitate the clearing of the area around Room 7 as well as causing significant damage to the walls and floor of Room 8. During such architectural modifications, involving a significant degree of renovation and redecoration, the plastering and any painting of the floor would through necessity be the final stage of the process of refurbishment. However, in preparation, both to act as a temporary, functional surface and to provide a foundation for subsequent rendering, the rooms were provided with tamped clay

⁶ This room, with a slab of crude brick narrowing the doorway, may never have been intended to be visible or regularly used (Blegen & Rawson 1966: 151-52). As the only artefacts found here appear to have fallen from above, little can be deduced as to the functioning of this space.

surfaces; the fact that these surfaces were reportedly very uneven simply reinforces this. Further confirmation for the ongoing modification of the AC, and in particular Room 8, is provided by the clay bench that forms an integral part of the internal architecture.

Benches, doorways and 'sentry stands'

The so-called 'bench' surrounding the north-west, south-west and south-east walls of Room 8 represents the only fixed architectural feature found within the AC. The construction, described as being of crude brick or clay and finished in a rough clay plaster, has been assumed to function not only as a seat, but also as a surface on which the scribes could lay out tablet sets (Kourouniotis & Blegen 1939: 563-64; Blegen & Rawson 1966: 96; Palaima & Shelmerdine 1984: 80; Palaima & Wright 1985: 261; Palaima 1988: 183). Significantly, the bench has also been taken as convincing evidence that Room 8 is indeed part of an AC, as this type of architectural feature appears to be common amongst other known ancient archives, particularly in Mesopotamia (Pluta 1996-1997 [1998]: 240). Yet because Pylos is the only potential example of a functioning archive within the Aegean world, it is doubtful whether such direct comparisons are valid. Nevertheless, given that such care was lavished by the builders at Pylos on what appears to be the only permanent furniture that the room was provided with, it is clear that whatever the purpose for which Room 8 was envisaged, the bench was a vital component in its operation. It is therefore surprising that this feature has largely been ignored, dismissed simply as an inevitable component of an archive room. Yet considering their apparently mundane function, benches are relatively uncommon in the palace and this alone makes them worthy of greater attention. One other bench exists in the nearby Room 10, although intriguingly, the construction characteristics of these two benches are similar to those of the stands provided for the storage of pithoi in Rooms 23 and 24 of the Oil Magazines, of one in Room 10 and, to a lesser degree, the stand in Room 43.

The bench also provides evidence for the late modification of Room 8 and the incompleteness of the building works. Crucially, the Room 8 side of the partition wall was finished in the same coarse plaster as used on the bench (Blegen & Rawson 1966: 95-96), consistent with the notion that Room 8 was awaiting final decoration. Comparison with other such constructions within the palace shows that, in all likelihood, this would have eventually been finished with a coating of stucco, and possibly later with painted decoration. Particular confirmation comes from the stands in Rooms 23 and 24, where not only does the stucco survive on bench-like structures used to enclose oil-storage pithoi, but also where there is incontrovertible evidence that this surface and the stucco floor were laid at the same time (*ibid.*: 135). A useful contrast also exists between this and the only other bench, that in Room

10, where once again the upper surface has been finished with decorated stucco (*ibid*: 104). Further corroboration, if any is needed, is provided by the larnax in Room 43 supposedly used as a bath, which has been set into a stand of very similar construction that once again has been finished in stucco. Nearby, a further stand containing two large jars was constructed that was again finished in a similar fashion with painted decoration but was evidently subjected to several coats of stucco indicative of different phases of redecoration (*ibid*: 187-89). It seems then that the rough plaster finish to the bench, along with the evidence of the flooring, is clearly representative of an incomplete programme of refurbishment and decoration. But there is one further line of evidence that supports this and indicates that Room 7 was a later phase of construction and this concerns the interconnecting doorway between the two rooms.

Although much of the doorway in the partition wall was removed in the same extraction process that created the Chasm, we are fortunate that one of the threshold blocks holding the doorjamb was sufficiently well-preserved for certain key details to be discerned. In particular, the positioning of the socket for the doorjamb and the rabbeting for the doors themselves indicate that it would have opened from Room 8 into Room 7 (Blegen & Rawson 1966: 95, fig. 432; Palaima & Wright 1985: 254-56), entirely consistent with this wall originally having been an external wall to the palace complex. This substantial threshold stone, extending beyond the width of the partition wall by approximately 0.1m in either direction, was evidently laid over the foundations of the wall separating Rooms 7 and 8, slight evidence of which was found to extend beneath the width of the door. These features alone suggest that the door was a later addition to the façade of Room 8, but in combination with the direction in which the door apparently opened suggests that this modification occurred somewhat before the construction of Room 7.

Indeed, the key observation is the direction in which the door opens in relation to the supposed operation of the AC. Conceptually the complex has been viewed as operating as two interdependent units: Room 7 as the working area and delivery point for the tablets and Room 8 the storage area. In this model, the primary direction of movement is from external areas into Room 7 and finally to Room 8, yet the interconnecting doorway, counter-intuitively, opens in the opposite direction against the flow of traffic. The door would appear then not to have been fitted with the notion of scribal use in mind but with a more pressing concern of preventing uncontrolled movement in that direction. Such a concern would seem unjustified when further passage from Room 8 into the Main Building is, itself, mediated by the door connecting Room 8 with the Inner Propylon. The direction would appear only to be important if the door between Rooms 7 and 8 was, at some time, an external door on the

façade of the Main Building, where it would have been desirable for it only to be opened from inside the palace. If so, traffic would conventionally pass from the Inner Propylon into Room 8 and then eventually Room 7.

The distribution of one particular cache of tablets rarely considered and, I suggest, misunderstood in analyses of the AC helps to demonstrate this. During the excavations, a small but significant group of tablets was found on the floor of the Inner Propylon adjacent to the Chasm and Room 8, their location generally regarded as resulting from scatter produced by the destruction of the AC and by the later quarrying that created the Chasm (Blegen & Rawson 1966: 97). A closer examination of the pattern of the interconnecting tablet fragments (figure II-5), however, reveals that this group in all likelihood did not originate from Room 8 but was indeed deliberately placed in the Inner Propylon.

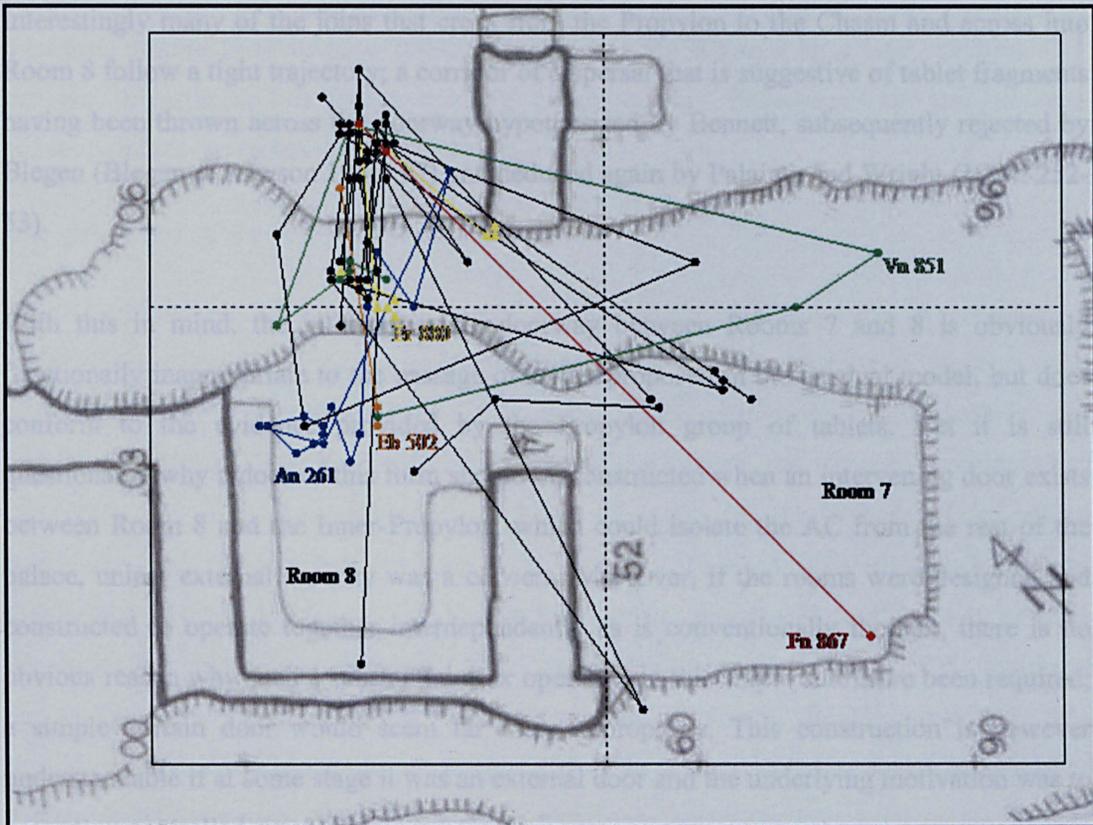


Figure II-5: The Inner Propylon tablet group interconnections.

The majority of the tablet joins link fragments recovered from the Inner Propylon and the Chasm, with only a small proportion showing connections to other areas. A few, such as An 261 and Er 880, may indeed have originated in Room 8 and have been subjected to the effects of both the destruction and the stone quarrying, whilst others such as Fn 867 appear to defy explanation. In the latter case, one fragment of the tablet was found beneath sherds of the broken pithos in Room 7 (see note 12) while the remainder were recovered from the tight

cluster of tablets in the Propylon area. Only two explanations are justified in this case; either the tablet was deposited in both areas already broken, or an error occurred in the recording of find-spots, a possibility that was mooted early in the study of tablet find-spots (Bennett 1964: 250). Both seem unlikely and only further detailed examination can provide the answer to such conundrums. Nevertheless, the majority of complete and fragmented tablets cluster tightly in a small area of the Inner Propylon revealing a discrete, coherent set deliberately placed in this location.

The mixed contents of the documents, which comprise examples from many of the major series of tablets including the Aa, An, Ea, Eb, Jn, Un and Vn series, indicate that this in all probability represents a 'final delivery' to the AC. Certainly there is no obvious common subject that could account for such a diverse range of tablets in one concentrated group, nor is there any apparent reasoning for this placement in an area with no specific function. Interestingly many of the joins that cross from the Propylon to the Chasm and across into Room 8 follow a tight trajectory; a corridor of dispersal that is suggestive of tablet fragments having been thrown across the doorway hypothesized by Bennett, subsequently rejected by Blegen (Blegen & Rawson 1966: 97) and deduced again by Palaima and Wright (1984: 252-53).

With this in mind, the interconnecting doorway between Rooms 7 and 8 is obviously functionally inappropriate to the passage of traffic proposed in the original model, but does conform to the evidence provided by the Propylon group of tablets. Yet it is still questionable why a door of this form should be constructed when an intervening door exists between Room 8 and the Inner Propylon, which could isolate the AC from the rest of the palace, unless external security was a concern. Moreover, if the rooms were designed and constructed to operate together interdependently as is conventionally thought, there is no obvious reason why such a substantial door operating in this way would have been required; a simple curtain door would seem far more appropriate. This construction is however understandable if at some stage it was an external door and the underlying motivation was to prevent uncontrolled external access.

I suggest, therefore, that together all of these factors point to Room 7 being a later addition to the palatial complex and that as a result, several important questions must be answered in relation to the reconstruction, function and operation of the AC. For example, does Room 8 indeed serve as an archive room and did it have any previous alternative function? Were the two rooms conceived as functioning interdependently as the palace archive, or did they have other functions obscured by the presence of the tablets? What is the nature of the activities

represented both by the tablets and the other artefacts found in the two rooms and how do the tablets from each area relate to each other? However, before these and other questions can be approached we need to examine the totality of the architectural reconstruction and the conventional model of operation and its validity.

Reconstructing the architecture and function of the Archives Complex

There can be little doubt that the original reconstruction was heavily influenced by the tablet finds themselves as well as the incomplete nature of the architecture. Subsequent analyses have largely followed this hypothetical reconstruction and have arguably been based upon a teleology: that the rooms functioned as an AC and so any reconstruction must conform to this reality. Unfortunately, the archaeological evidence is far from explicit. With little remaining of the walls of Room 7, the exact nature of the architecture is difficult to ascertain. So much so, that even the phasing of construction between Rooms 7 and 8 has been assumed, despite there being a strong case for viewing Room 7 as a later addition. One problem in particular has been created by the architectural deficiencies and that concerns access points to the rooms and their relative positions.

The doorways of the Archives Complex

As a detail fundamental to the derivation of operational models the problem of extrapolating the positions of access points has understandably been the subject of some debate. However, with only the interconnecting doorway preserved, all of the proposed reconstructions rely heavily upon interpreting the indirect archaeological evidence, the assumed functioning of the rooms and, in later modifications, with incorporating hypothesized models of scribal traffic and the AC operations (Palaima & Wright 1985; Palaima 1988: 171-89; Kyriakidis 1996-1997 [1998]: 213-19, 222-23). Yet the use of operational models is inherently dangerous in attempting to establish a likely architectural reconstruction since the argument can easily become circular; doors exist because the operational model dictates that they should, but that model is itself based upon the existence of particular doors. It would be preferable to determine, as far as possible, the likely reconstruction from archaeological evidence alone but, as will become apparent, much of the available evidence has either been neglected or simply not recognized. The many finds, excluding tablets, have, for example, rarely been examined in relation to the architecture and the potential for differential architectural phasing has never been considered.

The initial reconstruction, although positing a door from Room 7 to the Outer Portico, assumed that access to Room 8 was restricted to the interconnecting door, but as was

discussed above, despite discussing the possibility of a door connecting with the Inner Propylon, Blegen concluded that none existed (Blegen & Rawson 1966: 97). The preceding analysis clearly demonstrates that the restoration of this access point by Palaima and Wright (1985) was entirely justified, despite being reasoned on the basis of supposed architectural parallels within the palace and from a hypothesized mode of scribal traffic, rather than directly from the evidence surrounding the AC. Yet their analysis did not question the existence of the external doorway presumed to exist between Room 7 and the Outer Portico despite the tenuous archaeological evidence (Blegen & Rawson 1966: 93). The argument for its inclusion was predicated on the basis of two observations: the positioning of the 'sentry stand' adjacent to the Inner Propylon entranceway, and damage to the floor of the Outer Portico presumed to have occurred with the removal of large threshold blocks (*ibid*: 97). The latter is undoubtedly founded upon an incorrect assessment of the morphology of the Chasm both from an inaccurate understanding of the process of creation, but also in selectively privileging points of damage. As Palaima and Wright (1985: 252, n. 6) point out, such features are also present in the continuation of the trench through Room 8, but more importantly the ragged nature of the damage to adjacent stucco floors is replicated along the entire western face of the trench on the south-western façade of the Main Building.

It was, however, the positioning of the 'sentry stand' to the left of the Inner Propylon entranceway, apparently located on the 'wrong' side of the doorway and therefore logical only if it were guarding an external door to Room 7, that was seen as the strongest support for this reconstruction. Indeed this evidence was considered to be so strong that it was described as the single most important feature pointing to the existence of this door and that any attempt to question the validity of this reconstruction must address the position of this feature (*ibid*: 256). This, however, was founded upon the interpretation that they demarcate points for the positioning of sentries guarding the entrances to key points of the palace, an interpretation that has itself rarely been questioned. Can this interpretation and the label applied to them really be justified and, if not, what can be said of the nature of these features?

The 'sentry stands'

There are four so-called 'sentry stands' found in the palace; three along the main axis of approach to the central megaron and one in the entrance hall of the South-western Building. Contrary to the perception that these features are the same, table II-1, summarizing the published data, shows that in reality none exhibit precisely the same constructional characteristics. Indeed, two of these are not 'stands' at all, but sunken features (demonstrated

by the negative height measurements). Despite these discrepancies, the conventional interpretation has remained with surprisingly little dissent. The only notable exception being the suggestion by Mylonas (1966: 53-55), based upon Homeric references to the palaces inhabited by the heroes of Troy, that these were in fact spaces for the positioning of torch stands lighting the entranceways. This would seem equally as plausible a hypothesis and possibly more so than the notion of 'sentry stands'.

Room	Depth (m)	Width (m)	Height (m)	Associated artefacts
1	1.00	0.85 – 0.90	- 0.015 – 0.02	decorated, pedestalled krater?
4	0.90	0.85 – 0.92	0.08 – 0.09	
5	1.20	1.10	0.03 – 0.04	
64	1.12	1.02	- 0.05	decorated, pedestalled krater

Table II-1: The constructional details of the Pylian sentry stands (Blegen & Rawson 1966: 57, 62, 68, 74-5, 253).

The two innermost (Rooms 4 and 5) consist of platforms raised minimally above the surrounding floor surface, covering an area of ca. 1m². By contrast the outermost 'stand' (Room 1), adjacent to Room 7 and occupying a similar surface area, is actually a sunken feature described in the excavation report as being finished with stucco, with the final phase of pavement of the Propylon laid around it (Blegen & Rawson 1966: 57). These characteristics alone mark this architectural element out as different from the two other conjectured 'sentry stands'. The fact that a later pavement had been laid up to this point may suggest that the void originally contained some form of structure, architectural or not, against which the floor was subsequently laid. However, even the reported condition of the underlying stucco within the void, which has apparently suffered little damage, provides few indications as to what was originally positioned there, other than to show that nothing of any great mass could have occupied it. Despite a similar negative feature located outside of the entrance to the megaron of the Southwestern Building (Hall 64), functional interpretations remain problematic. There are no other architectural indicators as to their purpose, whether entirely functional or symbolic. The imagined function of positions for sentries, controlling access to various rooms is simply conjecture; an interpretation that panders more to a fantasy image of a palace and king employing a 'royal bodyguard' than to any archaeological evidence. Indeed, the architectural features of the palace argue against this interpretation. The only 'personal' space within the entire complex that merits architectural distinction is the throne in the main megaron. It seems implausible then to suppose that space for guards

would be delineated or highlighted in this fashion. Moreover, the form of these, either sunken or raised by around 0.01m would seem rather pointless for the positioning of guards and certainly this interpretation singularly fails to explain why there are two forms.

A possible clue to the function of the sunken features is provided by the broken remains of decorated, pedestalled kraters found in close proximity to both. Particularly noteworthy is the case of Hall 64 where the krater was found immediately adjacent to it, with the fragments extending across into the doorway of the megaron (*ibid*: 253, 400-01, fig. 387). Here the association of the vessel with the 'stand' appears to be incontrovertible whereas in the case of the 'stand' located in Room 1, a similar vessel was found lying next to a large group of tablets, but located immediately behind the intervening partition wall with the Inner Propylon (Blegen 1953: 63, fig. 15; Blegen & Rawson 1966: 62, 400-02, fig. 387). Although the physical association cannot be said to be as strong as for Hall 64, since both vessels are classified as having the same shape and were located close to virtually identical architectural features, it seems likely that they served a similar purpose. Significantly, these objects are found next to 'stands' located at the outermost entrance points to these buildings, suggesting that if the stands and kraters are indeed related then their combined purpose was connected to the primary points of access. Given all of these strands of evidence it is perhaps possible to conjecture that these features are the remnants of built plinths, presumably of wooden construction, on which the kraters were placed. To proceed from this is however simply speculation. We may imagine, for example, that the kraters were used as receptacles for offerings prior to an individual passing through into these buildings or perhaps, more likely, held water for ritual purification and cleansing prior to entry.

Importantly though, no such objects are recorded as being found next to the two raised platforms. Indeed, the two 'stands' of Rooms 4 and 5 are considerably more problematic. Both of these features are recorded as being positive features, raised above the level of the latest phase of stucco flooring. Yet, that of Room 4 would in fact appear to have had the latest phase of flooring overlying it, since the northeast edge of the stand 'at some time was for the most part covered by the stucco floor' (Blegen & Rawson 1966: 74). The stand in Room 5 is even more problematic. This clearly belongs to the final phase of palatial constructions but was executed carelessly and laid askew. Significantly, beneath this was the remnant of an earlier platform, which retained traces of a painted surface. In both cases, the platforms are raised only a minimal distance above the surrounding floor surface. The apparently carelessly executed construction would seem at variance with the attention paid to decorative detail and aesthetic appearance within these areas, particularly if these features were meant to be seen. This apparent carelessness in the laying of this stucco does suggest

that not only was the feature not actually meant to be seen, but also that something was in position hampering the work of laying the stucco.

Despite these observations, it is an unfortunate reality that without additional evidence we can in no way suggest what these features were.⁷ We may be faced with evidence of some architectural element, or simply a space reserved for some decorative or functional objects. The quality of the stucco does suggest a similar feature was in place on the two sunken features, but with no artefacts recovered nearby to suggest a purpose little more may be said. Nevertheless, the combined evidence from both types of 'sentry stands' shows that the original interpretation is almost certainly incorrect and, for the purposes of this analysis, that one of the two main factors for suggesting an external doorway to Room 7 is baseless. This, combined with the observations concerning the morphology of the Chasm in effect reduce the archaeological arguments for the reconstruction of the external door to Room 7 to nothing, but there is one further piece of evidence that confirms this door could not exist. The distribution of tablets in Room 7 (figure II-3), clearly shows that a substantial number of tablets lay across the span of the south-eastern wall of Room 7. These were interpreted as being present through scatter or spillage from Room 8 (Palaima & Wright 1985: 260; Palaima 1988: 183), but the coherency of find-spots contradicts this entirely. Several baskets of documents were evidently placed along this wall and were subsequently scattered by the collapse of the building and by the removal of building stone. The position of these groups prior to the destruction, though, would have directly blocked the conjectured door. This, combined with the issues surrounding the interpretation of the Chasm morphology and the sentry stands, demonstrates that no such door ever existed.

The significance of these observations cannot be overestimated since the operational models of scribal practice, tablet use and even relative tablet chronologies are dependant upon a model in which deliveries are made via Room 7 before eventually being passed to Room 8 for archiving. If, in the operational model, Room 7 cannot act as the delivery point for tablets, what can be said of the function of Room 8? This has been assumed to be the heart of the AC in which the various documents and dossiers were to be stored but, as we have

⁷ An interesting, and potentially crucial, observation in this respect was provided by John Bennet (pers. comm) in that there is some resemblance of these stands to the so-called 'altar' fronting the entrance of the North-East Building. The altar, consisting of a poros block measuring approximately 0.64 x 0.60m, is clearly somewhat smaller than the 'sentry stands' (Blegen & Rawson 1966: 302). Nevertheless, this feature, protruding irregularly from the surface of Court 92, was itself, like the stands, plastered. Intriguingly the plaster was painted with designs found in Hall 64 and the Inner Portico of the Propylon. In a not dissimilar location, fronting the entrance to a major complex, it is possible that this performed a similar function to the 'sentry stands'. Unfortunately, no artefacts are recorded directly in association with this feature. Nevertheless, the relationship between this feature and the 'sentry stands' is worthy of further investigation.

already seen, the evidence for differential phasing and the refurbishment of Room 8 indicates that the interpretation of this room may not be as simple as previously thought. In order to demonstrate this and assess the viability of current interpretations of the role of the AC, it is necessary to examine the central tenet of this model – the notion of tablet storage. In other words, does the pattern of tablet scatter reflect, as has been generally regarded, the storage of documents or are we looking at a snapshot of a more dynamic system in which a ‘temporal anomaly’ has created the illusion of storage, while the destructive events that overtook the palace have obscured a more active process which saw the tablets momentarily occupying these rooms?

Tablet storage?

Document storage or the provision for such activities is one aspect of the Pylos AC that has largely been assumed rather than proven. As with many such convictions constantly repeated and championed, this has reached the level of what can only be described as archaeological dogma. However, as with many such ‘factoids’ (Kilian 1988a: 135), based upon models of ‘common-sense’, a closer examination of the archaeological evidence reveals this ‘fact’ is in reality illusory. It is certainly a fact that the majority of tablets were recovered from Room 8, which is in no small part the reason for this room being interpreted as functioning as the archive room proper, yet the quantity of tablets involved is exceedingly large and the archaeological evidence for permanent storage facilities provided for is far from clear. Nevertheless, the quantity of tablets and their distribution pattern across the floors of the two rooms led the excavators, and subsequent analysts, to suppose that the tablets were originally stored on a combination of fixed and moveable furniture which were burnt away in the fire that engulfed the palace, scattering the tablets onto the floor below. In Room 8 this furniture was interpreted as consisting of fixed shelving arranged around the walls of the room; that in Room 7 as temporary shelving or benches on which the baskets of tablets were placed whilst awaiting processing (e.g. Blegen & Rawson 1966: 93, 96-98; Palaima & Wright 1985; Palaima 1988: 171-89; Pluta 1996-1997 [1998]).

These suggestions seem eminently sensible given the volume of tablets and their distribution pattern, but are we to suppose that this quantity could have been stored on shelving that was itself not supported? Indeed, if we are to presume that Room 8 was intended for the long-term (if annually based) storage of records, then we might expect a more permanent and organized solution to archiving. Judging from the evidence of other workshop and storage areas within the palace, it is clear that where necessary the organisation and provision of fixed storage solutions was undertaken. Rooms 19 and 20 for example, where similar

shelving has been inferred, and in particular Room 19, where a great mass of pottery was stored bear witness to a well-constructed shelving system with post-holes sunk into the floor to accept vertical supports. In Room 21 a different method appears to have been employed with stone pads placed on the floor to support shelving uprights, but no such features were found in the AC (Blegen & Rawson 1966: 97).

Such supports would have been necessary given the mass of the tablets and although it is a remote possibility that they simply rested on the floor surface and that the shelving system was essentially free standing, the evidence from other rooms suggest that the builders were fully aware of the engineering requirements. However, in the case of Room 8, there are three other fundamental hindrances to fixed shelving that common-sense has seemingly ignored. The first is simply that with the bench that surrounds three walls standing 0.3 to 0.4m high, for anyone to use the bench comfortably shelving would have had to have been positioned at a considerable height above. Furthermore, with the connecting doorway between Rooms 7 and 8 and that leading from Room 8 to the Inner Propylon, there is little clear wall space available. The physical limitations imposed by these features and the general spatial area involved clearly militate against the provision of fixed shelving for such a large volume of tablets. Moreover, despite the evidence presented for an ongoing process of refurbishment, the opportunity was not taken to install supporting timbers if the room was viewed as a document storage area. Yet even in those areas where it is conceivable that shelving could have been fitted, no such provision has been made. Architecturally at least, the room displays no evidence that it had a storage function either practically or conceptually.

A closer examination of the tablet distribution within Room 8 confirms this, although a simple examination of the overall distribution is somewhat misleading (figure II-3). The distribution of all the major tablets and fragments, at first glance, appears extremely confused although certain general features are readily apparent, in particular the difference in the scatter pattern between the two rooms. In Room 8 tablets are seemingly in considerable disarray and while certain groups, such as the Aa series, possess an integrity indicative of close association and relatively little disruption, the overall pattern is chaotic. It appears that the effects of the destruction allowed for considerable mixing of tablet sets, but more importantly the distance over which adjoining fragments appear to have travelled superficially supports the notion that some tablets were stored on shelving surrounding the walls. Furthermore, the distribution apparently shows a high proportion of tablets close to the walls with a patchier arrangement towards the centre, suggestive of scatter away from the walls as shelving collapsed. Given this pattern, it is unsurprising that Room 8 was, and continues to be, considered a storage facility.

This interpretation is, in reality, entirely subjective and considering the totality of the tablet fragment distribution actually disguises the key information of the focal points from which tablet scatter occurred. In other words, the chaotic pattern obscures the positions closest to the points at which tablets were originally grouped. Unfortunately the sheer volume of fragments present as well as the lack of any relative height data for each find-spot makes the determination of precise points of placement difficult. In addition, there is a danger in assuming that certain clusters of tablets were collected and stored together and that such interpretations of the contents of dossiers must define storage patterns and, by extension, the locations of these dossiers. Nevertheless, the problem of locating the points from which tablet scatter derived is paramount in understanding and interpreting the distribution pattern.

With the available excavation data and the limits imposed by the destruction, it is not possible to reconstruct these locations precisely but it is, I suggest, possible to approximate these locations by looking instead at the distribution in terms of the density of the fallen tablets. Specifically, we need to examine the density of tablets that were recovered essentially complete, and have only a single find-spot associated with them, as it is those that will define more precisely the points at which tablet groups fell. We should then expect to see the greatest density of complete tablets at the points where baskets or boxes of tablets were originally placed. A diminishing density away from these points would be indicative of the scatter produced by the destruction of the complex. Figure II-6 clearly shows that the foci of these are in fact very much more centrally located within Room 8, not as we might expect close to the walls where the distribution shows only low density accumulations around the periphery of the room.

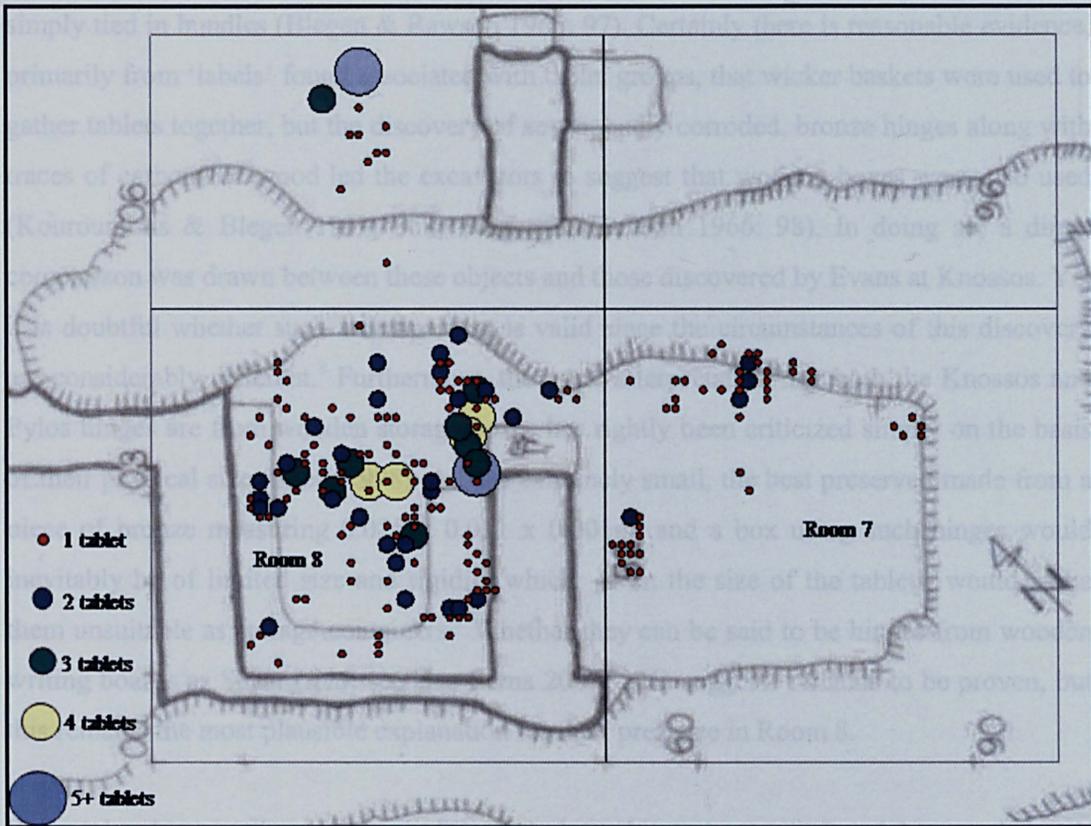


Figure II-6: A density plot of complete tablets in the Archives Complex.

This can only be considered an approximation since we cannot be certain how the tablets were broken and dispersed. A great many will simply have fragmented after falling from whatever furniture they had been placed upon, but the breakage and dispersal of others is likely to have been affected by the collapse of the room structure itself. Nevertheless, by considering only those tablets which were left largely intact, we are inevitably considering those that had travelled the least. If we consider the evidence of this as well as the overall distribution, it seems more likely that the majority of tablets were collected in discrete groups either placed upon free-standing furniture in the centre of the room or temporarily upon the clay bench. That they were placed upon furniture, rather than simply on the floor, appears to be the most likely scenario given that the degree of displacement apparent in both diagrams indicates that the tablets probably fell from some height. This would also explain why, where document sets have been identified with any degree of certainty (such as the Aa subset), the separation between individual tablets and tablet fragments is not always as great as we might expect if they had tipped from shelves mounted high upon the surrounding walls.

Whilst this re-appraisal of tablet storage shows that the notion of permanent storage is flawed, it does not expose the manner in which tablets were grouped together; whether they were contained within open baskets, wooden boxes, or indeed were not contained at all, but

simply tied in bundles (Blegen & Rawson 1966: 97). Certainly there is reasonable evidence, primarily from 'labels' found associated with tablet groups, that wicker baskets were used to gather tablets together, but the discovery of seven, badly corroded, bronze hinges along with traces of carbonized wood led the excavators to suggest that wooden boxes were also used (Kourouniotis & Blegen 1939: 569; Blegen & Rawson 1966: 98). In doing so, a direct comparison was drawn between these objects and those discovered by Evans at Knossos. Yet it is doubtful whether such a comparison is valid since the circumstances of this discovery are considerably different.⁸ Furthermore, the basic interpretation that both the Knossos and Pylos hinges are from wooden storage boxes has rightly been criticized simply on the basis of their physical size (Shear 1998). All are extremely small, the best preserved made from a piece of bronze measuring 0.025 x 0.011 x 0.001m, and a box using such hinges would inevitably be of limited size and rigidity which, given the size of the tablets, would make them unsuitable as storage containers.⁹ Whether they can be said to be hinges from wooden writing boards as Shear (*ibid*; see also Perna 2007: 226) suggests remains to be proven, but this remains the most plausible explanation for their presence in Room 8.

We might also question why no substantial charred remains were found in association if indeed the hinges were attached to wooden boxes. There is no indication from the excavation reports that the charred remains were anything other than extremely localized and insubstantial and there is therefore no clear evidence that wooden boxes were used as tablet containers. Moreover, Wa 569, thought to have been pressed onto the flat surface of a wooden box cannot be used as justification for this claim either (e.g. Palaima 2003: 181). In fact this label may indicate that the writing tablet hypothesis has some definite credence to it. Inscribed with *to-ra* (thorax, 'breastplate'), it appears to have a thematic association with the Sh series found in Room 7, yet these form a coherent group apparently labelled by Wa 732. No other tablets in Room 8 appear to contain subject matter pertinent to Wa 569. However, if the hinges are indeed remnants of wooden writing tablets, then it is possible that they contained information that has a bearing on this label, simultaneously explaining why Wa 569 is unique in having been pressed onto a flat surface. Indeed, this and the apparently close physical proximity of all the hinges does suggest a plausible scenario of several wooden diptychs having been tied together and labelled with Wa 569. However, this, although

⁸ There, Evans (1909: 41-43; 1935: 668) described how at least four wooden boxes, represented by the carbonized remains of the wood and seven bronze hinges, were found along with a substantial number of tablets in a 'closet beneath a small staircase'. However the hinges or the charcoal are interpreted, the tablets are clearly not part of an archive. They have evidently been packed away but the purpose of this is unclear and, although these objects appear to be in direct association, there is no direct confirmation that, even if the hinges are representative of storage boxes, it was the tablets that were contained within.

⁹ All dimensions quoted are maximum (Shear 1998: 188).

obviously not attached to a wicker basket, may have no relationship with these hinges but may simply have been attached to another medium, such as leather, as has been tentatively suggested with respect to a sealing from Thebes (Aravantinos 1990: 151, n. 10).

The precise means employed in containing discrete tablet groups remains unclear, as does the nature of any moveable furniture within the rooms but it is clear that whatever shelving, if any, existed around the walls was limited in scope and incapable of providing storage for all of the tablets. The vast majority of tablets in Room 8 were placed in groups either directly on the bench or on furniture in the centre of the room and as such the notion of protracted, long-term or permanent storage cannot be supported. This pattern is repeated in Room 7 although here the distribution pattern is much clearer, showing a greater degree of integrity between groups of tablets but, with the possible exception of the tablets located along the south-eastern wall, these were also evidently placed either upon furniture or directly on the floor. Although it has been suggested that shelving existed along this wall, the alternative suggestion of placement on a table would seem more appropriate given the tight clustering of tablet fragments (Pluta 1996-1997 [1998]: 242). As I observed earlier with regard to Room 8, tablets falling from shelving at any great height, whether contained in baskets or not, are likely to disperse far more than is apparent in the distribution plots.

These rooms were clearly not designed or engineered for permanent document storage, nor is there any evidence to indicate that the presence of the tablets was anything other than temporary. We are therefore faced with the inevitable conclusion that Room 8 does not, and was never intended to, function as a document storage area. With problems surrounding the accepted interpretations of both Rooms 7 and 8, there is consequently a much greater question of whether the accepted function of the AC as a whole is justifiable.

The finds from the Archives Complex

Apart from the many hundreds of tablets found, a significant group of other artefacts were recovered, predominantly from Room 7. These represent an eclectic range of objects including some that, superficially, appear to have a strictly utilitarian function such as the large, ribbed pithos, with others that have attracted more complex, votive interpretations as with the collection of diminutive kylikes and animal bones. Indeed, the confusion surrounding the composition of this assemblage led Blegen (1953: 63) initially to propose that Room 7 as an annex to the archives room proper was a form of shrine. This interpretation was subsequently rejected with the publication of the first volume of the excavation report in favour of one that saw Room 7 as 'probably the office of the tax

collector' (Blegen & Rawson 1966: 92), thus defining the rooms as functionally inter-dependant. With this interpretive framework established, explanations concerning these additional artefacts ultimately proved problematic and, I suggest, were generally interpreted not through recourse to their character, associations and archaeological context but from the supposed purpose of Room 7 itself and a basic perception of how the administrative system should operate. Those objects that could not obviously fit within this framework were, therefore, assumed to be essentially misplaced from their primary context of use and were not functionally related either to the context of deposition or to the tablets that defined the nature of this context. However, the preceding discussions have cast considerable doubt upon the integrity of current functional models and it is therefore necessary to examine the range of recovered artefacts free from the basic assumption that these rooms are an archive, whether they are working or storage in nature, and that explanations for these objects must be sought in relation to such an environment.

The pithos

Of all the remaining objects in Room 7, it is the large, ribbed pithos that once stood in the southern corner that has seemingly required the least explanation. Virtually unquestioningly this has been interpreted an object whose placement is directly related to the functioning of, and the actions of the people using that room. In this way, the pithos has been variously interpreted as being used to contain olive oil delivered to the palace (Blegen & Rawson 1966: 92), or water that was of practical value in the production of tablets (Pluta 1996-1997 [1998]: 246). Yet even without the scientific analyses desired by Pluta (*ibid*) which would, in any case, not remove the possibility of the pithos having been utilized in a variety of ways within these rooms, there is in fact little evidence to support these interpretations and every reason to consider them entirely conjectural. No trace remains of charred organic materials, clay or the oily residues apparent with the pithoi of the Oil Magazines were found, and the only clue to suggest that it may have contained water is the variable pattern of burning apparent on the pithos fragments. With only this tenuous observation, the evidence indicates that whilst it may have contained a measure of water, in all probability the vessel was indeed empty.

This argument aside, the majority of interpretations ignore the most obvious feature of this vessel; that in its complete form it would have stood 1.64m high and over a metre in diameter, excluding the lid that was also recovered (Blegen & Rawson 1966: 394), making it entirely impractical as a utilitarian vessel. Furthermore, this vessel is unique within the

palace both in form and capacity, being the largest vessel represented by far.¹⁰ Certainly the volume of water that could be contained would seem to be greatly in excess of any need by scribes producing tablets (contra Pluta 1996-1997 [1998]: 240, 246), particularly when no evidence for the mass production of tablets at this or indeed any other location exists. More generally, with the neck of the pithos close to head height, the filling of, or access to the contents of, the vessel would undoubtedly be problematic and emptying it extremely difficult in the confined space of the room. Also, if we are to consider that the pithos was intended for permanent use, it is surprising that a more functionally convenient and permanent solution was not employed.

A simple calculation based upon the maximum body diameter of the pithos (1.075m) reveals that the minimum area this would have encompassed would have been approximately 0.91 m², which in a space approximately 4 m² would be equivalent to around 22 to 23% of the entire surface area of the room. If the interpretation of Room 7 as a scribal working area has any validity, which I suggest that it does not, and in an area where space is at a premium, this pithos has to be considered an inconceivable extravagance if the only purpose it served was as a water container. Where such vessels are in constant use, or are functionally necessary for the activities of that room, the solution generally employed was to build the vessels into clay benches at a height that provided easy access, as in Rooms 23 and 24 of the Oil Magazines, and in Rooms 10 and 43. No such facility was provided for here or, based upon the evidence of recent construction discussed above, was ever intended to be.

I suggest then that current interpretations of the role of this object are simply assertions based upon no more than its presence in what has been assumed to be the working area of an AC and that there has been an implicit assumption that its presence is representative of a primary functional context. Yet with little or no evidence that the pithos contained anything when it was deposited in the room, there is no obvious functional reason for its presence and no reason to suppose that Room 7 represents anything more than a temporary storage area. It would also seem inconceivable given the spatial restrictions of the area that it could have been in storage unless extremely temporarily and even then, with every indication that both rooms were decoratively incomplete, why should such a manoeuvre to place it in this position be attempted in the first place, when in all likelihood it would have to be removed again to allow completion of the decoration?

¹⁰ It has also been noted by Stocker & Davis (2004: 70) that the fabric of this vessel more closely resembles pottery from much earlier phases of occupation. If this is the case, the close spatial association with other more archaic artefacts (discussed below) would suggest that this object also possesses a ritual component in its use, further emphasizing the inappropriateness of its positioning in Room 7.

Standard architectural reconstructions make the practicalities of the precise placement extremely problematic, as the only means of entry would be through an external doorway to Room 7, or via the Inner Propylon and the entrance to Room 8. However, the conventionally reconstructed doorway to Room 7 did not exist and without some other external access, this would have necessitated a manoeuvre through Room 8 and the various obstacles that would have entailed. While the vessel dimensions do not necessarily preclude passage through the relatively narrow doors connecting the rooms, it would nevertheless have been a difficult manoeuvre to perform for no obvious benefit.¹¹ Even then, it is difficult to explain the location of the pithos at the farthest point away from the partition doorway. If indeed this was the entry route, why was the pithos simply not placed in the opposite corner to the interconnecting door? In all respects, the presence, location and type of vessel are inappropriate for Room 7 and explanations forwarded on the basis of an inferred function within a working archive do not explain these incongruities. Surprisingly this object has never been considered in relation to other groups of artefacts that appear to have no direct relationship either to the tablets or to the AC itself. Yet there is a curious spatial arrangement of objects across the span of the south-west side of Room 7, of which the pithos is but one object found in close proximity with at least three significant groups of artefacts. One consists of a small cache of fragmentary tablets found beneath the broken remains of the pithos, but also found nearby were a large pile of animal bones and a collection of broken, diminutive kylikes.¹²

Ritual feasting debris? Faunal remains and kylikes

Immediately adjacent to the pithos lay a large assemblage of burnt faunal remains that has only recently been the subject of detailed analysis (e.g. Isaakidou *et al.* 2002; Halstead & Isaakidou 2004; Stocker & Davis 2004). The assemblage is dominated by cattle bones, specifically the humerus, femur and mandible, many of which bear butchery marks indicative of processes of dismemberment and filleting. Combined with the selective representation of body parts suggestive of both nearby slaughter and an obvious preference

¹¹ Unfortunately, with only one of the threshold blocks surviving, the precise width of the interconnecting doorway between Rooms 7 and 8 is unknown, but it is not inconceivable that it was sufficiently wide to accommodate the passage of the pithos. However, if the doorway was indeed narrower than the maximum diameter of the pithos, then either Room 7 was constructed around this vessel, or it demonstrates conclusively my conclusion below that Room 7 must have possessed an open façade.

¹² The position of the tablet fragments beneath the pithos is especially significant, suggesting that not only must they have remained more-or-less *in situ* during the destruction of the palace, but also in all likelihood they had simply been placed on the floor rather than on any furniture. Confirmation that the tablet fragments lay beneath the pithos has been provided by entries in Blegen's notebooks (Stocker & Davis 2004: 64).

for the major meat-bearing bones it seems most likely that their primary role derives from the provision of large-scale feasting or banqueting events (Isaakidou *et al.* 2002; Halstead & Isaakidou 2004: 145-47). Although dominated by cattle, the species representation is mixed and includes a limited quantity of pig and sheep/goat bones (unburnt) and possibly red deer (burnt), with all of the examples of pig and sheep/goat apparently recovered from the western side of the Chasm along with, possibly, all of the unburnt cattle bones. This mixing has been interpreted as being the result of the disturbance from the creation of the Chasm, with the identification of joining fragments of burnt bone from the Room 7 deposit and from the Chasm providing evidence of this (Halstead & Isaakidou 2004: 143-44, table 7.1).

This scenario is undoubtedly correct, but it nevertheless points to the positioning of the bone pile extremely close to the western wall of Room 7 for it to have been disturbed in this fashion. Even then, the profile of the Chasm against the preserved internal face of the Room 7 floor is such that it appears unlikely that the bones themselves, buried beneath an overburden of destruction deposits and later soil accumulations, would have been greatly interfered with (figure II-2). This is in direct contrast to the eastern side of the Chasm which clearly encroaches upon the internal floor surface and resulted in significant disturbance of the tablet groups along that side and their subsequent incorporation in the Chasm deposits. I suggest therefore that the questions of how the bones entered the Chasm, how the bone pile was positioned in relation to the architecture of Room 7 and, why they were placed here at all, still remain.

Whatever event prompted the gathering of these faunal remains, it is likely that their deposition in this context was not far removed temporally from that event (Halstead & Isaakidou 2004: 146), but conversely it seems unlikely that they would deliberately have been moved to and kept in an enclosed room of the palace for any great length of time. We therefore have to explain why such a multitude of bones was apparently placed in the most inaccessible point of the room, particularly when also considering the number of obstacles between the doorways and the point of deposition, where they would have only lain for a very short period. Why indeed were they deposited in Room 7 at all? The suggestion by Bendall (2004: 120-22) that these bones may well be related to feasting activities in Court 58 is eminently plausible, but this does not in itself explain their eventual placement in the AC. If retention was necessary, why, for example, were they not placed temporarily in Room 60 outside of the Main Building, or indeed in the North-East Building area?

Only one scenario would appear to answer these questions whilst simultaneously explaining the singular characteristics of this group of bones. However, before attempting to answer the

question of placement, one further group of objects that also suggested some form of ritual element to the deposition of material in Room 7 requires consideration and that is the set of diminutive kylikes. These miniature vessels were found in close proximity to the faunal remains in the western half of the room, close to the north-western wall. This apparent physical association and the character of the vessels, which not only allows for only a very small volume of liquid content, but is also awkward for practical use, seems to preclude a utilitarian function, leading to the suggestion that they could either be 'toys' for children or were votive in nature (Blegen & Rawson 1966: 93, 266). The first of these interpretations can almost certainly be dismissed simply based upon the numbers recovered from this room and from the rest of the palace. The second suggestion presents more plausible possibilities, but again the numbers involved suggest that if a ritual element is involved, then it is not to the vessel itself that we should look, but rather to their context of use. However, some caution is required in assessing the significance of these vessels both from the AC and the site as a whole, based solely on the reported numbers. For while the report details 11-12 examples found in Room 7, subsequent examination of the archived material identified further fragmentary kylikes giving a revised total of 20 to 22 vessels (Stocker & Davis 2004: 67-68, n. 28-29). Yet this group, whilst significant in number, does not represent the greatest single assemblage found in the palace, with the reported inventory showing three other areas with large concentrations.

Compared with the quantities of other vessels, the diminutive kylix is undoubtedly a rare form but the total recovered is nonetheless significant. 163 examples are counted in the final report (Blegen & Rawson 1966: 366), but as was noted above with respect to Room 7, this figure is likely to be a considerable underestimation. Of these, approximately 79% are associated with pottery storage areas, dominated by the assemblage of more than 80 from Room 60, the others coming from Room 7 itself, the main Megaron, Room 24 of the Oil Magazines and finally two from a drain. The numbers and locations do not appear to support either of the original interpretations since the locations outside of the storage areas seems to imply a definite functional use. Unfortunately without detailed study of this vessel type interpretations are extremely difficult. The reported range of capacities from 0.035 l to 0.009/0.010 l may well be significant in this respect, but as yet there is no statistical basis upon which to determine whether any definite pattern exists. Nevertheless, with the data that we do possess, the most likely interpretation is that proposed by Wright (1995: 302; also, Stocker & Davis 2004: 70): that these were used in a ceremonial context to contain a libation or ritual offering of wine. Certainly the numbers involved represent a significant proportion of the total number of these vessels found in the palace, far more than could reasonably be expected to have a practical use within Room 7. There would seem to be little doubt then

that their origin, like the faunal remains, was a feasting or banqueting event most likely held in some or all of the venues suggested by Bendall (2004). Whether, however, their numbers can be directly attributable to the attendees, or to the inventories of the Ta series found in Room 7, is more difficult to assess (cf. Palaima 2000; 2004b: 112-16; Halstead & Isaakidou 2004: 148-49; Stocker & Davis 2004: 70-73). Nonetheless, these not only reinforce the ritual element suggested by the bones, but also imply that a major event involving many venues and considerable numbers of people took place immediately prior to the destruction of the palace.

Additional ceramic finds from Room 7 appear to be consistent with, and reinforce, the theme presented by the material discussed thus far. Amongst the fragmentary remains are plain kylikes, bowls, cups, and small stirrup jars (Blegen & Rawson 1966: 95; Stocker & Davis 2004: 68, n.28), but unfortunately their precise context of discovery is unclear. Significantly no ceramics were recovered directly from the floor of Room 8, only from the overlying burnt deposits. The contrast between the rooms in this respect does at least suggest that these fragmentary remains are deliberate inclusions with a similar underlying motivation to the deposition of the diminutive kylikes. Whether or not this can be shown to be the case, Stocker & Davis (2004: 72) have rightly asked why there should be artefacts apparently not related to the Linear B tablets and scribal practices in Room 7 at all. Paradoxically, in posing this question, the pithos, assumed to be integral to the activities of the room, is excluded from consideration. However, I suggest that this object too needs to be considered alongside them and perhaps, in what could be regarded as a counter-intuitive statement and one that I shall address in subsequent chapters, that the tablets themselves should be considered in a similar vein.

These issues are undoubtedly brought together by one of the final questions posed by Stocker and Davis (*ibid.*: 73): whether these objects had only recently been delivered to Room 7. The tentative suggestion that this somehow represented a physical proof or audit that specific rituals and ceremonies had been carried out does not appear to satisfy their original question, nor explain the relative positioning of the objects within the room. Why, for example, were the kylikes not simply returned to the pantry from which they originated and why place the bones in Room 7 at all? Moreover, why would an audit be necessary after the event, when the objects would have been seen to have been used and presumably counted out in the first instance? There is, I suggest, no foundation or logic to these hypotheses and it is simply the presence of, and the primacy given to, the tablets that obscures the rationale behind these depositions.

Weapons, ceremony and sacrifice

The objects discussed thus far are undeniably the most significant groups of objects recovered from the AC and the Chasm, but a number of other artefacts were recovered from all of these contexts (Blegen & Rawson 1966: 94-95, 98-100). Many are too fragmentary to be recognizable and of worth in this analysis, but amongst them are several bronze objects of importance. The bronze hinges found in Room 8 have already been mentioned, but several other items were also found in Room 7. Two fragments of what appear to be blades were recovered but, importantly, so too were a sword and a spearhead, located on the floor amongst the cache of tablets close to the interconnecting doorway. Like the pithos, these are unique amongst the objects recovered from the palace excavations. They have no direct parallels and only two other possible sword fragments were found in Rooms 10 and 18 (Hofstra 2000: 100-01). Furthermore, not only were these the only weapons found here, but they are of a surprisingly archaic form (*ibid*: 98-100).

This antiquity does, however, suggest three possible reasons for their presence. The first and least likely, since there are no other obvious personal effects in the rooms, is that these are personal belongings. The archaic and exclusive nature of the objects and the lack of other identifiable personal objects in these rooms would seem to preclude this as a viable interpretation. However, given the contextual association with objects that point to a feasting context, two more probable explanations present themselves. Again, the less likely of these, is that they were intended as gifts to be presented at the feast. Their position, though, suggests that they had been returned to the room rather than placed awaiting presentation. Furthermore, if there is indeed an association between all of the artefacts, the deposition of the bones would indicate the completion of this event. Yet the key, I suggest, does lie in the strong associations of all of the artefacts with a major feasting event and the group of tablets, including Un 718, that appear to represent the provisioning of such a feast.

Un 718, laying amongst documents of the Ta series, details a series of contributions to Poseidon, presumably for a feast, that are to be given into the charge of a group known as the ‘sheep-flayers’, described by Palaima (2004b: 103; also Aura Jorro 1993: 58) as ‘some kind of sacrificial agents’.¹³ The key element here is the work of the sheep-flayers and the ceremonial nature of the event being prepared. In such a situation, the slaughtering of the animals would undoubtedly have involved considerable ritual and ceremony and it is entirely

¹³ Un 718 is one of the clearly identifiable prospective texts and it is tempting to see in this the replication of the event at Pylos that led to the deposits in the AC. Is it then possible that Un 718 was drawn up, in the context of the feast at the Palace of Nestor, in advance of another such event to be held at *sa-ra-pe-da*?

conceivable that in such a situation the tools used to despatch each animal were also imbued with ritual significance. It is entirely possible then that these weapons, which evidently possess great significance from their own antiquity, were the tools of such activities. It is further possible that such ceremony would have required different methods and tools of despatch dependent on the animals involved; as Stocker and Davis (2004: 70) observed, the spearhead may have a symbolic relationship with the deer remains and the association with hunting. I suggest then that the sword and the spearhead were for ceremonial use in the sacrifice or despatch of animals at a feast held at the palace and that, with the completion of the sacrifice, they were returned to Room 7.

Redefining the architecture and operation of the Archives Complex

The range and quantity of artefacts recovered from the AC is surprisingly large but at the same time is more or less restricted to Room 7. Although a significant quantity of material was also recovered from the fill of the Chasm, the provenance of this material is largely unclear. Yet even from this context, the ceramic finds, reported to include 60 kylix stems and 5 or 6 votive kylikes, continue the distinctive thematic association suggested by the objects from Room 7 (Blegen & Rawson 1966: 100). There can be little doubt that the common motif to virtually all of the artefacts is one of ceremony and feasting or banqueting, but it is one that is seemingly at variance with the dominant feature of the two rooms – the Linear B tablets themselves. The precise nature of the feasting activities that led to this accumulation is beyond the scope of this thesis, but the strong associations between all of the artefacts cannot be denied. If we simply accept the current reconstructions of the AC as the central hub of palatial administration, then we inevitably maintain the underlying problem of why any of this material should be present at all, particularly when there are many more suitable areas for deposition. There is clearly no reason to disassociate any of these objects from each other or from the underlying theme of feasting that appears to define not only their function but also their placement in the AC. The implications of this are profound for it suggests a common point of origin and a broadly shared timing to their deposition, which also has significant implications for our understanding of the architecture of the AC.

In the preceding discussion I have presented a considerable amount of evidence and argument that reveals many weaknesses in the standard interpretations of the architecture, role and operation of the AC at Pylos. In particular, I have identified probable differential building phases between the rooms, architectural reconstruction errors in relation to the access to Room 7 and operational inconsistencies in both rooms. Basic functions that have been assumed for the AC are not supported by the archaeological evidence and a closer

examination of the recovered artefacts and their distribution not only supports these criticisms but also suggests further means by which all of these factors may be understood. Firstly we must recognize that the Chasm creates problems not only in defining the architectural phases but also in understanding the nature of the superstructure. All we can deduce from the robbing activities is that foundation stones were removed from the entire length of the disturbance. However, there is no indication of how the foundation courses may have varied in construction or how such variations may have been reflected in the superstructure. In the case of the AC, there is in fact no architectural evidence to indicate that both rooms were fully enclosed and the problematic search for external doorways to Room 7 only serves to emphasize this. The bench provides us with our only firm indication of any enclosing walls and the remnants of the partition wall our only indication of the typology of construction. Crucially, such observations can only apply to Room 8; none of these clues can with any certainty be applied to a reconstruction of Room 7. There are, fortunately, additional clues to the architectural form provided by the nature and distribution of the recovered artefacts, in addition to the arrangement of tablets and the interconnections of adjoining fragments.

Considering the relative position of each of the material groups, in particular the faunal remains, pithos and kylikes, and understanding how they came to hold these positions is critical to a correct assessment of any architectural reconstruction. With the current reconstruction and operational understanding, this arrangement would seem an impossibility to achieve as well as to comprehend. All of these objects would have had to be manoeuvred around an already crowded AC for an unknown but evidently transitory purpose and one considerably more problematic to perform when it is not clear what external access existed to Room 7. The apparently deliberate placement at the least accessible point is entirely illogical unless the basic assumption as to the nature of the construction of Room 7 is fundamentally flawed. The most likely scenario is that another external doorway providing access to Room 7 did exist or, at least a part of the superstructure of Room 7 was open to the outside air (J. Bennet, pers. comm). Such access could not have existed along the north-eastern wall, as has already been discussed and the south-eastern wall also seems unlikely. However, the distribution of the major artefact groups and the apparent connection between these objects and the activities occurring in the outer courts does suggest that a point of access existed along the south-western wall.

With the scale of architectural destruction, structural evidence for such a reconstruction is lacking, but conversely there is no architectural objection to this scenario. Moreover, the reconstruction of Room 12 does provide a precedent in that here too the south-western wall

was removed down to the bedding layer in the same process that created the Chasm. Blegen restored a wide portal to this outer façade, suggesting that it may have contained a single column supporting the overhead lintel but crucially also admitting that ‘...the shape and width of this gap gave *no evidence for or against our conjecture* that there was an opening through the southwestern wall...’ (Blegen & Rawson 1966: 108, my emphasis). The situation with Room 7 would seem little different to Room 12 except that here there is at least indirect archaeological evidence to support the notion of an open or colonnaded façade, particularly with respect to the distribution of artefacts. Not only is a degree of confirmation provided by the pithos, which is so large as to be problematic to manoeuvre into the room in the conventional reconstruction, as well as being a completely impractical object for use there, but also by the faunal remains and kylikes. Why, after all, should such material be apparently retained in one of the rooms of the palace? As was observed earlier, the ongoing analysis of the bones has clearly revealed traces of butchery and filleting indicating that the meat had been stripped from the bones prior to their movement into this room.

In this scenario the placement of the bones, which would have presented an unpleasant problem if retained for any great length of time, is understandable as future access would be simple and an open façade would negate the potential problem created by the onset of decay. From here, they could easily be transferred to the arena in which they would finally be deposited. Similarly, the retention of the kylikes, whether they were broken or intact at the time of deposition, would also appear to be a part of this ongoing process of ritual activity. There can be little doubt that their role was particular, and probably ceremonial in nature, and it is entirely likely that it would have been necessary to remove these objects from potential future use and that, furthermore, they too would be incorporated in a later ritual deposition. In this way, the selection of Room 7, rather than for example Room 60, for the temporary storage of material is eminently sensible. Here there is no possibility for the kylikes to be accidentally re-used or mixed with unused vessels, whilst simultaneously maintaining access to the pantry itself. Whether such a process is reflected in the ‘standard’ vessels that are present is another question, but it is indeed possible that they too formed part of the ceremony surrounding the diminutive kylikes and as such were selectively removed to prevent further use. Whilst no such motivation can be envisaged for the pithos, with an open façade, Room 7 would be the most practical solution for the temporary storage of a vessel of this size, and the positioning in the corner of the room may indicate that further access was not as immediate a requirement as it was for the other objects.

By accepting this admittedly dramatic change in the architectural reconstruction of Room 7, all of the questions that have been advanced thus far are answerable. In essence the east to

west distribution of artefacts here can be seen to correspond to an identifiable sequence of events, beginning with the deposition of tablets at the eastern side and the placement of the bronzes and tablets next to the doorway to Room 8. As I shall argue in the next chapter, the deposits of tablets in Room 7 do not represent deliveries of documents and dossiers to be sorted and filed, but are directly involved in the activities occurring in the courts with respect to the feasting and ceremonial events represented by the other artefacts. As such, there are multiple deliveries of tablet groups accounting for the curious placement of discrete clusters of tablets both in the centre and the periphery of the room. The final act, I suggest, is the removal from the various feasting arenas of the pithos, animal bones and kylikes, which were placed just over the threshold of the room. There is every reason to suppose then that Room 7 was the scene of a constant to and fro of personnel and equipment servicing a banqueting event at a time very close to the final destruction of the palace.

However persuasive current interpretations of the role of these rooms superficially appear to be, a closer examination of the entirety of the archaeological evidence shows these to be illusory. Room 7 for example, has recently been argued to represent the working office of scribes, revising, organizing and even making tablets where necessary (e.g. Palaima & Wright 1985; Palaima 1988; Pluta 1996-1997 [1998]), essentially acting as the 'half-way house' between deliveries and storage; a scenario that has found broad acceptance across the scholarly community (e.g. Bennet 2001; Palaima 1988; 2003; Stocker & Davis 2004). There is however, no supporting evidence for this as a working area and none of the additional artefacts appear to have a functional relationship with the room itself or with scribal activities. Likewise, the generally accepted interpretation of Room 8 as a storage and consultation arena finds little corroboration. There is no evidence of storage facilities or of the long-term placement or accumulation of tablets. On the contrary, tablets appear to have been placed temporarily on furniture more centrally located in the room or on the bench itself. Neither is there any evidence that the bench can be associated functionally with the tablets and it seems far more likely that the refurbishment of this room, including the bench, was related to the wider changes that this wing of the Main Building was undergoing. With the evidence for direct access via the Inner Propylon and the tablets placed there, it seems likely that we are faced with Room 8 as a multi-functional space; one that could serve as waiting area adjacent to the Propylon, or when necessary could be combined with the added Room 7 to serve as an intermediary zone between the Main Building and the outer courts. Room 7 similarly appears to have a multi-functional purpose and would appear to serve both of the outer courts but there is unfortunately no evidence for any particular function other than that presented by the accumulation of objects in the last days of the palace.

The argument presented here is undoubtedly controversial but in essence is relatively simple. The long-held belief that Rooms 7 and 8 together comprise an official palatial archive is actually poorly supported by the archaeological evidence. Beyond the presence of so many Linear B tablets in these rooms, a critical examination of the archaeological evidence reveals that they were not designed, conceived or even conceptualized by their Mycenaean builders as an archive and there is, as a result, no validity to the modern label that has been applied. The evidence that I have presented clearly shows how problematic current models are from many perspectives including those of architecture, artefacts, function and operation. It is my contention that in developing and maintaining these models, basic archaeological methodologies of contextual analysis have effectively been discarded because of an unspoken consideration of the primacy of the textual evidence.

This chapter has focussed primarily upon the archaeological evidence with little consideration paid to the evidence of the tablets themselves, other than to show the potential for their presence being transitory. However, in order to provide some verification and justification for the radical changes that I have proposed and to understand the purpose of gathering the majority of tablets in this one place, we need to examine the evidence provided by the tablets themselves. Whether or not the basic notion of an archive is correct, these documents were deliberately placed within these rooms for a purpose and that purpose is most likely to be embedded, in part, in the physical organization of the tablets within the rooms and so it is to these documents that this study must now turn.

Chapter III

Setting the public agenda? Labelling gifts and obligations at Pylos.

Situated at the hub of palatial life, it is not surprising that the AC came to be a repository for the Linear B tablets, however temporarily. Yet our expectation is that these documents, and indeed writing more generally, should find their greatest functional expression within architectural elements of the palace. In other words, there is a general expectation that writing 'belongs' inside, rather than outside, the palace buildings. What is noticeable from the distribution of tablets around Pylos is that, rather than concentrating upon a single internal area of the palace, the distribution surrounds the major external areas of Courts 63/88 and 58, as well as internal areas such as the main megaron. The two rooms of the AC, rather than forming a peripheral element of the Main Building architecture, appear to represent a significant node of communication between these external areas. Despite these observations and the undeniable concentration of tablets within the AC, the archaeological evidence alone does not, necessarily, allow a functional attribution to be provided for this suite of rooms.

However, before beginning analyses of the tablet contents and theoretical discussions of their role in attempting to resolve this issue, it is worth prefacing such analyses with a potential scenario that accounts for the findings of the previous chapter. What follows is essentially a hypothetical reconstruction of events and, of course, begs as many questions as it may answer. Nonetheless, a simplified exploration of one possible context in which the tablets played a pivotal role provides a useful backdrop to the subsequent document studies. In this scenario, the AC simply represents a node of communication between the three main arenas of feasting activity deduced by Bendall (2004). The broad distribution of tablets that I previously referred to suggest that these three venues (Court 63/88, Court 58 and the main megaron) were not only the most significant areas for feasting but were also, simultaneously, the focus of ceremonial activities in which writing was an explicit element of proceedings.

Recent studies have established a strong connection between these areas of the palace and elaborate ceremonial or feasting activities and associated oral performances (e.g. Davis & Bennet 1999; Bendall 2004; Bennet 2007; Peters in prep. (b)). As far as I have been able to establish, there has been no suggestion that writing, and hence the tablets, played any role in

such proceedings. However, given the preceding analyses, it seems to me that the two rooms of the AC were ideally situated between all three conjectured arenas to have played a useful, perhaps mediatory, role. Indeed, the suggested reconstruction of doorways permits easy access both from the main megaron and the two external courts. It is perfectly possible, therefore, to imagine that in a large-scale ceremonial gathering at the palace these rooms were, at the very least, a pivotal node allowing access to rooms of the palace without interrupting events in any particular arena. It is in these arenas, I believe, that the purpose of the tablets becomes apparent and from where they were later taken and temporarily placed in the AC, forming the dense accumulation later to be discovered by Blegen. In such contexts many of the idiosyncratic characteristics of the tablets are understandable, from the formulaic nature of syntactic construction contrasted by the often irregular implementation of formulae through to the variability in compositional quality. If so, how could we arrive at the situation visible in the AC?

The faunal remains associated with the ceramics and, indeed, the metalwork from Room 7 suggest that one of the final acts of the palace was the provisioning of a large-scale event that included feasting, but what was this event that brought so many to attendance and, beyond the consumption of food and drink, what else may have occurred? It would be too simplistic to characterize this as a feast alone or to suppose that these gatherings were an altruistic act of entertainment by the palatial authorities. Instead, it seems likely that the underlying motivation operated at many levels, from the purely ceremonial and possibly religious through to more significant political concerns. The potential for the separation of guests between venues, alone, argues in favour of such a scenario. This was the ideal context for the palace to pursue a political agenda, maintaining or strengthening its own position as well as accruing new allegiances from all levels of society.

Nevertheless, defining the precise nature of this gathering remains problematic. The artefacts and architectural organization indicate a formal setting and highly ritualized proceedings. Participants were separated, in all likelihood, according to social status and, consequently, it is likely that different activities were occurring across each arena. But what would these have involved? Recent work into the importance of considering the courts as arenas of performance has demonstrated the potential for oral/visual performances surrounding the wall-paintings, particularly those of Hall 64. In the outer court (58), it is similarly possible to imagine that with the wall-paintings adorning the entrance system of the Main Building oral/visual performances were possible. These may have been aimed at entertainment, but as the exploration of embedded political statements within the paintings have demonstrated, it

is equally possible that the underlying motivation for performance was political in nature (cf. Davis & Bennet 1999; Bennet 2007; Peters in prep. (a)).

Given the undoubted importance of performance it is worth considering the possibility that writing, as another facet of communication, may have played a significant part in such proceedings. For the majority present, writing was an unknown quantity, mysterious and unintelligible. The tools involved in producing individual documents may have been familiar or through the materials utilized have possessed a degree of familiarity, but the activity and signs were probably not. As a purely functional tool, writing would provide little advantage other than to demonstrate the ability of the palace to command and exploit an unknown technology. Yet the mystical qualities surrounding writing provide a far more significant dimension that could be exploited, but one that would achieve its full potential only in a public setting. The preponderance of personal names, place names and other elements of individual and group identity, for example, suggest that the power of these documents lay in the ability to demonstrate the 'removal' of a part of this identity into the hands of the palace. The mysterious signs of writing were being used to subvert identity and gain a hold over those concerned. With little or no research conducted into the potential magical properties of writing in Mycenaean Greece, we can only speculate as to the power of such actions. Nevertheless, for the majority it is likely that consigning something as personal as a name to mysterious signs upon a clay tablet would be, at the least, disconcerting and recalls the Homeric reference to 'baleful signs' associated with name of Bellerophon (*Iliad* vi.168-69).

Writing could therefore be a powerful socio-political tool and, within an environment controlled by the palace, a further means of consolidating their position. At this event we can therefore contemplate a complex political agenda operating, supported by the presence and activity of writing. Within each arena a specific agenda pertinent to the assembled individuals and groups would have been addressed. However, we need not assume the presence of all those individuals and groups so considered. As will be discussed in Chapter V, there is evidence to suggest that representatives were present and could act on their behalf. Of course, the separation of attendees between the three venues complicates the modelling of presence or absence, particularly given the considerable overlap of names between documents highlighted by prosopographical studies (Lindgren 1973a; b; Nakassis 2006).

In considering writing as a significant element of such an event we are automatically faced with considering the so-called 'interface between the written and the oral' (e.g. Goody 1987; Thomas 1989). At Pylos, however, this situation is immediately complicated by the

suggestion that several venues are involved in such an event simultaneously, with writing potentially a component part of each. In the main megaron, for example, for which the cluster of tablets in the Inner Propylon may provide direct evidence, the underlying purpose of writing may have been different to other venues. Here we may conjecture that it was issues of policy that were of primary importance (as may be demonstrated by Jn 829) and, as such, this was an arena in which the political agenda operated at a different level from that in the external courts. Yet, it is also noticeable that the overall tablet distribution indicates that this was not the main arena in which the written word was significant. The majority of attention appears, instead, to focus upon the external areas. Nevertheless, there is every reason to suppose that the principles underlying the use and practice of writing were similar across each area. As policy was made, gifts distributed, disputes raised or the expectation of obligations to the palace detailed and enforced, written records were made. Literate officials would be on hand to record the nature of transactions, verify the honouring of debts and obligations, and the provisioning of gifts whether strictly material in nature or of work. More particularly, we can see an overriding concern with establishing and recording identities and relationships, further hinting at the 'magical aura' surrounding the technology of writing.

These, unlike the 'scribal offices' of conventional models, were undoubtedly dynamic arenas for the scribes to operate in with information, albeit often repetitive and reducible to apparently standardized syntactic formulae, being provided at a rate that made issues of form and neatness occasionally haphazard and sporadic errors inevitable. The potential for negotiations between the recipients of palatial favours and their patrons further complicated the process of tablet production, with changed decisions requiring erasures and corrections on the part of the scribes. Furthermore, whereas in this oral environment 'new business' required the creation and modification of documents from pre-prepared tablet blanks, most ably demonstrated by the cut tablets of the Jn series and the contiguities of the Sh series, the tablets also demonstrate the existence of obligations towards the palace and the expectation of service in support of their activities. In these situations pre-existing or, potentially, recently created summary records pertaining to previous or outstanding obligations would have been brought forth, for which some of the labels of the Wa series may provide direct evidence.

With these tablets, the potential existed for a complex process of monitoring and verification of the reciprocity expected from the debts and obligations, whether labour related or material in nature. As will be explored more fully in chapter IV, adherence to such obligations appears to have been less than systematic, which may point to the rewriting of documents with individual 'payments' received simply being removed. In some cases, however, it

appears that individual debts were checked with an incised cross as having been received, with outstanding issues left unchecked. Ultimately, as each 'item of business' was concluded, the corresponding tablet sets were removed from the arena in question. Some, particularly those concerning obligations that have been met, may have been publicly destroyed, emphasizing to all present the honouring of obligations and the removal of debt. Others, including outstanding obligations, were simply gathered in a convenient location away from these ceremonial activities in an area of the palace, which has come to be known as the AC, awaiting further attention. Whether these included tablets that were also awaiting disposal, or were retained until such time as the business they detailed was concluded remains to be seen. Yet, as will be explored below, it can be no coincidence that the resolution of issues is the one feature missing from the conventional models of palatial 'administrative practices'.

Beyond this point, we can only speculate as to what may have happened to the tablets. The temptation would be to assume that temporary placement in the AC conforms to an annual bureaucratic system of monitoring palatial transactions, with the evidence of the previous chapter simply reducing to an argument over the identification of the palatial archive and the existence of an intermediate or incomplete step in the archival process. Yet if the basic hypothesis of writing as a central element of a ceremonial event is plausible then it is equally possible that no regular cycle of 'administration' existed, or even that 'administration' should be considered the most accurate portrayal or characterization of the function of writing within the palace. Indeed, generalized descriptions of the documents as 'administrative' and 'bureaucratic' may, ultimately, be misleading. It is all too easy to envisage a uniform purpose to the documents, however generalized that purpose may be. However, in the context discussed here each list may have functioned very differently from the conventional notion of a Mycenaean 'bookkeeping' system. Some may, indeed, simply record incoming material. Others appear to be representative of gifts and favours bestowed by the palace. This, however, simply addresses the practicality of such a system. Underlying this are the mystical or magical properties that may have been of equal importance but which have yet to be explored.

Unfortunately, we currently have no means of establishing the frequency of ceremonial, religious or political events at the palace that may have involved such activities. The evidence from the AC and the well-stocked pantries suggests that the event discussed here was of a singular nature. There is, however, nothing to preclude other, similar events from having been expected or that this particular event was not itself interrupted and was originally meant to extend beyond the point at which the palace succumbed to fire.

Inevitably, these thoughts impinge upon the concept of an archive and the removal of tablets to the AC, however temporarily, does not exclude such a concept. Indeed, the very notion of a public display of records of debt, obligation and beneficence implies the retention of records in some form. Yet, such retentions need not have been for extended periods for all documents, let alone a full year. Similar, if smaller, events may also have involved documents such as these. The result of this would be to have 'records' constantly in flux and the number of documents retained may have been significantly less than has previously been imagined for a purely bookkeeping system of writing.

Documents may, as has often been speculated, have been transferred to perishable materials, or the tablets themselves may have been kept. In my opinion, however, there is little conclusive evidence to suggest that the AC was the hub of such activities. Neither is there any substantial evidence for the long-term retention of clay tablets. Their functional properties appear to have been fleeting. Even the few tablets that have been tentatively identified as older than the majority seem to reinforce this notion. Whether their existence owes more to fortune than deliberate retention, the essentially synchronic nature of the documents argues against a simple bookkeeping tool. The Ma tablets, for example, that refer to 'last year's' debts in conjunction with the current year's assessment, provide a level of detail that would imply the existence of earlier documentation. Yet, none has been recovered. If, however, these documents were produced in the context I am arguing for, such absences are not surprising. Records pertaining to previous obligations were addressed publicly, outstanding matters recorded alongside newly established obligations, and the earlier records destroyed along with the honoured debts and obligations that they recorded. This simple scenario also caters for the curious inclusion on some tablets of individual exemptions amongst town assessments. Publicly, either an agreement has been reached or a policy has been imposed that privileges have been granted to certain individuals.

Despite my denial of the AC as a functioning document archive, this does not equate to a denial of the principle of the retention of records somewhere in the palace. Temporal indicators within the tablets found in the AC give no information concerning the span of time that they were located in these rooms. It is simply to suggest that disastrous events may have overtaken the palace and that archaeological investigations have revealed a 'snapshot' of a dynamic process interrupted by terminal events in the palace's history. Indeed, it is entirely possible that the assemblage of objects recovered from the AC represent the material remnants of the final activity, or activities, which occurred prior to the fall of the palace.

What I have attempted here is to illustrate a possible alternative to the conventional model of tablet use that, I suggest, provides a more parsimonious explanation of the archaeological evidence and the obvious problems surrounding the interpretation of the AC. Once again, it must be emphasized that this is a somewhat crude outline. Yet even these initial thoughts highlight the potential complexities involved in the accumulation of documents in the AC. As with any new hypothesis, however, the emerging questions outweigh the potential answers and further progress requires a consideration of the contents of the documents themselves. In what follows I examine one of the organisational devices that has traditionally been viewed as employed in the archival grouping of documents sets, the labels of the Wa series. Extending my analysis into a specific case study of how these were used in relation to the Sa series, I suggest an alternative scenario in which the labels provide strong supporting evidence for the situation summarised above. Rather than a single, static function, these appear to relate to issues raised in these performances from outstanding records and to organize newly created sets of tablets.

Reconsidering the Pylian ‘filing labels’

Perhaps one of the most significant and under-utilized lines of evidence available at Pylos are the group of 19 ‘tablets’ of the Wa series, found in both rooms of the AC, commonly referred to as labels. The description arose from both their physical form and the consistent presence of impressions of wickerwork on the reverse (Chadwick 1958; Bennett & Olivier 1973: 261).¹⁴ These are not tablets in the conventional sense, but consist of small lumps of clay evidently pressed onto a surface creating an irregularly shaped object. The visible face (*recto*) is uneven and seldom smoothed and their seemingly careless production and inherent fragility has, unfortunately, resulted in relatively poor preservation. The nature of the inscriptions varies greatly from a single word, such as on Wa 732, to more verbose descriptions such as on Wa 730 and 917 and as a result little critical or systematic work has been carried out on this group following the brief assessment conducted by John Chadwick (1958). The majority of later discussions have been highly selective, concentrating on those labels which are superficially identifiable as being associated with known series of tablets. From the outset, these have been viewed as representing an organizational tool of the scribes, essentially functioning as abstracts for a semi-permanent archive of written documents stored in wicker baskets within the AC (*ibid*: 5). According to this argument, they are in effect a

¹⁴ The only exception to this is Wa 569 which was evidently pressed onto a flat surface rather than onto wickerwork. This label was originally unclassified but later analysis led to its inclusion within the Wa series and was interpreted as having been possibly attached to the exterior of a wooden box (Melena 1996-1997 [1998]: 161; Palaima 2003: 181). A summary of the labels can be found in appendix A and transcriptions of the texts of this series are provided in Appendix B.

direct reflection of the organisational structure desired by the scribes in the ordering of files and dossiers, and the inscriptions they bear should therefore relate directly to the subject matter of the tablets themselves.

Subsequent studies of the AC have resulted in this basic model being questioned and revised, and the labels have come to be viewed not strictly as archival organization labels but as transportation or delivery labels attached to baskets of tablets being delivered to the AC (e.g. Palaima & Wright 1985: 260-61; Palaima 1988; 2003: 178-81; Pluta 1996-1997 [1998]: 243-44). According to this hypothesis, labelled baskets of tablets were brought into Room 7 and left next to the doorway connecting with Room 8, prior to tablet processing and removal for storage. This attempted to reconcile the problems of why a large proportion of the labels were found in a single grid square within Room 7 and why, if Room 8 was the document storage area, fewer labels were found there relative to those recovered from Room 7. Nevertheless, both of these models of label use ultimately rely upon a basic conviction that these two rooms functioned as a document processing and storage area. But, with a degree of circular logic, the labels have also been used to confirm the storage of document sets in Room 8 (Palaima & Wright 1985: 260).

As with many studies surrounding the bureaucratic, administrative and archival activities of the Mycenaean palaces, there is a temptation to support such an interpretive model with comparative evidence from other ancient archives, predominantly those from the Ancient Near East, particularly from sites in Syria and Anatolia (e.g. Pluta 1996-1997 [1998]). In the latter case apparently similar labels were recovered from the ruins of the libraries of the Hittite capital which, attached to wooden storage shelves, marked the contents of tablets placed there. In addition to these brief labels a series of catalogue texts were found that summarised the contents of the archives, but also recorded omissions or documents that should have been present (Bittel 1970: 16-17; Bryce 2002: 64-66). These organizational texts were evidently produced with care unlike the Pylian examples which, although they are clearly identifying tags of some form, are merely crude lumps of clay, created rapidly, to serve a particular and transitory purpose. Any basis of comparison is, I suggest, slight with the form of organization represented by the Hittite examples considerably more ordered, complex and long-lasting.¹⁵

What has emerged from these deliberations is a model of administration in which the labels are viewed simply as part of a hierarchy of information processing and storage (figure III-1).

¹⁵ As discussed further in chapter IV.

Within this model, their production and use is viewed as parallel or supplementary to the primary concern of creating administrative documentation and, depending on which function they are deemed to undertake (transport, storage or a combination of both), their position in the hierarchy changes accordingly. While the veracity of this basic administrative model will be examined in greater detail in subsequent chapters, the place of the labels within it is, nonetheless, far from convincing. Indeed, their function has largely been assumed on the basis of the core structure of information processing and the apparently logical premise that some means of identifying sets of documents would be required by a scribal community. However, in this chapter I present a detailed analysis of these objects demonstrating that these assumptions disguise a more subtle, transitory and perhaps less tangible function than has been envisaged.

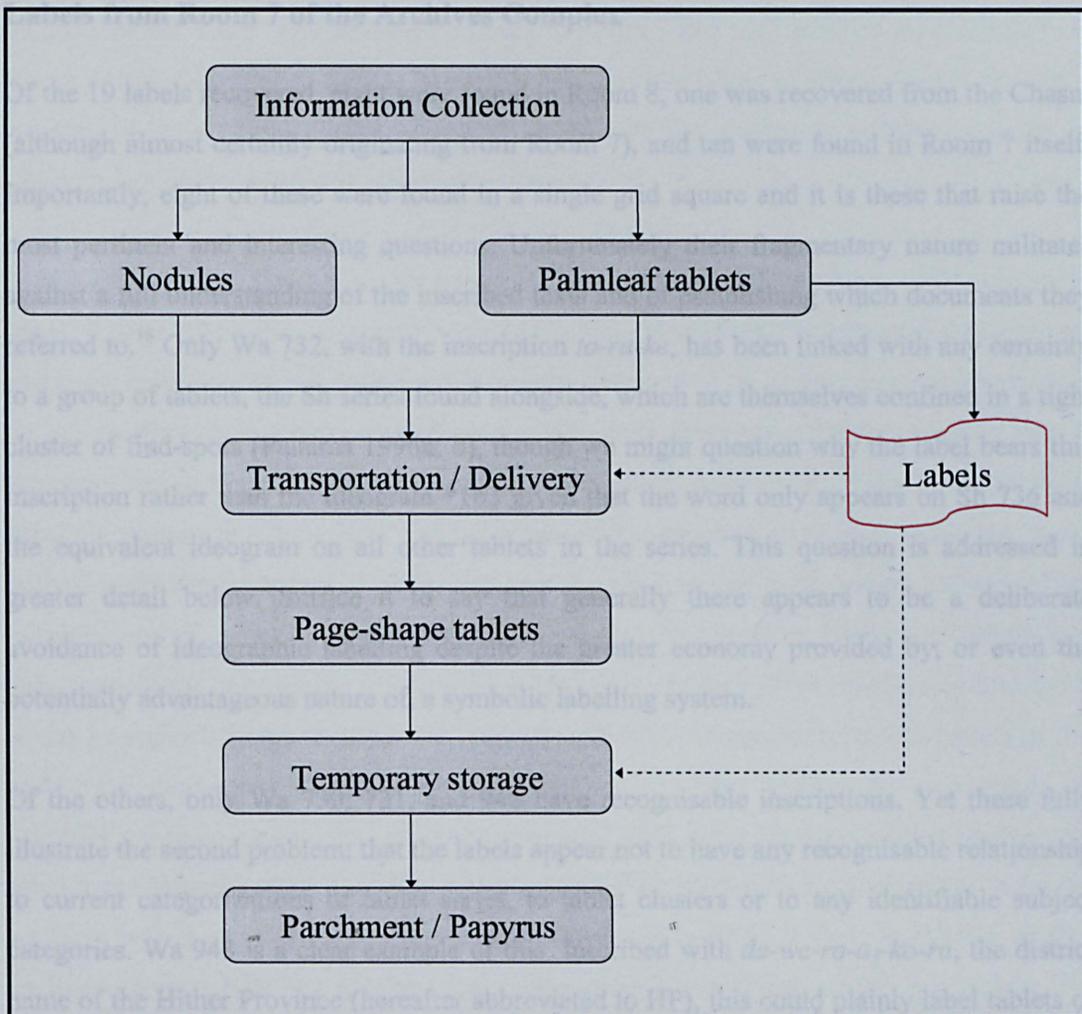


Figure III-1: The hierarchy of stages in information recording and archiving at Pylos (adapted from, Driessen 1999: 208, fig. 2; Schoep 2002: 193, fig. 4.3).

One general issue needs to be addressed first and that is the use of scribal identifications. Although many of the labels have been assigned a scribal hand (Palaima 1988), this data is

of marginal value in the analysis of 'labels'. Because many are in a poor state of preservation and bear very short inscriptions, several of the assignments have been based upon interpretations of the subject matter to which these labels may have belonged. Despite considering the general characteristics of material, vocabulary and writing styles, identifications are generally with the hand that wrote the series assumed to be the subject of the label. This in itself assumes a model based upon the labels being transportation docketts written by the scribe responsible for collating the tablets being deposited in the archives. The circular nature of this argumentation means that while some labels possess relatively secure scribal identifications, many can only be considered tentative and insecure. These data, collectively, cannot be viewed as anything other than unsafe and will therefore be avoided in this analysis.

Labels from Room 7 of the Archives Complex

Of the 19 labels recovered, eight were found in Room 8, one was recovered from the Chasm (although almost certainly originating from Room 7), and ten were found in Room 7 itself. Importantly, eight of these were found in a single grid square and it is these that raise the most pertinent and interesting questions. Unfortunately their fragmentary nature militates against a full understanding of the inscribed texts and of establishing which documents they referred to.¹⁶ Only Wa 732, with the inscription *to-ra-ke*, has been linked with any certainty to a group of tablets, the Sh series found alongside, which are themselves confined in a tight cluster of find-spots (Palaima 1996a; b), though we might question why the label bears this inscription rather than the ideogram *163 given that the word only appears on Sh 736 and the equivalent ideogram on all other tablets in the series. This question is addressed in greater detail below. Suffice it to say that generally there appears to be a deliberate avoidance of ideographic labelling despite the greater economy provided by, or even the potentially advantageous nature of, a symbolic labelling system.

Of the others, only Wa 730, 731, and 948 have recognisable inscriptions. Yet these fully illustrate the second problem: that the labels appear not to have any recognisable relationship to current categorizations of tablet series, to tablet clusters or to any identifiable subject categories. Wa 948 is a clear example of this. Inscribed with *de-we-ro-a₃-ko-ra*, the district name of the Hither Province (hereafter abbreviated to HP), this could plainly label tablets of potentially differing subject matters but all relating to this region. How many individual texts it may have covered is largely conjectural. It may be that only few tablets were collated

¹⁶ All find-spots have been converted according to the method outlined by Pluta (1996-1997 [1998]: 233) to produce co-ordinates that map onto a standard X-Y co-ordinate plan. All records of find-spots in this thesis have been adjusted accordingly.

under this heading if the subject matter was specific, or conversely, that a large number were involved if the organizational imperative was not specific subjects. We can be sure that some tablets were at least considered in this fashion, if not stored along these lines. The Aa series, for example, enumerating the disposition of women workers has been shown to be divisible by province (e.g. Chadwick 1988), and the recent analysis of the Ma tablet find-spots in Room 8 (Firth 2006) has demonstrated that in all likelihood, these tablets were also grouped together according to province. It has also been suggested, based upon scribal hands and vocabulary that this label and Wa 917, also found in Room 7, can be clearly identified as belonging to the Na series (Palaima & Wright 1985: 260, n. 31; Palaima 1988: 121). This suggestion alone amply illustrates the confusion surrounding these labels, for alongside this description of a clear identification, is the contrasting statement that this is only a tentative identification (Palaima 1988: 41). Ultimately though, such ascriptions cannot be considered as unequivocal, since both labels were found in Room 7 and the Na series predominantly in Room 8. It is clear then that if the traditional model of the labels as organizational devices is maintained, it is virtually impossible to identify with any degree of certainty a group of tablets to which this label was intended to belong.

In contrast, Wa 730 may well have served to label only a very few tablets, again relating to place, despite it being an unusually verbose example. The text is, unfortunately, incomplete and once again unclear but possibly refers to the distribution of 10 unknown commodities to an unidentified place and the contribution of one unknown commodity to *sa-ma-ra*, one of the major towns of the Further Province (hereafter abbreviated to FP).¹⁷ Whilst the numbers involved suggest that the quantity of individual documents may have been small, the place-names again demonstrate a classification that does not appear to correspond to any of the current tablet groupings, or to any identifiable tablets. Nevertheless, based upon vocabulary this example has been related to texts of the Ma series and then, from considering find-spots, to the Es series (Chadwick 1958: 2-3; Palaima 1988: 67). Found next to four tablets of this series, the description of 'contributions' made on the label appears to correspond, but crucially the place-name *sa-ma-ra* does not appear at all in the Es tablets. Conversely, if the label could be demonstrated to refer to the Ma series, which does include reference to *sa-ma-ra*, there is then the problem of why the label was found in Room 7 while the Ma series were in Room 8, and how the numbers involved correlate with those of the Ma tablets. The preferred explanation for this is that the label, attached to the delivery basket, had been discarded in Room 7 following the filing of the Ma series in Room 8 (Palaima & Wright

¹⁷ The latest transcription notes that the number '10' may not be genuine but rather a 'scratch' (Bennett *et al.* n.d.). Whether or not this is the case, the basic argument forwarded here regarding the surprising specificity of the label is unaffected.

1985: 260, n. 31; Bennet 2001: 28). Yet if Firth's (2006) conclusions are correct, the means by which the Ma tablets were contained and organized was by reference to the two provinces rather than to individual town assessments; thus it would appear that once again, no group can be singled out as pertaining to this label.

A very different and revealing picture emerges with Wa 731, which apparently refers to tablets recording unknown contributions to a group of people known as the *o-wi-de-ta*. This term clearly points to an association with Un 718 found nearby, but there is no obvious means of relating this inscription to any others (Chadwick 1958: 3). Here then we are faced with a conundrum; why should a lone tablet require its own label, and if the labels in this grid square are there because they have been disposed of, why is Wa 731 there at all? Several possible explanations would seem to present themselves:

- ❖ the associated tablets are simply not preserved,
- ❖ the corresponding tablets have been taken elsewhere,
- ❖ the majority of the information has been transcribed onto another medium and the original tablets disposed of,
- ❖ since Un 718 is a prospective text, with the word *do-se* clearly setting out events that are yet to occur, the remaining tablets have not yet been received,
- ❖ despite the word *o-wi-de-ta* not occurring on any other tablets, texts detailing named individuals known implicitly to be members of this group are indeed present within the AC but are simply not obvious to us.
- ❖ the tablets were undergoing reclassification and the label became redundant,
- ❖ the label simply does not relate to the organization of document filing or transportation.

The first five explanations all raise serious objections. It seems unlikely that we are faced with a simple preservation issue involving what would amount to the inopportune and selective disappearance of an entire class of tablets. Similarly, if the tablets had simply been moved elsewhere it is unlikely that the apparently important text Un 718 would be kept separately, or that the label would have been removed from it. The argument against transposition is very much the same. Why is Un 718 still present but the label apparently disposed of? Moreover, as Un 718 is prospective, it is unlikely that documents related to future events would have been transcribed prior to validation of these events. The possibility

that tablets containing associated information have failed to be recognised is just as implausible for it certainly brings into question the choice of label inscription employed. Why would the scribes choose to file tablets under the least obvious subject heading? In any event, it again fails to explain why the label was removed from the presence of Un 718. Equally, reclassification of subjects cannot be realistically considered when no other tablets can be identified belonging to this particular classification, and there is no evidence for new labels being created to replace it. Only the last explanation provides a plausible scenario, which will be discussed in detail below.

Of the remaining four labels found in the same location the inscriptions are poorly preserved. One remains unknown (Wa 748), while Wa 931 has been suggested could be reconstructed as bearing the place-name *a-ri-go* (Melena 2000-2001: 375) and likewise, Wa 947 may plausibly be reconstructed as the place-name *ka-ra-do-ro* (Chadwick 1958: 3). In the case of Wa 931, the suggested place-name recurs only on the single tablet An 723.2 in the AC, which would produce a similar labelling conundrum as that provided by Wa 731 and Un 718. By contrast, the place-name *ka-ra-do-ro* appears on numerous tablets, but of those found in the AC, all have a find-spot within Room 8 or the Chasm. The closest example would be Cn 608.10 found close to the connecting doorway, but with no other evidence upon which to base an interpretation, no tablet association with Wa 947 can be safely made.

Wa 930, inscribed with]*re-ke*[and reconstructed as *we-re-ke* (enclosures) has been associated with four tablets of the Cn series (Cn 131, 202, 328 and 441) located in Room 8 that bear this term (Chadwick 1958: 3; Palaima & Wright 1985: 260, n. 31; Palaima 1988: 42-43), but it is not at all clear what the file classification is meant to represent. The term does not appear on any other tablets in this series, the AC, or indeed elsewhere. If we are to assume that this is a filing classification and the reconstruction of the text is valid, then it appears to be peculiarly specific and unrelated to the details of the documents. Of the four related documents, two (Cn 131 and 328) discuss deficits of stock from enclosures within named districts, while the others (less well preserved) apparently discuss the disposition of enclosures of named individuals again according to a named district. So, here we are presented with a label that does not refer to the issues for which the tablets were written, but rather a label that is apparently designed to identify very specifically these four tablets. Yet if this is so, as with the example of Wa 731 and Un 718, why should the label have been removed from these tablets, unless the label was created to fulfil a specific and temporary purpose.

The presence of faintly incised checkmarks on Cn 131 and 328, indicative of marking after the clay had dried, and therefore some reasonable time after the documents themselves had been written (Palaima 1988: 42), strongly suggests that this is the case. Checkmarks are generally scarce, but importantly where they do exist, as in the case of the four Cn tablets, they point to the original tablet being formulated prior to the accumulation or confirmation of all the relevant information. It is clear then that although prospective texts are markedly few, when these four Cn tablets were written, they too were prospective, if only for a brief time. The details they record were clearly, at some later date, checked against either the reality itself or reports of the situation. This could suggest that the labelling of these texts was necessary to allow for subsequent information acquisitions to be added to these documents.

These documents will be discussed more fully in the following chapter, but it is interesting to note that the only tablets to which this label appears to belong are checked documents in which not all of the entries have been verified. Indeed, it can be no coincidence that the very specific vocabulary of the label refers to one of a select group of tablets bearing such marks, and so it seems highly likely that the impetus behind labelling this group is in some way linked to these checks. This begs the question of whether the label originally referred to a greater number of documents relating to the disposition of enclosures that were also the subject of a verification process, but when the other documents had been completely verified they were subsequently destroyed. As will become apparent this may not simply be idle speculation, but whether or not this is the case, the question that arises from these examples, is in what context did these checks take place and why, given that the records were still incomplete after these checks were made, was the label removed? Suffice it to say that this situation suggests that the label served a purpose beyond the confines of the archives room, which once achieved allowed for the disposal of Wa 930 amongst the other apparently redundant labels.

These few examples reveal a number of features that create problems with either of the previously discussed models of label function and use. The first of these relates to the 'degree of specificity' of the inscription. On the one hand, labels have such a high degree of specificity that they can only refer to a single tablet whilst on the other, the degree of specificity is so low that any number of tablets could fall within the particular subject heading. Combined with this is their manifestly transitory nature, particularly evident with Wa 731, where there is no immediately obvious reason why a label should have been disassociated from the tablet(s) it was labelling. In these instances, there is no evidence to suggest that any form of reclassification had taken place or that the label itself had been

replaced with another. Neither does it appear that this characteristic is reconciled by the notion that these are transport labels; for once again, there is no obvious reason why the labels should have been removed from the respective tablets, particularly if the notion that Room 7 represents the delivery area is maintained. Moreover, with this model we are faced with seemingly contradictory practices. Both Wa 731 and 930 are disassociated from the tablets they were labelling but whereas Un 718 is located nearby in Room 7, the Cn tablets were found in Room 8. In other words, the argument that the disposed labels are still within Room 7 simply as a fortuitous coincidence of timing relative to the destruction cannot be applicable, since in the latter instance there was clearly time enough for the Cn tablets to have been moved from one room to the next.

Considering the discrete label cluster in Room 7

Undoubtedly the most curious feature of this group of labels is the common point of deposition; why should so many labels all have been found in one spot? With no obvious associations to established tablet series beyond Wa 732, they appear to have no immediate function within the AC as it has been operationally understood, with the result that it has been hard to explain their presence. The wicker impressions on the reverse do reveal in all likelihood that they were once attached to wicker baskets but clearly whatever function they served, reference to archived documents is no longer one of them. It is principally the lack of other documents in this particular spot that led to the suggestion that these are transport labels; tags attached to baskets of tablets being brought into the AC from other areas of the palace. With their accession to Room 7, the tablets were removed elsewhere, while the baskets with their attached labels were placed in this spot for subsequent disposal (Palaima & Wright 1985: 260-61; also Palaima & Shelmerdine 1984: 80-81; Palaima 1988: 182-86; Pluta 1996-1997 [1998]: 243-44).

This seemingly logical interpretation, at first glance, appears to encompass the only reasonable solution to the question of location as well as the apparently transitory nature of these labels; once their purpose had been served, they were simply disposed of. Unfortunately several problems are raised by this scenario, not least of which is the apparent timing involved. It implies that following delivery, tablets were removed, resorted and possibly recategorised and the baskets with labels attached temporarily stacked in this spot. But why have the labels been discarded in this manner and if they have, why had they not already been pulped or removed entirely? If we are to assume that these do represent transport or transference labels from one area of the palace to the other, there is no logical reason to dispose of these labels until such time as the tablets they relate to had been filed

and reclassified according to the needs of the AC. Yet there appears to be no other mode of classification in terms of file labels or lists and the distribution patterns of the tablets argues against the notion that ordered reclassification has taken place. If they are not to be kept with the relevant tablets, what ultimately is the purpose of a transport labelling system? Furthermore, the brevity of the inscriptions, and the lack of specificity on some, hardly seems informative either as to the contents of the basket or to a filing classification foreseen by the scribe delivering such a basket. A basket, for example, labelled simply with the name of one of the provinces would certainly be superfluous to requirements in the areas where the tablets were drafted but also, seemingly, of little or no use to scribes in the AC. We are therefore faced with the simple conclusion that these objects were not transport labels, or delivery tags of any description. There was simply no need for such a system.

Many of these issues come to the fore with the case of the Sh series of tablets and label Wa 732, probably the most secure combination of tablets and label that we possess. The document set consists of 12 palmleaf tablets, two of which are summary or totalling texts. The remaining 10 sub-divide into two sets recording the disposition of sets of armour, all apparently contained within a single basket and located adjacent to the cluster of eight labels that included Wa 732. A thorough analysis of this series, greatly facilitated by the presence of tablet contiguities, led to a hitherto unseen level of detail in the composition and processing of an individual set of documents, which included a reconstruction of how the tablets were originally arranged in the basket in which they were contained Palaima (1996a; 1996b). Importantly the analysis of these contiguities also demonstrated the rapidity by which the information was recorded and the scribal consideration of how the information was required to be arranged within the container. Indeed, the rate at which this process was conducted was such that the clay was still substantially wet when the AC was destroyed.

The proposed model viewed these tablets as having been written outside of the AC, possibly in the Northeast Building, delivered in a labelled basket to Room 7 and placed in the location in which they were discovered (Palaima 1996a; 1996b: 37). Here, it was suggested, they awaited final processing and probably transferral and storage within Room 8. However, this hypothesis, while appearing entirely logical, contains a number of problematic elements. Why, for example, was this delivery placed alongside material supposedly being disposed of? Why was such concern lavished on the physical arrangement of the tablets within the basket if it was to act merely as a transportation medium? What were the circumstances by which the information contained therein led to such a rapid production of the texts and why, ultimately, was a transport label needed at all? It does, after all, seem entirely unnecessary merely for the transportation and deposition of this series when, in all likelihood, the

uppermost tablets in the basket were the two summary texts. It would be a simple matter for the relevant scribe(s) to look at these documents to establish their contents. Moreover, the means by which these texts were composed, using the armour ideogram on a relatively large scale would make these texts readily identifiable with only a brief examination of the basket contents.

Over and above these questions, however, is the fundamental contradiction presented by this model. If the Sh series had indeed just been delivered and awaited processing then we are fully justified in asking what processing was required? They are, undoubtedly, the most organized set of documents that we possess and their careful arrangement shows that no further ordering was necessary. Why then should they need processing when they could simply have been transferred directly to Room 8? This question gains further significance when the evidence for label production is taken into account, for there is no evidence at all to indicate the manufacture of labels outside the AC. If we are to suppose that by a 'delivery' label, we are actually referring to a tag that was quickly produced in the AC upon receipt of this group, then it would have been necessary for the basket to have first been examined to establish the nature of the contents and to compose a suitable label. A simple inspection would have been all that was required to establish that a coherent set of documents had been delivered and that the basket could be archived.

Of the other possible processing tasks that could be envisaged, there is no evidence that documents such as these were ever the basis for the production of the larger page-shaped tablets, nor is there any direct evidence that these were to be transcribed onto a different medium. In any case, the proposal of any such additional processing tasks faces the same problem of the speed involved in production and transferral. It would seem then, that the only possibility is that these documents were required to fulfil a very specific and time-dependant task. As with the discussion of Wa 930 above, it therefore appears more likely that the presence of the label suggests that the requirement was not for identification within the AC but rather for use in a separate context entirely, when the identification of particular baskets was more difficult and where the precise ordering of the individual documents within the basket was a necessity.

The separate Room 7 labels

Two additional labels were found in Room 7 in positions distinct from the group discussed above. The first, Wa 917, is a particularly intriguing label bearing an unusually explicit inscription; the only one identified thus far, which bears the name of a particular and well-

known individual, *a-ko-so-ta*. Moreover, the find-spot is also somewhat surprising and puzzling. Found towards the western side of Room 7 next to a small cluster of seemingly unrelated tablets overlain by the smashed remnants of the pithos, the respective find-spots place the label and tablets at some considerable distance from the majority in Room 7, and it is perhaps its contents, rather than necessarily the subject matter, that makes this label of considerable interest.

The text itself has been rendered in a variety of ways depending upon the translation of *e-re-u-te-re*, but the suggested translation of Nakassis (2006: 418) that it should be read as ‘how *a-ko-so-ta* distributes to the *hequetās ereutēr*’, is sufficient for the purposes of this discussion. Although much has been written concerning the lexical relationship between this and Cn 3 (e.g. Palmer 1963: 172-75; Chadwick 1973: 206-07, 435; Lindgren 1973b: 50-51, 186), no similar physical relationship can be said to exist. Only four tablets, and all of these fragmentary, were found directly associated with this label. Two (An 615 and Fn 867) appear to have no direct connection with the subject of the label. However, Cn 702 and Cc 665 superficially at least may have a connection. The first records the ownership of 100 rams by *a-ko-so-ta*, the second the offering of livestock to Potnia; in this case 190 pigs and 100 rams. The correspondence between the figures for rams may simply be coincidental but, given the archaeological context and the implications of the label inscription, this seems unlikely. Certainly the inference that animals as religious offerings are being delivered to responsible officers such as the *heq^uetai* is reflected in other tablets, most notably Un 718. However, both Cn 702 and Cc 665 are fragmentary with components spread across Room 7 and even though some figures corresponds, the same cannot be said for the 190 pigs and so this can only be considered a possible association.

Nevertheless, the label itself shows similar characteristics to those discussed previously. The subject it concerns is extremely specific and, although associations are difficult to establish, it is likely that few tablets would have been brought together under this heading. In addition, the subject matter does not conform either to the standard thematic classifications that apparently existed, or to a classification that could be envisaged as being created for the delivery of a batch of tablets. As such, this label does not appear to fit with either proposed model of label function and use. The only clues to the use of this label would seem to lie in the nature of the duties being recorded for the individual *a-ko-so-ta* and the unusual find-spot. The fact that individual responsibilities are being referred to is contextually incongruous within what has been deemed to be an administrative archive devoid of social concerns and, as I have implied in several other cases, suggests that the context of use is other than that of an archive. The find-spot only serves to emphasize this and potentially

supports the suggestion made in Chapter II that access to Room 7 could have existed on the south-western side. Indeed, as I expand on in the final discussion below, it is entirely feasible that this label represents that last movement of tablets into Room 7.

The final label from Room 7 to be considered, Wa 784, also found apart from the main cluster of labels, clearly relates to the E-series concerning land holdings, but as has been pointed out the vocabulary alone makes no distinction between the various subsets of this series (Chadwick 1958: 3; Bennett & Olivier 1973: 90, 261). Unlike the parallel groupings of the Eb/Ep and Eo/En series which are distinguished by the opposing land types of *ki-ti-me-na* and *ke-ke-me-na*, or those of the Er series concerning the royal estates or, indeed, the so-called *dosmos* tablets of the Es series characterised by references of contributions to Poseidon, no such distinctions are provided by this label. As a result, definitive associations are difficult to make. However, its find-spot places it directly amongst a large cluster of the Ea series found alongside the north-eastern wall of Room 7 and has generally been regarded as associated with this group (Palaima 1988: 102). Given this association, superficially Wa 784 appears to fulfil the expectations of a filing label in much the same way that Wa 732 apparently serves the Sh series, in that the inscription closely resembles the subject matter of the associated series and the find-spot provides a close physical relationship.

This scenario is however problematic for several reasons, not least of which is that the Ea series is itself split between both rooms of the AC in no obvious, organized pattern. A plot of the relevant find-spot data (figure III-2) is highly revealing in this respect. The tight clustering of tablets and label in Room 7 is contrasted by a relatively dispersed pattern in Room 8. Yet, the interconnection of tablet fragments, with the exception of Ea 922, suggests that the effects of the destructions were not overly disruptive. Even those tablets in Room 8, which appear to be in some disarray, have not been overly affected and the tablet interconnections suggest that several separate groups were deposited here. More importantly, the separation between the two rooms is clearly not a function of archaeological formation processes but represents a deliberate separation. If Wa 784 were a filing label then we must question why these patterns exist and why there is no evidence of labelling in Room 8 despite the indications of the deliberate separation into small groups.

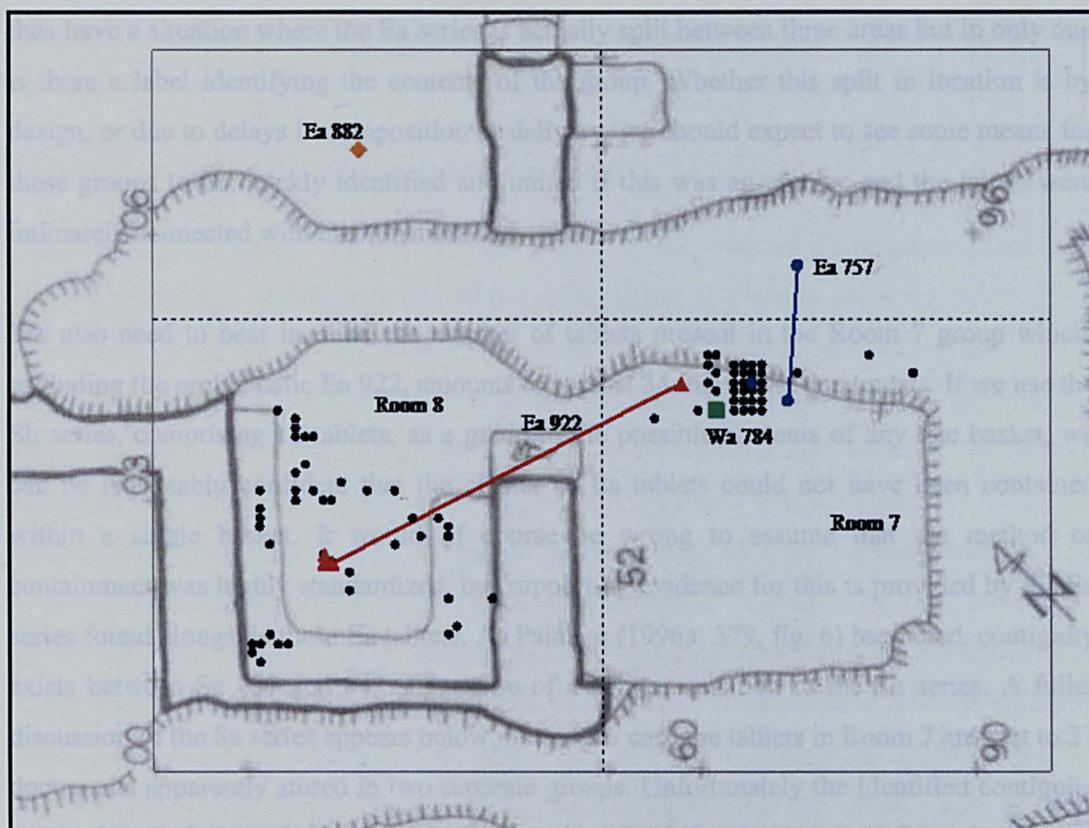


Figure III-2: The distribution of Ea series tablets in the Archives Complex.

This problem becomes more significant when the content of the Ea series is considered. As I have previously indicated, while the label clearly relates to issues of land holding, it does not make any distinction or specify the conditions under which such land was held. Yet the content of the Ea series is far from consistent and refers to several forms of land tenure including land held privately by individuals, land leased from individuals and land given over from the *damos*. Such distinctions are not, however, apparent in the distribution of tablets between the two rooms, and Room 8 contained many examples that could have easily been filed under the classification provided by this label. Moreover, these single-entry records appear to perform a similar function to those of the Eb series (which incidentally, Bennett & Olivier (1973: 90) observed could also be connected with the text of Wa 784) and it is therefore entirely possible that parallel, page-shaped tablets were intended to be composed from these in much the same way as the Eb/Ep and Eo/En series. If so, this label would be functionally inappropriate as an archival filing label.

However, these issues also reveal that the interpretation of labels as transportation or delivery tags is problematic. As I argued previously, Room 7 does not appear to function as the delivery point for tablets entering the AC but rather that documents entered from two directions, as the batch of tablets placed in the Inner Propylon demonstrate. Amongst this mixed group is the single tablet Ea 882 (figure III-2) but without any identifying tags. We

thus have a situation where the Ea series is actually split between three areas but in only one is there a label identifying the contents of the group. Whether this split in location is by design, or due to delays in composition or delivery, we should expect to see some means for these groups to be quickly identified and united if this was an archive and the labels were intimately connected with this function.

We also need to bear in mind the number of tablets present in the Room 7 group which, excluding the problematic Ea 922, amounts to at least 34 individual documents. If we use the Sh series, comprising 12 tablets, as a guide to the possible contents of any one basket, we can be reasonably confident that the cluster of Ea tablets could not have been contained within a single basket. It would of course be wrong to assume that the method of containment was highly standardized, but supporting evidence for this is provided by the Sa series found alongside these Ea tablets. As Palaima (1996a: 379, fig. 6) has noted, contiguity exists between Sa 787 and 843 suggestive of a similar situation to the Sh series. A fuller discussion of the Sa series appears below, but in this case the tablets in Room 7 amount to 27 documents apparently stored in two separate groups. Unfortunately the identified contiguity cannot be verified on the basis of find-spots since Sa 843 was recovered from the Chasm. Nevertheless the find-spots of the fragments are consistent with the tablet having originally been grouped with Sa 787 prior to the destruction and the subsequent creation of the Chasm. As is shown later in the detailed analysis of this group, the arrangement of the tablets within the basket probably does not mirror the Sh series, in that the totalling tablet must have been arranged next to individual records rather than kept separately layered. Nevertheless, the figures involved do indicate that a single basket contained considerably fewer tablets than those represented by the Ea series of Room 7.

It is reasonable, therefore, to understand the Ea series distribution in Room 7 as reflecting the contents of at least two baskets placed close to each other but with only one labelled. The find-spots also reveal that these baskets separated two baskets of the Sa series where again only one was labelled. If these represented the latest deliveries to the AC and only one label was deemed necessary to identify the baskets, logically the two groups of the Sa series should have been found directly adjacent to each other. The fact that they were not perhaps indicates that whatever function the labels served had already been fulfilled when they were placed in their respective positions. I suggest then, that as I concluded with Wa 732 and the Sh series, the labels had been produced to enable identification of their respective documents in a context other than that of the AC, and that both the extremely tight and coherent clustering of the Room 7 tablets compared to those of Room 8, and the positioning of the

two sets of Sa tablets separated by the Ea tablets, indicate that by the time the baskets had been placed in these positions, the role of the labels was already redundant.

Labels from Room 8 of the Archives Complex

If the labels from Room 7 do not fit either model of use, how are we to assess those from Room 8? With this widely regarded as the storage area, this would seem the most logical room in which to test the hypothesis of labels functioning as organisational devices. Of the eight labels concerned, Wa 401 and 1248 are too fragmentary to be of any help in this discussion whilst Wa 1148, along with Wa 1271 recovered from the Chasm, are considered separately in the analysis of the Sa series below. One label, Wa 569 (note 14), is unique amongst this series in having an impression on its reverse of a flat surface, interpreted as evidence of the label having been pressed onto a wooden container (Melena 1996-1997 [1998]: 161; Palaima 2003: 181). This interpretation is partly based upon the close proximity of the bronze hinges discussed in the previous chapter, but as we have seen, it is highly unlikely that these are evidence of wooden boxes and so the interpretation offered for Wa 569 must be considered speculative. Indeed if the interpretation of the bronze hinges as being components of folded writing tablets is correct (Shear 1998) then it is entirely possible that this label was attached to one such object. Little more can be deduced from this particular example for as with many of the labels from this room, the text is poorly preserved.

Wa 1093 is similarly problematic because of the brevity of the inscription, but once again presents comparable features to those already encountered. The suggested reconstruction of the inscribed word as the place-name *me-te-to* (Chadwick 1958: 4; Bennett & Olivier 1973: 261) and the find-spot alongside tablets of the Na series has prompted the suggestion of an association with Na 337 on which *me-te-to* appears (e.g. Palaima 1988: 120-21). However, corroborative evidence is lacking; the assignment of this label to the same scribal hand as Na 337 remains insecure despite similarities in sign forms (*ibid*: 121). Significantly, whilst the label find-spot places it directly amongst tablets of the Na series, Na 337 was found some distance away in a mixed group of tablets. Conversely, only three other tablets also bear this place-name (Mn 456.5, Vn 130.11 and An 35.2) and all were found within Room 8, but none provides more convincing associations than Na 337 with respect to find-spots. However this problem is resolved, it nevertheless provides another example of a label with a high degree of specificity, with which it is not likely that many tablets were associated.

By far the most informative label is Wa 114, which appears to address the 'monthly rations' of women in the FP and has therefore been taken to relate directly to tablets Aa 60-98. This

subset of the Aa series was found tightly clustered in Room 8 and the label found directly within the cluster. This situation, where a unified set of documents along with a complementary label were found in close proximity, is paralleled only by the case of Wa 732 and the Sh series, but unlike the latter Wa 114 could indeed have functioned as a filing label. Yet, once again, this correlation is not as straightforward as it first appears. If the interpretation of Wa 114 is correct then it relates specifically to the ‘monthly rations’ or allowances of food to women of the FP, not simply to cataloguing numbers of personnel, which is all that the associated set of documents accomplishes. This subset of the Aa series, also known as the ‘minor set’, unlike the ‘major set’ counterpart, has no complementary tablets in the Ab series; the series which details food distributions to the women (Chadwick 1988).

We are therefore faced with a scenario in which none of the tablets associated directly with the label detail quantities of foodstuffs assigned to the women, and so in this respect at least, it appears to have no bearing on the Aa series documents assigned to it. Considering the unusual verbosity and specificity of this label, and the accepted interpretation of Room 8 as a document storage area, the absence of any records relating to ‘rations’ is perplexing. The most obvious explanations, that these are documents yet to be delivered or, that given that they refer to the FP are potentially to be found at the FP capital Leuktron, run counter to the notion of organizing archival storage, but also to the notion of a function as a delivery tag. Uniquely then, Wa 114 appears to contain information that is supplementary to the series it pertains to but, as is discussed below, it is this feature that I suggest provides a clue as to the intention behind the use of these labels.

It has been assumed that Wa 1008, again bearing the MUL ideogram, parallels the function of Wa 114 and relates to the subset (Aa 240-1182) concerned with women of the HP (Chadwick 1988: 75; Palaima 1988: 45, 52). However, the find-spot places it in direct physical association with the same cluster of tablets as Wa 114 and while a few tablets of the HP subset were found in Room 8, the majority were recovered from Room 7. In addition, the text of this label does not preserve any mention of the province name or reference to ‘rations’ as might be expected if this was a mirror of Wa 114. It has been suggested (Palaima 1988: 72, n.92) that an association to three tablets of this Aa series subset can be inferred through the text of An 292 found close to Wa 1008. This text is headed by the trade-name *si-to-ko-wo* with the connection taken to be the incomplete word of Wa 1008.2 with the prefix *o-* (‘thus’). Whilst this interpretation is plausible, the proposed association is considerably more tenuous, since two of the Aa tablets concerned (Aa 788 and 792) were located in Room 7 and only Aa 354 found in relatively close proximity. Given the pattern we have already

observed with the Room 7 labels, it is not inconceivable that Wa 1008 is associated with An 292 alone. Whether or not this is the case, it does point to a very different function and degree of specificity for Wa 1008 compared to Wa 114 and one that cannot involve all of the Aa series concerned with the HP.

This variable pattern of function and specificity is maintained by the label Wa 362, which although badly damaged, has been plausibly reconstructed as bearing the word *ki-ri-te-wi-ja* (Chadwick 1958: 3-4; Lindgren 1973b: 82).¹⁸ Regrettably, line 1 remains unclear with no obvious correlations between the sign group and known vocabulary. Nevertheless, if the reconstruction is correct Wa 362 may be seen in a similar vein to Wa 731, bearing as it does the name of a known group of people. As with Wa 731, however, the number of tablets containing references to this group is small. Four tablets in the Pylian corpus refer specifically to this group: Un 1426 can be dismissed as it was found in SW Area 31. Two others, Eb 321 and Ep 704, are parallel texts concerning the same group of people but are probably unrelated to the fourth potential tablet, An 607 (see Chapter VI). The only clues as to which may be the correct association comes from the find-spots, which are largely inconclusive. Ep 704 is the least likely candidate for association as it was found within Room 7, but the parallel text Eb 321 was found reasonably close to this label. We cannot be certain that this is the associated tablet however, for amongst the general disorder of the tablets in this room An 607 also appears in close enough proximity to create doubt. Indeed, the latter association is more likely based upon the contents of the respective documents since Ep 704, of which Eb 321 is a single-entry version of one element of this document, does not concern the *ki-ri-te-wi-ja* specifically, but records the land-holdings of several key religious personnel as well as the *ki-ri-te-wi-ja*. It is only An 607 that is concerned with this group alone, and despite the problems of interpreting the initial line of the label, it does appear likely that it too was concerned specifically with the *ki-ri-te-wi-ja*. Yet, whichever of these tablets relates to Wa 362, a similar situation exists to that of Wa 731 in that the label appears to refer only to a small number of tablets and, as seems more likely, only one. To consider Wa 362 as a file label would therefore be unjustifiable but, unlike Wa 731, there is no evidence to suggest that it could be a delivery or transportation label either. Once more, the combination of this label and tablet appears entirely superfluous in an archive room and suggests that, again, the reasoning lies beyond these rooms and the immediate context of discovery.

¹⁸ The original analysis of this label refers to Wa 1087 subsequently joined with Wa 362.

Two labels remain to be discussed, Wa 1148 found in Room 8 and Wa 1271 which, although recovered from the Chasm, almost certainly originated from Room 7. Both of these labels appear to relate to a relatively under-discussed group of tablets, the Sa series, which is represented by tablet clusters in both rooms of the AC. In relation to the archival organisation and particularly to the question of the labels this series has already been seen to have important ramifications for the overall interpretation and is therefore worth analysing this group in greater detail.

Considering the Sa series and the associated labels

The Sa series comprises 34 tablets of palm-leaf type. With the exception of Sa 1313 recovered from Room 98, all of the tablets were found spread across Rooms 7 and 8 of the AC and the Chasm, with the largest concentration of tablet fragments discovered adjacent to the Chasm where the northeast wall of Room 7 had been robbed out. Tablets were assigned to the Sa series based upon the use of the wheel ideogram *243 (ROTA), with the exception of the unique tablet Sa 22 only, bearing the ideogram *105 (EQU). Two labels were subsequently identified as probably being associated with this series, Wa 1148 found in Room 8, and Wa 1271 recovered from the Chasm. All of these tablets and labels were written by the same scribe (Hand 26), again with the exception of Sa 22 (Class Ciii), and possibly Sa 1266. Based upon content of the inscribed texts, the Sa series has been subdivided into three categories or subsets (table III-1).

Group	Subject	Tablets
A	Dispositions	Sa 487, 753, 755, 758, 760, 763, 766, 767, 768, 769, 774, 796, 797, 834, 1264, 1265, 1266(?), 1267
B	Wheel condition	Sa 287, 488, 682, 751, 761?, 790, 791, 793, 794; 840?
C	Summaries / totalling	Sa 483, 787, 843
	Omitted ¹⁹	Sa 22, 403, 1313

Table III-1: The current Sa series categories (Palaima 1988: 92).

¹⁹ Sa 22 has no direct relationship in terms of subject matter with the rest of the Sa series but was originally classified as such based upon location and scribal identification (Bennett & Olivier 1973: 226). However, because the uniqueness of the sign groups provides a degree of uncertainty in the scribal identification it was subsequently reclassified by Palaima (1988: 132) and, with no commonality of subject, was omitted from these series subdivisions. The uniqueness of the subject matter within the Pylion corpus, the lack of any common textual content, the difference in scribal hands and the find-spot some distance from the clusters of other Sa tablets, in my opinion, clearly demonstrate that this tablet should not be classified within the Sa series. As a result this tablet is not considered further in the following analysis. By contrast, Sa 403 and Sa 1313 were excluded because they were deemed to be too fragmentary (*ibid*: 92, n. 137). See Appendix B for all tablet transcriptions.

Using these groups, the relative positions of the tablet fragments can be plotted (figure III-3) apparently showing little coherency in their positioning within the AC. Tablets from all three groups are present in Rooms 7 and 8 as well as throughout the Chasm.

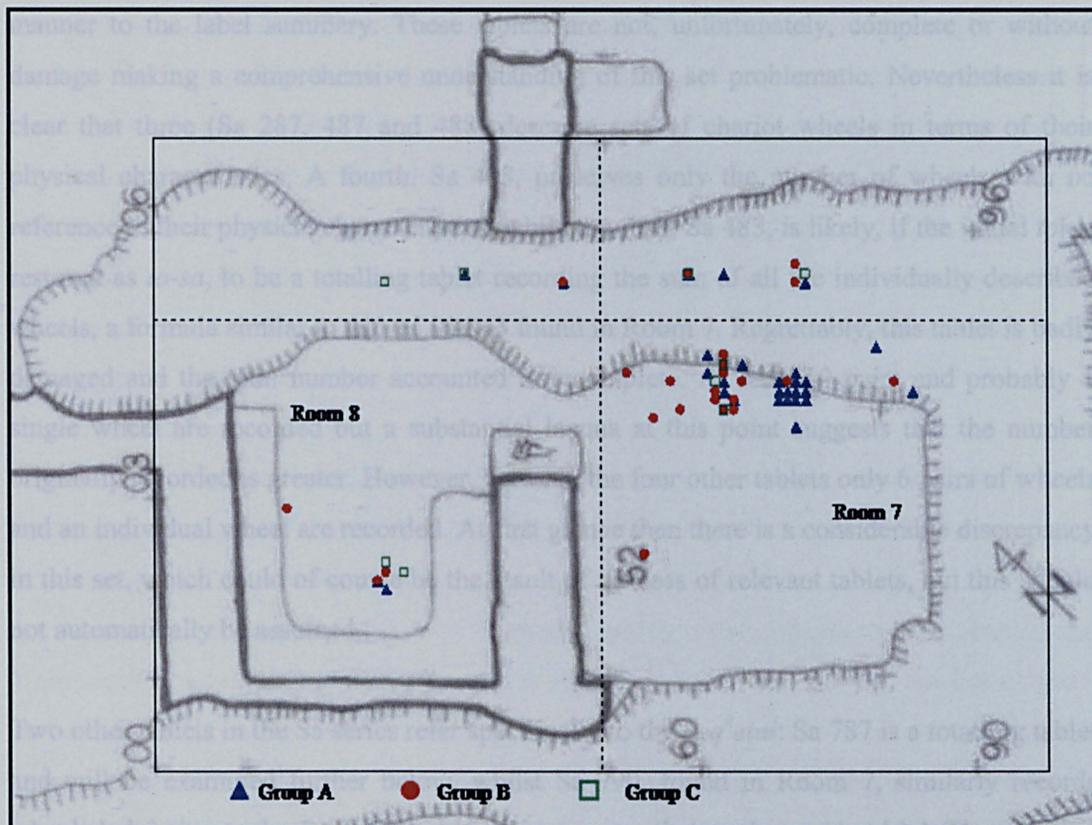


Figure III-3: The tablet distribution of the Sa series according to current groupings.

If these categories reflected the scribal organization of these tablets we would expect the distribution pattern, based upon these analytical groupings, to reveal greater consistency in their spatial arrangement despite the potential for tablet movement during the destruction of the AC. Clearly this is not the case. Instead, I suggest that the series analysis needs to start from the premise that the spatial distributions, with a margin of error, do reflect the deliberate separation of tablets. Thus the analysis of any series should begin with the archaeological evidence, rather than from purely textual readings. In order to illustrate this point, the discrete cluster of tablets in Room 8 provides a logical starting place. If we begin with the assumption, based on their close physical association, that these tablets form a coherent set and that the label, Wa 1148, similarly belongs to this set, then the combined texts should constitute a coherent and logical subject matter. Unfortunately, beyond originating from this room, the precise location of Wa 1148 is unknown but given the limited number of tablets of the Sa series in Room 8, its association with these is a reasonable

inference. The label itself bears an inscription describing ‘serviceable wheels belonging to the *heq"etai*’.

We should expect then that the five tablets in Room 8 should not only have a textual content that possesses an internal logic, but also that that logic should relate in some identifiable manner to the label summary. These tablets are not, unfortunately, complete or without damage making a comprehensive understanding of this set problematic. Nevertheless it is clear that three (Sa 287, 487 and 488) describe sets of chariot wheels in terms of their physical characteristics. A fourth, Sa 403, preserves only the number of wheels with no reference to their physical characteristics, while the fifth, Sa 483, is likely, if the initial *to[* is restored as *to-sa*, to be a totalling tablet recording the sum of all the individually described wheels, a formula similar to that of Sa 843 found in Room 7. Regrettably, this tablet is badly damaged and the total number accounted is incomplete. At least 10 pairs and probably 1 single wheel are recorded but a substantial lacuna at this point suggests that the number originally recorded is greater. However, between the four other tablets only 6 pairs of wheels and an individual wheel are recorded. At first glance then there is a considerable discrepancy in this set, which could of course be the result of the loss of relevant tablets, but this should not automatically be assumed.

Two other tablets in the Sa series refer specifically to the *heq"etai*: Sa 787 is a totalling tablet and will be examined further below, whilst Sa 790, found in Room 7, similarly records wheels belonging to the *heq"etai*, but in this case ones that are ‘unserviceable’. The question therefore, is whether Sa 790 has any relationship to the Room 8 group concerning the *heq"etai* and, as such, can it account for the apparent discrepancy in the figures for that group? If so, then its exclusion from the Room 8 group must be related to the ‘unserviceable’ adjective since label Wa 1148 categorically refers to ‘serviceable’ wheels. In fact Sa 790 records 6 pairs of ‘unserviceable’ wheels belonging to the *heq"etai*, a figure that is indeed consistent with the 10+ pairs recorded on Sa 483. Along with the total recorded in Room 8, this would suggest a figure of 12 pairs of wheels originally recorded on Sa 483. Importantly, Sa 483 makes no reference to their serviceability, it merely records the number belonging to the *heq"etai*, and so the apparent exclusion of such an adjective may be deliberate and not just an accident of tablet preservation or scribal omission.

It would seem then, that the difference in location reflected the need to provide a distinction between those wheels that were both serviceable and assigned to specific people and those that remained unserviceable. Thus the Room 8 tablets provide the information that 6 + 1 pairs of wheels out of 10+ pairs were serviceable and assigned/despached, whilst the

remainder, recorded under Sa 790, could not be incorporated until such time as their serviceability status changed. If these relationships are correct, then quite clearly the tablets found in Room 8, along with Wa 1148, form a coherent and logical group in contrast to the current subdivisions of the Sa series. Superficially at least, this set of texts does support the hypothesis that the find-spots are meaningful and that deliberate separation of tablets within the series was carried out. To demonstrate this more fully, though, it is necessary to explain how the remaining tablets of the Sa series relate to this pattern and what more can be said of the 6 pairs of unserviceable wheels attested in Sa 790? First we have to recognise that Sa 790, listing 6 pairs of unserviceable wheels belonging to the *heq^uetai*, is logically associated with the Sa tablets of Room 8, despite being housed in Room 7. Secondly, that this tablet serves a dual function, not only establishing the disposition of the remaining wheels attributed to the *heq^uetai*, but also acting as a totalling tablet against which the details of the unserviceable wheels could be viewed. But which tablets relate to this total?

A clue on Sa 790 is perhaps afforded by an erasure under the *a-mo* of *a-mo-ta*. Beneath is the word *ka-ko* (Bennett & Olivier 1973: 226), a word that is repeated on Sa 794 itemizing 1 pair of unserviceable wheels with bronze rims. It seems possible therefore that Sa 790 and Sa 794 are related. There nevertheless remain 5 pairs of unserviceable wheels to be accounted for. Unfortunately none of the remaining texts is explicit in terms of detailing ‘unserviceability’ and so we must rely on a greater degree of inference. The majority of individual records in Room 7 unambiguously refer to serviceable wheels (excluding totalling/summary tablets), however, three documents (Sa 753, 761 and 840) do not specify serviceability in any form. I suggest therefore that it is not unreasonable to suppose that the omission of a clear indication of serviceability is indicative of unserviceability. Between them, these three documents detail 5 pairs of wheels which, in addition to the one pair of Sa 794, suggest that Sa 790 does indeed total the unserviceable wheels described in these tablets that belong to the *heq^uetai*.

Significantly, the combination of this ‘subset’ of tablets and the summation tablet, Sa 682, provides a total of 12 pairs of ‘unserviceable’ wheels. It cannot be a coincidence that this figure matches the number of ‘old wheels belonging to the *heq^uetai*’ itemized in the totalling tablet Sa 787. Indeed, this tablet is the key to understanding the organization of this series, for as has long been recognized there is evidently some equivalence between the numbers listed on this and the remainder of the tablets. As Chadwick (1976: 170) notes, ‘the last entry on this tablet (Sa 787) matches Sa 751 so closely that it must surely refer to the same lot; but it is not clear how the other figures square with these tablets.’ Now, I suggest, these figures can be shown to square with those on Sa 787.

The last entry that Chadwick refers to itemizes 32 pairs of 'Zakynthian' wheels and, indeed, Sa 751 provides the information that 32 pairs of 'Zakynthian' wheels are unserviceable. Clearly these entries are related, and as I have shown, the entry detailing 12 pairs of 'old wheels belonging to the *heq^uetai*' corresponds to the combination of Sa 790 and Sa 682, but what of the entry describing 30 pairs (+1 extra) of old, serviceable wheels? It is important to note that this figure is itself not clear; originally this was read as 31 pairs but subsequently doubt was cast on this figure with the suggestion that the 1 of the 31 was an accidental stroke (Bennett & Olivier 1973: 226). The most recent transliteration maintains this position (Bennett *et al.* n.d.), but as I shall show, I believe that the original reading was in fact correct.

The starting point for this group must of course be Sa 793, which lists 11 pairs of old wheels. These are described as being 'thin' or possibly 'worn thin' but in either case it seems likely that despite being 'old' these are indeed serviceable. This would leave 19 or 20 pairs to be accounted for, depending upon the reading of the total on Sa 787. It then becomes obvious that this figure can only be related to Sa 843, another totalling tablet, which records the total number of 'new, serviceable' wheels as 20 pairs. Thus with Sa 793 we arrive at a figure of 31 pairs of serviceable wheels, sub-divided according to the status of refurbishment, the relationship among which is summarized in table III-2.

Sa 787 (pairs of wheels)	Sa 790	Sa 682	Sa 751	Sa 793	Sa 843
12 pairs (belonging to the <i>heq^uetai</i>)	6 pairs (unserviceable, belonging to the <i>heq^uetai</i>)	6 pairs (unserviceable)			
32 pairs (Zakynthian)			32 pairs (unserviceable, Zakynthian)		
31 + 1 pairs (serviceable)				11 pairs (last year's, worn thin)	20 pairs (new, serviceable)

Table III-2: A summary of the relationships of the Sa series totalling tablets.

Following the patterns of the previously discussed subsets though, Sa 843 must itself be summarising the details of a set of documents recording these 20 pairs of wheels. Here, we are fortunate that of the remaining tablets, fifteen contain an identical syntactic formula: a man's name in the genitive, followed by the number of pairs of serviceable chariot wheels (all with the ligature TE) attributed to that person. In total these record the ownership of, or

responsibility for, 18 pairs of wheels, leaving an apparent discrepancy of 2 pairs. Of the two remaining tablets not discussed, Sa 1266 is peculiar in terms of its layout. Ruled for two lines but with only vestiges of signs on its *verso*, its *recto* is similarly ruled with two lines with an abbreviated text (Bennett & Olivier 1973: 226). In content it conveys similar information to the 15 tablets mentioned above but without the use of the wheel ideogram and in a layout that is unique. The incorporation of *te-mi-de-we-te* does indeed suggest that the subject of this text is ROTA+TE, but the positioning of the word appears to be more of an afterthought, particularly given the extremely formulaic nature of the texts discussed thus far. Perhaps more significantly, as Palaima (1988: 93) has noted, the spelling of this word contradicts all other instances in this series (*te-mi-dwe-ta*) suggesting that the stylus attribution may be in doubt. That leaves Sa 791, which in all respects except for the omission of a personal name corresponds in form and content to the previously mentioned fifteen tablets. It documents the serviceability of two pairs of wheels with the TE ligature. Thus with Sa 791 we have the 20 pairs of ROTA+TE totalled in Sa 843.

The veracity of this group, in the same way that the veracity of the group in Room 8 can be established with the aid of Wa 1148, is improved by the existence of label Wa 1271, found in association with fragments of the Sa tablets in the Chasm. This too has been tentatively identified as belonging to the Sa series despite its poor preservation. According to Palaima (1988: 92, n.139) the reading of]*ra-ke*[could possibly be instead]*je-ke*[which would fit with the common formula of the Sa series in Room 7 of *wo-ka we-je-ke*.²⁰ This seems a more plausible reconstruction of this label and that it was intended to group the tablets relating to the disposition of 20 pairs of serviceable chariot wheels belonging to non-*heq^uetai*, and its location, along with fragments of this group lends further weight to this argument.

This set of records, along with the summary tablet Sa 793, provides us with the 31 pairs of serviceable wheels detailed in Sa 787. There is, however, a minor discrepancy that needs to be addressed in relation to this total. Sa 787 clearly includes an extra wheel in this sub-total (*MO* 1) and just as obviously this is missing from the sets that I have described. At first sight, this points towards a scribal error since the only record of an individual wheel is apparently on Sa-488 from Room 8 and with this single exception, the figures on Sa 787 can be shown to correlate with the individual records that we have. However, of all the Sa series tablets recovered from the AC, I have thus far avoided discussing the problematic tablet Sa

²⁰ Again, the latest transcriptions maintain the reading]*ra-ke*[but with a footnote to suggest a possible restoration of *to-]ra-ke* (Bennett *et al.* n.d.). Given the existence of Wa 732 in close proximity to the Sh series, this suggestion must be regarded as suspect.

1266, but it is this that I suggest holds the key to understanding the organization of this series.

The proposed reconstruction, at first sight, appears to leave no room for this lone tablet, for as I previously indicated, it seems not to integrate logically with the document organization. The first point to be made is that doubt has been cast upon the scribal identification of this text based not only upon a spelling incongruity, but also on the characteristics of the tablet itself (Palaima 1988: 92-93). This in itself is puzzling given the consistency of the scribal hand in the remainder of the series and the relatively formulaic nature of the inscriptions. The second is that although the text, in essence, relays the same information as the individual texts under Sa 843, namely the quantity of serviceable wheels attributed to a named individual, the format is entirely different. Not only are we presented with two lines of text but unusually it does not include the ROTA ideogram. Although the syntactic construction is akin to the individual tablets collated by Sa 843, in that the text begins with the name of an individual responsible for the wheel followed by the standard formula *wo-ka we-je-ke-e*, the subsequent formula ROTA+TE ZE is missing. Instead there is simply the number and, included on the line above, the variant spelling of *te-mi-de-we-te* as what is seemingly an afterthought.²¹

Furthermore, this is a palimpsest text which, tantalizingly, concerns the same general subject as the prior text but with some information apparently removed. Unfortunately, what can be discerned of the original contains significant unknown vocabulary and so is of marginal help in interpreting the later text, or establishing what motivated or necessitated the revision of this information. Crucially though, the ROTA ideogram does not appear in the original text either. I suggest that this restoration is incorrect and that this tablet did not concern pairs of wheels but in reality recorded a single wheel. This tablet certainly appears unique and potentially written by a scribe unfamiliar with their general format and perhaps also in a dynamic context. Indeed, it is noticeable that the entry on Sa 787 referring to this lone wheel, and the preceding total of paired wheels, is itself written over an erasure, suggestive of composition in an arena where information was being updated. With this interpretation the apparent discrepancy in the Sa 787 totals described above, is removed and we thus have a complete dossier, with no discernible errors in the figures involved. One further observation needs to be made, though, and that is that although Sa 1266 appears to mirror the documents collated under Sa 843, the total recorded on that tablet makes no mention of a single wheel.

²¹ The transcription of Sa 1266 includes [ROTA ZE], but this is simply a reconstruction based upon assumptions of standard textual formatting. Of particular concern is the assumption that the abbreviation ZE should be included that assumes wheels were always recorded in pairs. The totalling tablet Sa 787 recording a single wheel clearly demonstrates that such an assumption cannot be made.

It therefore seems likely that the 31 + 1 pairs recorded on Sa 787 were composed of 3 entities: the 20 pairs recorded under Sa 843; the 11 pairs recorded on Sa 793 and the lone wheel on Sa 1266.

A further clue to the peculiarities of this document may possibly exist in the oft-mentioned Sa 1313 found outside the AC in Room 98. This poorly preserved tablet (only the 'serviceable' adjective remains with the ideogram ROTA assumed to be present) has been the bedrock of many discussions about the movement of tablets within and around the palace (e.g. Palaima & Shelmerdine 1984: 84-85; Palaima & Wright 1985: 258; Palaima 1988: 93, 156; Kyriakidis 1996-1997 [1998]: 218, 225). Its presence outside of the AC has not only been used to suggest that the Sa series was originally written in Room 98 and that scribal activities were not confined to the AC, but also that the evidence for storage in baskets points to the transportation within these of groups of tablets to the complex. The location is clearly at variance with the rest of the Sa series, but so too are the physical characteristics including a different clay composition and much smaller size. Superficially, it is difficult to see any association with the other tablets. Nonetheless, I suggest that Sa 1313 and 1266 are potentially associated. Sa 1266 is itself unique with the apparent modification of an earlier text, the use of two lines instead of the more usual one line and the subject itself.

It has been suggested that Sa 1313 is in some way a preparatory text (Kyriakidis 1996-1997 [1998]: 218, n. 45) and to an extent I would agree that this is possible, in that this and Sa 1266 are essentially the same document. The rather extensive palimpsest text on Sa 1266 may indicate that if Sa 1313 was the original, that it would have been of insufficient size to carry the full inscription and was therefore discarded. If this is the case, and the characteristics of the preserved texts do not preclude this, then we might surmise that the single wheel was a late inclusion in the 'inventory', requiring an amendment to the totalling tablet and a tablet to be written that did not conform to the usual format. Unfortunately the find-spot evidence is itself problematic in understanding this process since Sa 1266 was itself recovered from the Chasm, although we may surmise from other Sa tablets found here that it did originate from Room 7. As we have seen, the texts from here form coherent groups in terms of the numbers involved, with the exception of this single wheel, whilst the totalling tablet from Room 8 has a substantial lacuna creating some doubt as to the true total originally recorded. This possibility would seem to take into account all of the relevant information that we possess, including the find-spots, document preservation, general subject matter, the details of syntactic construction, and the totalling figures available to us.

What then are we to make of the divisions of tablets into these 'subsets' and between the two rooms? In general, we can see that the Sa series records work in progress in the refurbishment of sets of wheels under the heading of Sa 787. The majority of the entries refer only to the serviceability or otherwise of these sets, but 12 sets are described as belonging, or allocated, to the *heq^uetai* of which six pairs remain unserviceable. Six pairs, belonging to the *heq^uetai*, however are recorded as being serviceable and these records along with a totalling tablet and a label were placed separately in Room 8. The analysis presented above clearly shows that the totalling tablet associated with these six pairs was originally intended to convey the same information that is on Sa 787 (i.e. that despite the lacuna it should read that 12 pairs of serviceable wheels belong to the *heq^uetai*). Therefore the division between the rooms merely reflects the stage of refurbishment and that half of this set of wheels is complete and allocated while the rest await completion and allocation. I suggest then, that the Sa series is complete in and of itself, and that the organization of the tablets between the two rooms possesses a coherent logic that reflects the situation at that particular instant of time. To illustrate this analysis more clearly, it is possible to construct a map of the interconnectivity of the tablet contents (figure III-4), in which this internal logic becomes apparent.

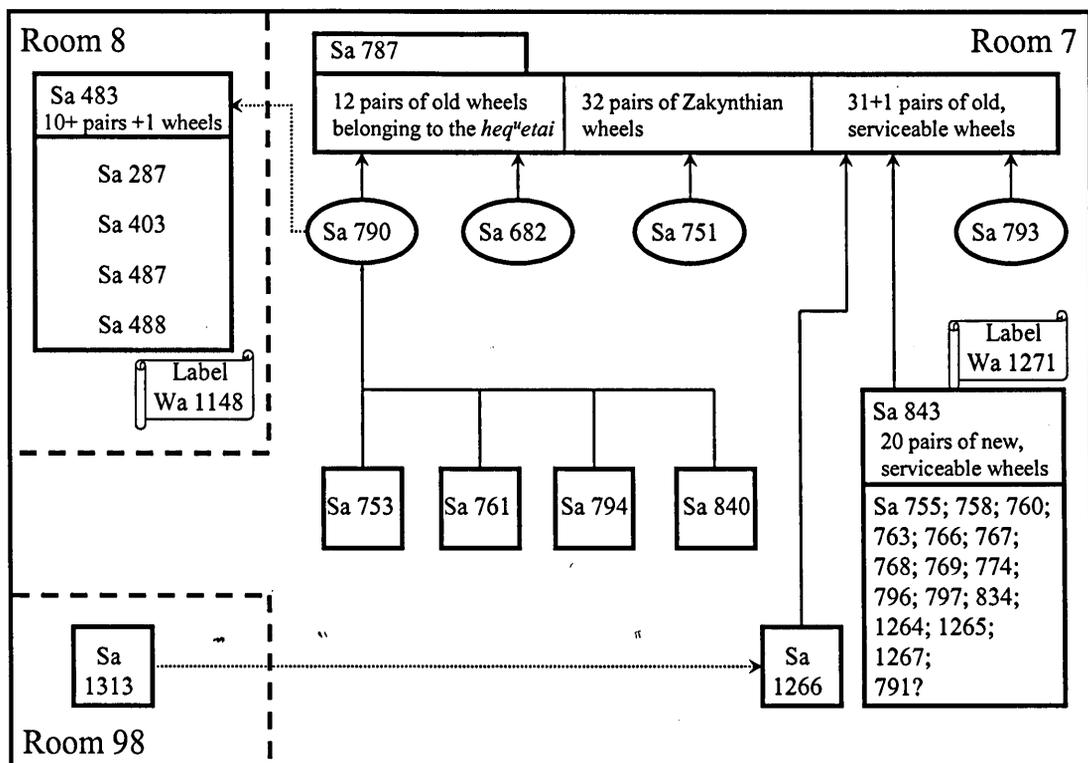


Figure III-4: A schematic illustration of the relationships between the tablets of the Sa series.

Can the proposed interpretation be verified in any way? If we return to the archaeological evidence, specifically the find-spots for this series, I suggest that confirmation can be found.

As discussed previously the Sa series had been sub-divided into three groups, A, B and C, based upon apparent thematic patterns but as we have already seen from the tablet distribution, there is little apparent spatial coherence in these groupings (figure III-3). However, from the preceding analysis and the reconstructed tablet relationship map it becomes clear that the current ordering of series subsets can be significantly modified. It would be possible to construct several variations of groups but given the relatively small number of tablets involved it is more desirable to limit the number of subsets (table III-3), despite the possibility that smaller files may have been in the process of preparation by the scribes. One further factor needs to be borne in mind however and that is the carrying capacity of individual baskets. As I discussed earlier in relation to the Ea and Sa series, the Sh series analysis shows that the basket capacity is relatively limited and so any modifications to the Sa series subsets must reflect a reasonable division of quantity as well as subject matter. With this in mind, it is likely that the large group of individual records collated by Sa 843 were kept together and that as they number 16 tablets in total, it is unlikely that they were kept with any other tablets other than Sa 843 itself. The tablets have therefore been grouped as follows: group A logically collates all of the tablets found in Room 8; group B the totalling and summary tablets of Room 8; and group C the individual records of sets of wheels defined by Sa 843.

Sa Tablet Group	Current Groupings	Modified Groupings
A	Sa 487, 753, 755, 758, 760, 763, 766 – 769, 774, 796, 797, 834, 1264 - 1267	Sa 287, 403, 483, 487, 488
B	Sa 287, 488, 682, 751, 761?, 790, 791, 793, 794	Sa 682, 751, 753, 761, 787, 790, 793, 794, 840, 1266
C	Sa 483, 787, 843	Sa 755, 758, 760, 763, 766 – 769, 774, 791, 796, 797, 834, 843, 1264, 1265, 1267
Omitted	Sa 22, 403, 1313	Sa 22, 1313

Table III-3: The proposed modifications to the Sa series tablet subsets.²²

If we now plot the distribution map according to these subsets a far more coherent pattern emerges, indicative not only of the validity of the individual groupings proposed above but also demonstrating that their physical location in the AC was most definitely ordered. Indeed, we can also suggest from this patterning, that the archaeological record is far less distorted by the destructions than previously thought (figure III-5).

²² A full summary of these groups and their associated find-spots appears in Appendix A.

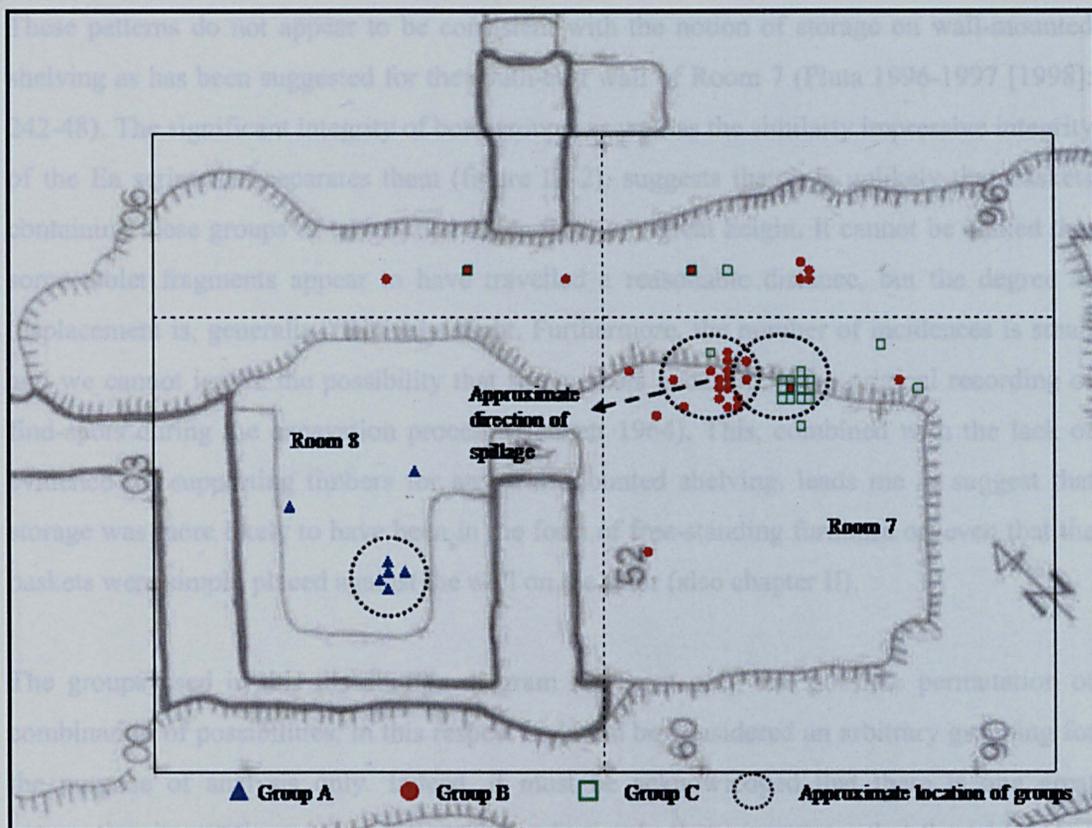


Figure III-5: The Sa series tablet distribution according to the modified subsets

There are clearly three discrete groups, excluding those spread across the Chasm. The pattern produced by these, though, is consistent with them having originally been located with the groups in Room 7 and their subsequent dispersal the result of the robbing activities disturbing the fallen tablets adjacent to the northeast wall. Moreover, the pattern itself supports the notion that the direction of robbing along this wall was from south to north (chapter II). All of the main groups have suffered a degree of displacement during the destruction of the AC, but the pattern suggests that essentially the groups in which they were placed are intact. Furthermore, the pattern of groups B and C clearly shows two distinct clusters. The area of division is approximately 0.4m in width and an examination of the tablet distribution shows that this is populated by tablets of the Ea series. The overlap between the two Sa series groups is therefore most likely the result of tablet dispersion during the destruction, but unfortunately the distribution data do not include relative height positions and so it is not possible to evaluate which tablets were laying on which. This would inevitably make the task of establishing how these groups were placed in relation to each other, or to other tablet sets and defining the direction of dispersal, easier. Nevertheless it is possible to see two broad directions of dispersal. Group C tablets have shifted in a broadly north-westerly direction whilst group B tablets have fallen more to the west.

These patterns do not appear to be consistent with the notion of storage on wall-mounted shelving as has been suggested for the south-east wall of Room 7 (Pluta 1996-1997 [1998]: 242-48). The significant integrity of both groups, as well as the similarly impressive integrity of the Ea series that separates them (figure III-2), suggests that it is unlikely that baskets containing these groups of tablets had fallen from any great height. It cannot be denied that some tablet fragments appear to have travelled a reasonable distance, but the degree of displacement is, generally, relatively slight. Furthermore, the number of incidences is small and we cannot ignore the possibility that some errors occurred in the original recording of find-spots during the excavation process (Bennett 1964). This, combined with the lack of evidence for supporting timbers for any wall-mounted shelving, leads me to suggest that storage was more likely to have been in the form of free-standing furniture or, even that the baskets were simply placed against the wall on the floor (also chapter II).

The groups used in this distribution diagram represent only one possible permutation or combination of possibilities; in this respect it should be considered an arbitrary grouping for the purpose of analysis only. Indeed, it must be acknowledged that there is one great assumption in creating such groups and that is simply that we assume that the tablets had been placed in the baskets in some meaningful order. However, as is discussed below, there are certain situations in which it can be envisaged that these baskets became mixed and as a result had no underlying organization according to subject. In the following illustration (figure III-6), I have attempted to show that this is a very real possibility in the case of the Sa series by re-organizing the make-up of groups B and C according to the find-spots of interconnecting fragments, whilst maintaining the logical restriction imposed by the carrying capacity of the baskets (table III-4).

Tablet Group	Groupings
A	Sa 287, 403, 483, 487, 488
B	Sa 682, 751, 774, 787, 790, 791, 793, 794, 796, 797, 840, 843, 1266
C	Sa 753, 755, 758, 760, 761, 763, 766 – 769, 834, 1264, 1265, 1267

Table III-4: Hypothetical groupings of the Sa series according to find-spots.

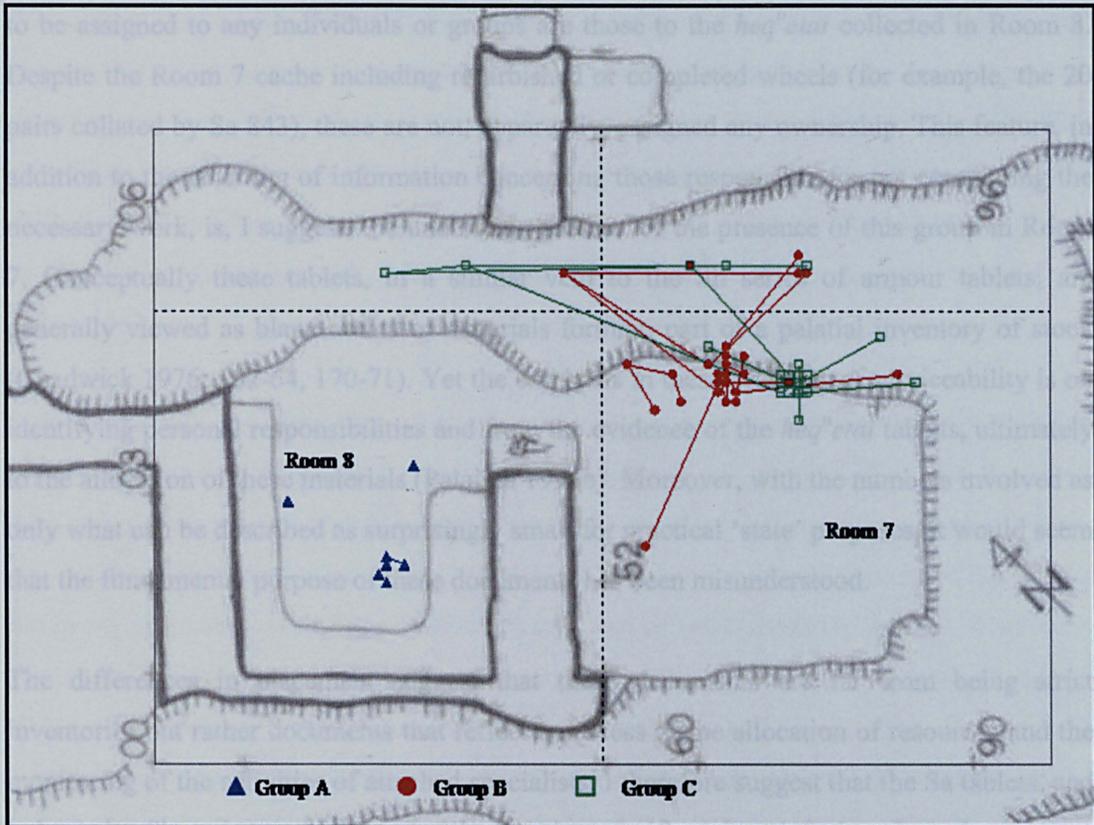


Figure III-6: The distribution of hypothetical groupings of Sa series tablets based upon find-spots.

With this arrangement, there is no inherent logic to the distribution based upon the contents of the tablets; the relationships are only defined by their respective find-spots. Yet the distribution pattern produced is at least feasible as a reconstruction of which tablets may have been placed together in the baskets. Certainly the quantities of tablets in each group are consistent with the figures suggested previously; 13 in group B and 14 in group C. So, although I have suggested above a pattern of containment that is feasible in terms of tablet location as well as according to textual relationships, this example does show that there is the possibility that the tablets were contained in no particular order. Unfortunately the disruption from the Chasm makes any further deductions extremely difficult and certainly there is no definitive means by which to distinguish between the two scenarios outlined. It is possible to suggest further subdivisions based upon both find-spots and textual content, but given the relatively small number of tablets involved it is doubtful that this would be helpful or, indeed, anymore convincing than the two already suggested.

The analysis of the texts clearly reveals that they were placed in these rooms in a precise and meaningful form. If the interpretation of the majority of personal names on the Sa series as those responsible for refurbishing or supplying the wheels is correct (Palaima 1996b: 40), then the distribution between the rooms reflects one other important factor: the only wheels

to be assigned to any individuals or groups are those to the *heq'etai* collected in Room 8. Despite the Room 7 cache including refurbished or completed wheels (for example, the 20 pairs collated by Sa 843), these are not, apparently, assigned any ownership. This feature, in addition to the collating of information concerning those responsible for not completing the necessary work, is, I suggest, the underlying motive for the presence of this group in Room 7. Conceptually these tablets, in a similar vein to the Sh series of armour tablets, are generally viewed as bland audits of materials forming part of a palatial inventory of stock (Chadwick 1976: 162-64, 170-71). Yet the emphasis in these accounts of serviceability is on identifying personal responsibilities and from the evidence of the *heq'etai* tablets, ultimately to the allocation of these materials (Palaima 1996b). Moreover, with the numbers involved as only what can be described as surprisingly small for practical 'state' purposes, it would seem that the fundamental purpose of these documents has been misunderstood.

The differences in placement suggest that these documents are far from being strict inventories but rather documents that reflect a process of the allocation of resources and the monitoring of the activities of attached specialists. I therefore suggest that the Sa tablets, and indeed the Sh series, attest to palatial activities of gift giving; but that they also serve a secondary, more subtle, socio-political purpose through the emphasis on craft responsibilities. By recording either the craftsmen or intermediary providers involved, the documents make a statement about the activities of, and relationship between, personnel attached or patronized by the palace and the palatial authorities. This scenario will be explored more fully in the following chapters, but essentially this points towards a more active role for the tablets than has ever been previously suggested. It is the active nature of the documents, the recorded activities such as gift giving that they reflect, and perhaps most importantly individuals and groups that have a relationship to the palace that I suggest are visible in these so-called 'labels'.

Labels, prompts or reminders?

As far as the study of the labels is concerned, the emphasis to date has been simply on attempting to assign each to an extant series of tablets, a process which occasionally forgets that the groupings are modern constructions designed to facilitate the analysis of the texts within a logical framework of apparently common subjects. These series do not necessarily reflect 'emic' classifications used by Pylion scribes to organise an archive (Hooker 1980: 36), if indeed it is correct to view these documents as part of an archive. It is perhaps not surprising then that only a small number of these labels have been positively linked to groups of tablets, the remainder simply dismissed as old or outdated tags that had been discarded.

While their fragmentary nature is undoubtedly a major limitation to any interpretation, I suggest that attempts at establishing associations between labels and tablets and interpreting their function thus far have been theoretically flawed. When attempting to interpret the label classifications themselves it has often been an automatic assumption that the process of data collection mirrors data organization within an archive in as much as data has been gathered to fit with a passive (or static) bureaucratic documentation system formed around fixed categories.

It is clear from the preceding discussion that numerous incongruities exist within the previously proposed interpretative models. While the analysis, particularly of the Sa series, does reveal that in a few instances labels appear to correlate with specific groups of tablets and organize them in a way that it is both archaeologically and textually recognisable, it does not in any way reveal what this organization reflected or was intended to achieve. It certainly cannot be concluded that they define the ordering of the tablets for long-term storage or archiving, particularly when the nature of the inscriptions and their degree of specificity appears largely inadequate for the task of specific information retrieval. When we consider the obvious grouping of the Sh series with label Wa 732 compared with the complex organization and grouping of the Sa series and the associated labels Wa 1148 and 1271, we are confronted with a model of a labelling and administration that is inherently contradictory. For while the analysis of the Sa series appears at first glance to suggest that the underlying motivation was to provide for the labelling of archival files, in reality closer inspection reveals that although there is an inherent logic in the separation of the subsets, they are obviously temporary divisions. It is only the supposed context of an AC that suggests these divisions were archival in nature.

If we are to view labels as organizational devices we inevitably have to face the question of why so few were found in close proximity to groups of tablets. Even if we were to suppose that the relative disorder in Room 8 was indicative of an interrupted process of file or dossier reclassification, we should nevertheless expect to find more labels than are actually present. It could be argued that the paucity of labels reflects an ongoing process of re-organization and reclassification but then we might expect to find considerably more in Room 7, and more particularly labels which have greater affinity with tablet groups in Room 8. Overall however, we are actually faced with an exceedingly small number throughout, a number considerably less than the identifiable sets of documents, particularly when the evidence from those we do possess which label very small groups of documents is taken into account. This general feature is all too easy to simply dismiss as being the result of preservation, but I suggest that far from being the result of loss or degradation the corpus that we possess

ultimately represents the reality of their use, a feature that the analysis of the Sa series fully supports. Their number was indeed small since they performed a precise, and ultimately transitory, purpose. Given these observations, we are faced with the inevitable conclusion that these objects were never intended to function as classificatory tools within an AC.

Likewise, if we are to understand labels as transportation or delivery tags then there are certain general features that need to be explained, particularly in relation to tablet groups where we would expect to find a delivery or transfer note. One such group is the cluster of tablets located in the Inner Propylon. As I demonstrated in chapter II, these tablets far from having inadvertently migrated from Room 8 have a coherency in find-spots indicative of deliberate placement, but no underlying common theme to explain their presence in this specific location. Yet neither amongst them nor within the entire area were labels found that could be associated through vocabulary. However, it is close analysis of those labels that have been promoted most strongly as positive evidence for their use as transport labels that fundamentally undermines this model; in particular, the cases of Wa 731 and the associated Un 718, Wa 732 and the Sh series, Wa 930 and the four tablets of the Cn series and finally, Wa 784 and the Ea series. Whether the issue that emerges concerns the unexplained disassociation of tablets and label (Wa 731), the provision of a label that is seemingly unnecessary or redundant (Wa 732), the retention of a label when additional tablet processing has occurred (Wa 930), or where there are insufficient labels, and insufficient detail on the label, to allow related tablet groups to be united (Wa 784), the evidence against this model is extremely strong. Combining these issues with the evidence of labels that have no obvious association beyond a single tablet we are inevitably faced with the conclusion that current models of label function obscure a more complex pattern of use. Yet, if they do not represent labels in the strictest sense, what function did they perform?

There are several features which I suggest provide clues to the motivation behind their use. The first of these that appears to be common to all is their transitory nature; both the crudity of production and the nature of the inscriptions clearly show this. The second is that the vocabulary appears carefully chosen to match the contents of specific documents whether that is single or multiple documents and, where labels can be identified as associating with specific groups of tablets or entire series, the label bears a keyword that can be found on those documents, but not necessarily the word (or words) that define the underlying purpose of the documents. It is also noticeable that, with the exception of Wa 732, the scribes deliberately chose a method of inscription that was not necessarily the most obvious or efficient. By-and-large ideographic classification was avoided and in doing so the scribes chose to use a system of labelling that was not necessarily the most recognisable or the most

efficient. This is particularly surprising in the case of the labels associated with the Sa and Sh series and suggests that there was a need envisaged for the words to be presented in their full, syllabic form.

There is little doubt that the labels were once attached to wicker surfaces, most likely baskets used to contain sets of tablets. The precise context in which this combination was functionally significant is, however, far from clear. The preceding analysis strongly suggests that the answer is not to be found in the AC but in a dynamic context where issues were being addressed that are not necessarily recognizable in our current categorizations of document sets. In the previous chapter, I suggested that the reconstruction of the AC was fundamentally incorrect in that Room 7 was directly accessible from Courts 58 and 59 and that the evidence of the animal bones, kylikes and pithos implied both access from these areas and that a feasting event had occurred prior to the destructions. In such a context, the labels and the peculiar nature of the tablets that they appear to be associated with becomes more understandable, if they too are considered as a part of the event represented by the other artefacts. It is no longer surprising to witness only a few tablets (or even a single tablet) referenced by a label, when that label is in effect a reflection of a particular topic or issue to be addressed at a public gathering. Indeed, in this respect they could even be regarded as reflecting the agenda of the gathering. In this scenario, the careful selection of vocabulary on the labels may even be understood on the basis of an auditory prompt to present, or address the issues contained within, groups of tablets at the appropriate juncture. Among this agenda, we can envisage specific sets of activities or issues being addressed: gift-giving (the Sh and perhaps the Sa series), the witnessing of debts, obligations and responsibilities (e.g. Wa 731 and Un 718, Wa 730, and the Sa/Sh series), the confirmation of ownerships and holdings, possibly with legal undertones (Wa 784 and the Ea series, Wa 930 and the Cn tablets) and issues of palatial patronage (Wa 114, 1008 and the Aa series, and possibly Wa 362 and An 607).

Significantly, I suggest that this prompt is concerned with either the direct verbal presentation of the contents of specific groups of documents or their use in the emphasizing of verbal performances. These tablets have typically been regarded as passive accounts of the palatial administration and the AC as simply the palaces' 'filing cabinet' from which various inventories could have been consulted as and when required. Although this is an extremely simplified view of current opinion it nevertheless reflects steadfastly held, fundamental beliefs concerning the purpose of the Linear B tablets in general and the Pylian archives in particular. Yet the nature of this 'administrative documentation' is such that Mycenaean bureaucracy appears naïve in the extreme in comparison to the complex earlier

bureaucratic institutions in the Near East such as that at Ebla (Pettinato 1981; Archi 2003) or those of Mesopotamia (e.g. Black & Tait 2000; Postgate 2001; Steinkeller 2003), but only if the notion of the Linear B tablets as passive record-keeping is maintained.

To accept, for example, the Sa and Sh series as inventories of palatial stocks of armour and chariot wheels, given the numbers involved, is untenable, but the analysis above offers an eminently feasible and attractive solution to this paradox. In a society that is dominated by oral communication rather than the written word, an assembly or performance in which the objects containing the written word are seen to be used would inherit a power and significance of considerable worth in such a socio-political environment. Combined with the act of gift-giving, the social consequences of such actions would be of considerable value to a political authority. Significantly, these tablets also contain information regarding individual and group relationships, obligations and responsibilities, which are reflected in many of the other cases discussed above. The broadcasting of such obligations and responsibilities to the palace in the context of a social gathering, such as at a feast, would incur considerable benefits upon the authorities through the strengthening of social ties and the advertisement of their power through benefaction and patronage, and ultimately redress for obligations not met.

This model would also seem to account for the apparent disorder between the placement of groups of tablets and labels within Room 7. Each of these discrete clusters would seem to be testimony to intermittent returns of document groups to the AC, particularly evident in the placement of the two baskets of the Sa series with the intervening baskets of the Ea series. With the former, we can also imagine a scenario where gifts of wheels are made to the *heq'etai* and those tablets recording the benefaction taken away to Room 8, whilst the remainder concerning craft responsibilities or potential allocations are retained until a later time. With that business concluded, these tablets are returned at a later time to Room 7. Indeed, this hypothesis has the benefit of explaining the one element of the conventional economic model of tablet use that has remained problematic and that is the issue of time-depth to the archives. In this chapter it has become clear that the labels served a very transitory purpose, but so too do the documents as a whole. There is little evidence that these records are concerned with anything other than the year in which they were produced and reasonable evidence to suggest that the period they cover is as little as 2 to 5 months (e.g. Palaima 1995a; 2003: 169-70). If, as I am suggesting, the importance of these documents lay not in record-keeping but in the use of writing as a tool in the manipulation of social and political relationships, then the short period represented by the majority of documents reflects a similar dynamism to the labels themselves. The majority of documents were

ultimately produced to fulfil precise, but dynamic, requirements in the manipulation and maintenance of palatial authority and power.

The model that I propose is undoubtedly controversial and would be entirely unjustified if it was to rest on the evidence presented in this chapter alone, and indeed there are several interconnected themes which need to be explored in greater depth before this can be considered as anything more than speculation. In the following chapter these issues, including further evidence for the oral performance of the tablets, the nature of the Pylian administration and palatial concerns with patronage and the maintenance of social boundaries, are analysed more fully.

Chapter IV

Patronage in an oral context: Rethinking the role of the tablets.

In presenting a review of the progress of Mycenaean studies Palaima (2003b: 66) expressed the opinion that ‘...interpreters of Linear B tablets have acted – and I hope will continue to act – as sober police officers curbing the tendencies of anthropological and archaeological theorists to speed ahead of the limits of available textual data or even to overlook the implications of closely nuanced interpretations of the details in the texts...’. But what are the limits of the textual data and can such nuanced interpretations be considered meaningful if the basic model of textual use is uncertain or unproven? Indeed, Palaima’s opinion succinctly presents the basic assumptions facing Mycenaean studies for it reflects the prevailing scholarly view that the fundamental questions posed by the discovery of the tablets have been solved and, that from a theoretical perspective we have entered into a period of consolidation. Yet the evidence presented in previous chapters suggests that many of the long-held convictions concerning the AC and the role of the Linear B documents at Pylos are questionable and that the situation is far more complex than previously assumed. Reappraising the archaeological evidence demonstrates that in all likelihood the AC was not the focus for a bureaucratic regime, but simply a location for the fortuitous (for us) accumulation of tablets whose functional context appears, instead, to be the same as the many artefacts that were found alongside. The tablets were essentially ‘temporary residents’ of this suite of rooms, ideally situated between the three main feasting venues (Bendall 2004) that I suggest were among the true arenas in which the tablet purpose was manifested. These analyses showed how several basic assumptions have combined to produce a false image of the function of the AC, including the often forgotten characteristic of the tablets that they themselves are portable artefacts and that their presence in any one place cannot be taken as evidence of either their production in that location, their storage or demonstrate a primary role in the activities at that location.

I also suggested that the purpose of the tablets was not embedded in a conjectured bureaucratic archival requirement, but in part in the need for public dissemination or accumulation of the information contained therein. Indeed, the questions that arose from an analysis of the filing labels concerned the role of the Linear B documents in the social arena and whether, far from being passive administrative accounts, they were active devices in the

manipulation of socio-political relationships. Such questions inevitably draw together several interconnected strands of social studies, from concerns with identity, individual and group relationships to the politics of patronage. Each of these, I suggest, finds direct expression in the texts of the Linear B tablets and so in this chapter such concepts are examined in relation to the administrative system that the tablets are assumed to represent, in an effort to define more precisely the purpose and role of the documents and what they may be able to reveal about social relationships in the Pylian region.

Linear B as an administrative tool: ‘red tape’ or a ‘red herring’?

Even before the decipherment revealed the language of the texts as Greek, scholars were convinced by their general form that these were essentially economic documents. The repetitive itemizing of commodities, later to be a determining factor in classifying the tablets according to groups, the obvious brevity of many documents and the common occurrence of recognizable figures and quantities all suggested that these were analogous to inventories and accounts providing records of the economic activities of concern in the day-to-day running of the palaces. Such bureaucratic concerns were originally highlighted with respect to the Knossos tablets by Evans (1909: 36, 38-54; 1935: 694-736) and were seemingly vindicated following the decipherment. Initial studies of the deciphered texts focused considerable attention on the presumed economic basis with comparisons drawn between the apparent similarities of the Linear B tablets and the already well-known archives of the Ancient Near East. These main themes of administration and economics were rapidly encapsulated in two major syntheses of the tablets by Ventris and Chadwick (1956; also, Finley 1957) and later by Palmer (1963), which although widely differing in their interpretations of the texts both utilized thematic presentations of the documents reinforcing the concept of tablet groups as administrative dossiers (Chadwick 1968) – a scheme that was maintained in the subsequent revision of Ventris and Chadwick’s initial publication (Chadwick 1973). Over the subsequent decades of study the economic model has remained the bedrock of Mycenaean studies and, although the interpretation of the nature of the economy at large has undergone several, often substantial revisions (cf. Killen 1985; de Fidio 2001; Halstead 2004; Bennet 2007a), the basic character of the documents has never been questioned, thus fulfilling the prophesy of Sir Moses Finley (1957: 128) that, ‘...there is every likelihood that economic history will be one of the chief beneficiaries.’ Subsequent studies have continued this focus on a presumed economic purpose even developing recently into a discussion of Mycenaean fiscality (arguably a misleading term in itself), often using analogy with broadly contemporary Near Eastern documentary sources in attempting to

reconcile the problems inherent in established models (e.g. Voutsaki & Killen 2001; Perna 2004; 2006; Jasink 2006).

Writing in the Hittite world

Even without considering the specifics of the texts, the administrative and economic interpretation was, I suggest, inevitable. In many respects the development of writing, first in Minoan Crete and then Mycenaean Greece, has been viewed within an evolutionary scheme intimately connected with the emergence and development of the palatial centres themselves. As such, Linear B is generally considered within a framework that sees it as developing out of the Linear A and Hieroglyphic scripts of Protopalatial Crete, themselves considered to be derivatives of an early administrative system based upon a system of sealings and nodules that may have its origins in the Prepalatial period (e.g. Olivier 1986; Schoep 1999; 2002: 22-37; Whittaker 2005; Skelton 2008). Such interpretations, of course, parallel and complement models for the genesis of writing and the appearance of the first state societies in the Ancient Near East. From the earliest examples of the deliberate use of symbolic notation as a tallying device, to the beginnings of the use of clay tokens amongst early Neolithic farming communities, counting and the abstract representations of this concept, have come to be viewed as the symbolic precursor to the invention of pictographic and phonetic writing (e.g. Goody 1986; 1987: 3-56; Larsen 1988; Schmandt-Besserat 1996: 122; for a consideration of polygenesis in the context of the development of cuneiform, see Damerow 2006). This evolutionary approach is seductive but ultimately neither informative nor indeed proven. Indeed, one of the basic criticisms of Goody's analysis is that by examining three millennia of written tradition only the most generalized conclusions could be drawn (Larsen 1988: 177), and this view is borne out by even the most cursory comparison with contemporary writing systems.

Linear B does not stand alone as the only written evidence of the Mediterranean Bronze Age but is a chronologically late addition to the panoply of ancient scripts. Comparisons and analogies with the writing systems of both earlier and contemporary literate neighbours in the Mediterranean and Near East have often been seen as a justifiable means of approaching many interpretive problems. One contemporary example is the use of written records in neighbouring Hittite Anatolia (e.g. Deger-Jalkotzy 1972; Uchitel 2005). However whilst they are useful to a limited degree, such comparisons are often highly selective in nature and ultimately dangerous, especially when used to examine the social role and status of individuals and groups. Ostensibly, it is an eminently sensible approach; the documents are superficially similar in form and depositional contexts. Nonetheless their content is

indisputably different and contextually the use of writing in these areas is considerably more diverse. Even in the area of Hittite control, arguably the closest parallel to Mycenaean society chronologically and geographically, we find writing used in a wide variety of forms and contexts and for an extensive range of purposes. It would therefore be remarkable if the documentary sources were to be comparable in terms of the underlying motivation, reasoning, function and use. Yet these are precisely the comparisons that have been made from the outset of Linear B studies. I suggest these comparisons have prejudiced not only the interpretation and reading of specific texts, but also the overall interpretation of what these records are, how they function and what they represent. A full comparative study is beyond the confines of this thesis, but there are several salient points worth mentioning in relation to the comparative status of scripts, language and scribal organization. Even a superficial appraisal of writing in the Hittite world shows that the degree of sophistication in the use of the written language, the uses to which it was put, as well as the psychology of the written word – how it was perceived and understood – differed considerably.

Hittite scribal organization, for example, has been found to be comparatively complex, with the documents revealing a rigid internal hierarchy from the lowliest apprentice through to scribal masters. Combined with analyses of scribal handwriting attributions, the inclusion of the names of scribes at the end of documents along with occasional notes to their colleagues have enabled scholars to identify the progress of individual scribes through the professional hierarchy and to appreciate their role in the scribal organization (Bittel 1970: 16; Bryce 1998: 418-20; 2002: 56-71). Individually these were specialists, indeed a fully professional group, formally schooled in the multiple languages, scripts and practices central to their occupation. More significantly the organization apparent in the schooling of Hittite scribes, reflected in both the writing of the documents and the archival system, is complex and extensive. The profession was evidently specialized, highly placed and a long-term commitment. In contrast, despite attempts to identify scribes in the Pylian documents, in particular the suggested identification of *a-ko-so-ta* with Hand 1 (e.g. Kyriakidis 1996-1997 [1998]: 220-24; Bennet 2001), we cannot reliably name any one individual or follow their activities outside of the tablet production arena.²³ Indeed, it is questionable whether or not the authors of the Linear B tablets should even be referred to as scribes. Certainly they were schooled or trained in writing but there is no evidence to indicate that these skills were

²³ If the suggested identification of *a-ko-so-ta* with Hand 1 proves correct it would constitute a major difference between Pylian and Hittite scribes. There can be little doubt that the latter constitute a professional class whose activities appear to be confined solely to the scribal environment, whereas prosopographical studies reveal that *a-ko-so-ta* is a highly significant individual with considerable responsibilities extending beyond this potential scribal employment including the inspection of land, the distribution of goods and acting as one of the 'collectors' (Kyriakidis 1996-1997 [1998]: 220-24; Bennet 2001: 31; Nakassis 2006: 385-86).

employed on a full-time basis in tablet composition or the other scribal activities we can observe amongst the Hittites. While styles of handwriting betray the presence of considerable numbers of writers, there is no supporting evidence to suggest the existence of a professional group and no vocabulary relating to scribes or their activities has yet been identified. Neither can we observe any system that led to their training so that any attempt to determine whether their writing abilities developed as a direct result of their position within the political elite or whether their origins lay elsewhere is entirely speculative.

In stark comparison to Mycenaean Greece, definitive evidence also exists from material remains and documentary references for a wide variety of writing media in the Hittite world. Beyond the ubiquitous clay tablets, documentary and archaeological evidence demonstrates the widespread use of wood, undoubtedly in the form of folded writing boards (e.g. Symington 1991; Bryce 1998: 416; 2002: 69-70; Uchitel 2005: 55-56), and metals such as lead, silver and most famously a large bronze tablet (Otten 1998; also, Hawkins 1987; Bryce 2006, van den Hout 2007), materials arguably more appropriate to the scripts employed than in the Mycenaean case. Cuneiform is particularly suited to impressing in clay but, by contrast, the cursive Linear B script should logically be found more widely on media accepting ink and paint, such as is seen on inscribed stirrup jars. Linear B, however, is widely attested only on the medium of clay, predominantly in the form of clay tablets but also as sealings. Only tentative evidence exists for other writing media, such as the hinges in the AC at Pylos that suggest the existence of wax-covered diptychs (e.g. Shear 1998; Perna 2007). Two other objects are problematic: the bronze vessel from Shaft Grave IV in Grave Circle A at Mycenae and the inscribed pebble from Kafkania near Olympia (Arapogianni *et al.* 1999; Palaima 2002-2003; 2003c). The interpretation of the former in this respect is uncertain since it comprises a single sign of unknown form that may be Linear A or represent a transitional form of Linear B. The latter, although apparently inscribed in Linear B, remains controversial with a significant degree of scepticism as to its authenticity. However, two recent discoveries of stone inscriptions are worthy of consideration. Most recently, an inscribed stone block was found at Dimini on which are clearly visible the inscription *e-qe-qi* (Whitley *et al.* 2006: 73, fig. 116).

More controversial is the claim that a Linear B inscription was found on a lintel stone of a Mycenaean tholos tomb at Kazanaki near Volos (Whitley 2005: 59-61, fig. 103, 104). This apparently preserves seven symbols (*ka*) corresponding to the number of cremations in the tomb, but full publication must be awaited before any meaningful assessment can be made of this claim. Whilst the notion that the inscribed signs denote the occupants of the tomb may be valid, it is less certain that these signs are in reality Linear B; certainly there are no

obvious parallels that may explain why the single sign *ka* should be used for such a purpose, particularly when it is supposed that differences in the size of the inscribed symbols denotes occupants of different ages. Archaeologically, the evidence is similarly problematic: there are several phases of activity apparent within the tomb and relating the act of inscription to any one of these is virtually impossible. It would seem that the tomb was originally constructed for the interment of a significant individual in LH IIIA, but by LH IIIA2 this had been supplemented by the burial of seven individuals (one adult female, two adult males, one unsexed adult and three children). Even if the inscription could be shown to relate to these latter burials, it also appears that a separate burning of these individuals took place in LH IIIC subsequent to the destruction of the Mycenaean palaces. It is therefore entirely possible that these signs were inscribed at this later date which would consequently make this the latest attestable use of Linear B. However, even then, there is no evidence that the signs were inscribed with any knowledge of the script itself. Ultimately, this must remain a doubtful case, but it is nevertheless interesting that these latest discoveries have been made in the Thessalian region, well beyond the areas of greatest Linear B concentration and from an area where no tablets have yet been recovered.

Meagre as these examples are, we can no longer assume that Linear B was not inscribed on stone, but although it may be tempting to extrapolate from these that examples of the monumental use of Linear B are waiting to be found, they cannot be justifiably compared with the relatively extensive and overt use of monumental inscriptions by the Hittites. Indeed, the Hittite use of monumental writing highlights one of the greatest differences in the use of script and language between the regions, since for these a specific language and script was used. In this instance the language was Luwian and the script Hieroglyphic. Other than monumental inscription this is found only as graffiti and on personal seals, most notably those of the Hittite kings (Hawkins 1986; Bryce 1998: 423-24; van den Hout 2006). While the majority of these contain only a Hieroglyphic inscription the royal seals alone incorporated both of the Hittite scripts: a central Hieroglyphic inscription providing the name and title surrounded by a cuneiform version.

However, the relationships between scripts and language in the Hittite region are complex. Most widely attested of the two scripts is cuneiform, which alone was used to write the principal language of Hittite. Cuneiform was also used to write several other languages including Hattian, Hurrian, Luwian, Palaic and Sumerian as well as the recognised 'international' language of diplomacy Akkadian (Bryce 1998: 420-22; van den Hout 2006). By contrast, the Hieroglyphic script appears only to have been used for Luwian and, aside from seals, for monumental inscriptions, in direct contrast to earlier Mesopotamian

inscriptions that were executed in both cuneiform and Hieroglyphic. Conversely, the cuneiform script alone was used on clay tablets (Hawkins 1986: 365). Whether for internal consumption or the necessities of international diplomacy, Hittite scribes were evidently adept with many languages. Not only do the seals demonstrate the use of different scripts on the same object, but also tablets have been recovered containing bilingual texts (e.g. Süel & Soysal 2007), whilst other documents were drafted in one language and presented in another (Bryce 2006; van den Hout 2006: 234). Thus the Hittite use of script and language differs dramatically from that in Mycenaean Greece where only a single script is attested representing a single language. Even Akkadian the common language of diplomacy across the Mediterranean and Near East is missing, demonstrating the remarkable insularity of the Mycenaean documents. Indeed, not only is there a complete absence of diplomatic records, but this insularity extends to contact between the palatial centres, for which the evidence whether commercial or diplomatic is slight at best (cf. Postgate 2001).

The relative complexity of the Hittite system is further demonstrated by the complex relationship between the principal written language of Hittite and the widely spoken Luwian. Recently, van den Hout (2006: 234) suggested that by the 13th century BC Hittite society was essentially bilingual. Hittite remained the dominant language of the political ruling class and the official written language, but was nevertheless integrated with ‘an increasing Luwian speaking or increasingly Luwian-speaking population’. The complexity of this relationship in this respect is shown by a Hittite document that records two texts originating from Hieroglyphic Luwian inscriptions, one of which has been identified at Hattusa. To an extent, this complexity also highlights the inappropriateness of conventional categorizations of literary form – analytically, certain features that are attached to one form are ignored for another. Monumental writing, for example, undeniably requires consideration of the visual characteristics of the script. By this I refer not to considerations of visibility, but to the perception of the script by literate and non-literate alike. Literacy would appear in this case not to be a prerequisite or even necessarily a consideration, for the symbolism alone would convey the power and influence of the state and literate and non-literate alike would appreciate the political statement that such an inscription conveyed. Whether or not the script can actually be read, the context in which it appears provides numerous associations immediately evident to any observer, whether to the power of the elite, to the mysteries surrounding the religious heart of society or as a permanent symbol of the presence of the king. Such concepts may well be paralleled or replicated in the Linear B tablets as well. If, as I am hypothesizing, the documents found their true purpose in a public arena they could well have possessed as much of a visual statement as a monumental inscription, bearing the same connotations of power and mystery, with the added distinction of having been seen to be

written by those concerned. In this respect, van den Hout (*ibid*: 235) makes a crucial observation: ‘The message of a public inscription is often not so much its contents but simply its being public in a specific form: the form itself is an important part of the message.’ In the Hittite case, the choice of Hieroglyphic Luwian for monumental inscriptions is alleged to have been political expediency – to create the illusion of solidarity and social cohesion. By contrast the restrictive use of cuneiform on the royal seals alone emphasizes the position of Hittite as the language of power.

Comparisons then between Linear B and other writing systems are fraught with danger. Despite superficial similarities in the writing media, the organization behind document creation is undeniably divergent. Indeed, when we look at the relatively large numbers of writers responsible for producing what is in reality an extremely small number of surviving Linear B documents, we see no evidence behind them for the complex organization visible in the Hittite scribal arena or more generally in the Ancient Near East. Here I have focussed upon the Hittite scripts, languages, media and scribal organization, but the differences in the use of writing extend much further. Hittite writing was by no means restricted to administration, but included, amongst other types, laws, myths, prayers, treaties, medical texts, histories and general correspondence; categories that appear entirely absent from the Mycenaean world. Writing was not simply an economic tool but a complex communicative system that permeated many areas of social life. Crucially, while this makes comparisons between the two extremely difficult, the Hittite use of writing demonstrates that we should not be too hasty in concluding that early writing systems either focussed upon, or were developed for, the purposes of economic administration (cf. Postgate *et al.* 1995).

A case for rethinking the role of Linear B?

Despite the lack of evidence for a monetary or market economy (Killen 1985: 252), or for any system of comparison by which the relative worth of differing commodities could be established, the English equivalents of Linear B vocabulary retain an illusion of modernity that disguises the true nature of the Mycenaean economic landscape, if indeed the term ‘economic’ is strictly valid in this context (e.g. Halstead 2004: 191-92). Social information, whilst obviously embedded within the texts, is generally viewed as a by-product of the primary compositional motivation of recording facts necessary to the functioning of the palatial bureaucracy. The tablets are viewed simply as lists, inventories, records of redistribution, taxation and transaction, personnel deployments and land registries. Such interpretations of the tablet contents have, unsurprisingly, led scholars to variously describe or characterize them as ‘unpromising source material’ (Palaima & Shelmerdine 1984: 77),

'drab and lifeless' (Chadwick 1976: ix), 'mundane' and 'summary' (Bennet 1988: 509), 'minimalistic' (Palaima 2004a: 270), ' cursory' and occasionally 'poorly drafted' (Palaima 1984: 39). Functionally they are described simply as 'administrative' or 'bureaucratic' to the extent that 'the scope of these documents is limited to economic administration' (Shelmerdine 1999: 20), whilst the various caches discovered are 'fragmentary' and 'incomplete' and often explained with reference to issues of preservation (cf. Chadwick 1976; Bennet 1988: 509-10; Palaima 2003a). The documents as physical objects have also received unfavourable descriptions from the simple 'cumbersome' (Thomas 1984: 249) through to 'scrappy lumps of clay' (Postgate *et al.* 1995: 478). Even the script itself has been referred to as 'inadequate' with respect to its ability to record the Greek language (e.g. Chadwick 1973: 67; Hooker 1979; Shelmerdine 1985: 4) and that understanding the contents is made difficult by 'the ambiguous and defective nature of the syllabary' (Hooker 1995: 8; contra Schwink 1998-1999). Indeed, the general approach to studying these documents has arguably become somewhat dogmatic and mirroring these epithets, analyses are presented using a very familiar and repetitive vocabulary based upon pre-conceived notions as to the function of these documents. It is my contention that many of these statements are in need of serious critique and revision. Nevertheless, in combination, the effect that these seemingly obvious and innocuous statements have had is to create an enduring image of the tablets as the physical manifestation of a less than extraordinary bureaucratic system of bookkeeping and accounting but, crucially, based around modern, economic concepts and terms.

To add to this problem, while the terms 'bureaucratic' and 'administrative' collectively refer to officialdom and the management of business, they are nonetheless ambiguous and overburdened with modern connotations. So, whilst superficially they appear either individually or combined to represent an explanation of the tablets and their purpose, in reality these terms explain very little. This tendency to view the documents as essentially elements of a dull, routine Mycenaean 'civil service' (cf. Chadwick 1959) does, I suggest, place unnecessary constraints upon interpreting the textual and archaeological evidence. Indeed, it is unfortunate that this most basic interpretation has never been subjected to critical assessment; it has been given the implicit status of an indisputable fact. Formulated without precise knowledge of the contemporary motivation, conception and intent behind tablet production, this basic administrative model drives an expectation of content as well as an expectation of the types of information that we hope to extract from the tablets. However, it is not sufficient to simply apply a bipolar critique and question whether they were viewed at one extreme as 'mundane' or at the other as 'special' but to question the superficiality of the theoretical model that views these texts on the basis of their material appearance and nature.

In other words, the texts are viewed as a documentary record, albeit cursory, but one in which their nature and underlying motivation are directly conceived in their material being. We see the written word and make the assumption that this is the critical aspect of these objects, but it is how the written word was used or conceived as being used that should provide the foundation for analysis. We should not be misled by the form in which the written word appears. For although the tablets for the most part do contain only lists, superficially supporting the economic and administrative interpretations applied, it is nevertheless a significant leap of extrapolation to conclude that such apparently mundane statements must be purely bureaucratic in nature. A list, after all, is simply an abbreviated form of writing providing a succinct presentation of ordered data which, as Goody (1977: 80) suggested, can be understood broadly in three ways: as retrospective inventories, future requirements (the 'shopping list') and the lexical list (an 'inventory of concepts'). Although these categorizations are perhaps overly simplistic, the documents superficially conform exclusively to the first two categories with the major emphasis upon the first and examples of lexical lists entirely absent. Despite the crudity of these categories they nevertheless demonstrate the variability and range of functions and meanings this apparently simple literary format or tool can convey. Yet studies of Linear B have singularly failed to ask the broader question posed by Goody (*ibid*: 74-111) of 'what's in a list?' The function of these lists has been assumed rather than proven and their nature is such that many possibilities exist to explain the intent behind their production and how such information may have been used.

For instance, while this basic writing form appears far removed from the natural styles of oral communication, it does represent a common mode of reduction from the oral to the written (Goody 1977: 82) and as such we should not so easily dismiss the interface between these two modes of communication simply from the format of the written word. Mycenaean society was after all predominantly oral in terms of basic communication, business and history and yet rarely is the interaction between the written and the oral considered. Standard analyses of early writing systems tend to maintain a strict delineation between oral and written modes of communication and regard the latter largely in isolation. There must, however, exist a mid-point: a point of integration between the two. As I argue, the (extra)ordinary nature of these documents may well lie in the vocalization of the written word and just as significantly in overt acts of inscription in the context of oral performance, rather than simply in a passive role as written accounts. As such, I suggest that the existence of the text as a physical object is only a reflection of the greater significance of an oral

performance in which the composition or reading of documents emphasizes the obligatory nature of the relationships and transactions referred to within.

The subject of oral communication rarely receives attention in the archaeological literature. Even in the most recent studies, this has seldom been considered except from the perspective of information gathering and the possible identification of distinct dialects (e.g. Thomas 1984; Palaima 1998-1999; Jasink 2005; 2006). Within popular themes such as feasting too, where a wealth of new data and research has been generated covering a multitude of related issues, from the archaeology of the feasting arenas through material analyses of the equipment and provisioning, to the social importance of such events, it is largely ignored. Even at Pylos, where considerable progress has been made in understanding the social role of imagery in providing a backdrop to both music and poetry particularly in the context of feasting, the issue of simple verbal communication has been missing; the participants of these events remain uncomfortably silent (although see Davis & Bennet 1999; Bennet 2001: 33-35; 2004: 100-01; 2007b, for the start of such a dialogue). When we turn to the Linear B documents themselves, the relationship between the written and the oral is similarly overlooked as indeed is the venue in which the documents became functional objects. Although information gathering may be considered in this vein, the tablets as physical entities remain significant to scholars only for the written word.

In the preceding chapter I argued that the labelling system was not evidence of practices of document organization or archiving but rather served to identify particular groups of tablets whose contents were to be made a matter of public record. In other words, that the functional context of these tablets was not one of a static information archive but was, instead, a public event or ceremony held at the palace. It is my contention that this was one of the arenas in which the tablets gained their greatest significance through the presentation of their contents or the visible recording of pronouncements made. One of the fundamental questions I am posing therefore is whether the significance of these texts lay solely in the written word or whether they were simply a reflection of, or supporting mechanism to, an oral performance. It would be entirely wrong to imply that the tablets are in some way the poetic forerunners of Homer or that we should consider any such similar recitations to the later oral traditions, but nevertheless, the oral character of these documents cannot be dismissed out of hand. Linear B remains our only substantive evidence for literacy in Mycenaean society and there can be no doubt that the knowledge of its use was highly restricted. So, although from our perspective these documents provide a previously unparalleled opportunity for examining Mycenaean society, that society was nevertheless essentially oral in nature. Indeed, the accumulation of evidence presented thus far clearly suggests that the intention behind these

documents has been misunderstood and misrepresented. Far from being a static documentary archive, a 'palatial filing cabinet' of accounts; their 'raison d'être', I suggest, lies either in their publicly visible composition as the physical manifestation of an oral process of negotiation or in certain cases, such as in the provision of gifts, the recitation of their contents. The power of these documents lies not in the written word itself but the potential they possess for public performance and the reinforcement of ties of obligation and the projection of central power and authority. To fully comprehend these documents and the context in which they exist we must therefore attempt to define the relationship between them and the oral environment.

Exploring the written/oral interface

Several characteristics that demonstrate scribal inconsistencies combine to indicate that the purpose of these documents cannot simply be reduced to an ordered process of palatial bookkeeping. From the variability in tablet form, the characteristic inconsistency of spelling that has helped to identify individual scribal hands, the limited range of vocabulary despite the variety of topics addressed, through to the likely identification of dialects from the written word, are all surprising features in a regular, organized administrative system. Even the nature of the script itself, being essentially cursive is far less suited to the medium of clay and more to writing with inks or paints, as indeed is visible on examples of stirrup jars that bear brief inscriptions. However, other similar features provide compelling evidence that the fundamental purpose of these documents has been misunderstood and that the tablets function instead in what could be described as an 'interface between the written and the oral' (Goody 1987; Thomas 1989).

A consistent feature, for example, of the documents is their extreme brevity, the repetitive formulation of phrases, sentences and multiple entry documents, the regular use of abbreviations and even the deliberate omission of identifying vocabulary. While these general features regularly occur throughout the document corpus, there do not appear to be any underlying rules governing their use. Unless we are to assume that the scribes are making records purely for themselves, we would expect the shorthand form of syntactic construction to be formalized. A particularly noteworthy example of the deliberate abbreviation of content is provided by Cn 328. This document lists individual holdings, or responsibilities for flocks of sheep and goat of variable size at *a-ka-na-jo*. The first three lines begin with the place-name *a-ka-na-jo* followed by the shepherd's name and the nature of the holding. On Cn 328.5, however, the opening *a-ka-na-jo* has been reduced to the initial 'a' and on subsequent lines there is no trace of any inclusion of this place-name. Evidently

the writer made a conscious choice to omit *a-ka-na-jo* from the introductory point of each entry, but why? The initial 'a' on Cn 328.5 makes it reasonably certain that in an unabbreviated form the document would have included this on each line and as such these omissions must be considered as deliberate. It hardly seems credible to attribute this simply to the tedium of repetition and I suggest that this reflects a need on the part of the scribe to speed up the process of writing. It is inconceivable within the context of formalized and controlled production of palatial records in a 'scribal office' that this should have been necessary, and I therefore suggest that a choice such as this is evidence of tablet composition in a more fluid arena, where an increase in the speed of writing became a necessity.

Given the inconsistent use of such compositional techniques, if these documents represent 'account books' stored in the equivalent of a Mycenaean filing cabinet and kept for future consultation, who was the intended readership? The consensus view is that literacy in the Mycenaean world was extremely limited (e.g. Chadwick 1973: 109-10, 406; Palaima 2004a: 286) and the possession of what amounts to an extremely limited account of practices within a particular territory in a largely oral society seems entirely incongruous. There would seem to be few people that could actually make use of these documents, even fewer when the peculiarities of individual scribal characteristics are taken into account, and even then the level and extent of information recording is such that it would be of questionable worth in a society in which oral communication is the norm. Yet, it is also evident that considerable effort was expended on their production (cf. Bennet 2001: 28). This, I suggest, represents one of the biggest conundrums, made even more problematic when consideration is given to the timescale referred to, and the timeframe covered by the documents. With no evidence for the systematic retention of tablets beyond a single year, clay documents were clearly not written to provide an internal economic history to be accessible for future consultation, whether or not the argument for transference of the information onto another medium has any veracity.²⁴

²⁴ The model I am proposing here accords well with Killen's (1984a) assessment of the payments, debts and deficits recorded on the Ma series. Here, a plausible scenario was presented as to how the extended retention of data concerning the previous year's debts may have worked (*ibid*: 183-84), which remains entirely feasible within the model I am proposing. The major difference, given the oral context in which I suggest tablets were being composed, is that there is not necessarily any need to temporally separate 'payment' and 'assessment' records. Within an oral arena, we can envisage negotiations occurring (perhaps indicated by the exemptions on these tablets) in which assessments for certain areas are being determined for future payment, whilst others are being settled within that arena. Furthermore, as will become apparent in the discussion below, one of the possible objection that Killen (*ibid*: 185) identified in relation to his own hypothesis, namely that none of the tablets demonstrate the clearing of previous debts, appears entirely logical if documents relating to full compliance are being deliberately destroyed. In other words, it is only those documents recording a non-compliance of some description, or those that represent newly negotiated assessments for the future, that are retained.

I suggest that this debate is fundamentally distracting and misleading, for while cogent arguments can be made for and against the practical need for long-term records derived wholly or in part from the tablets (Chadwick 1959; Hooker 1979: 33-35; Driessen 1994-1995: 244; Palaima 2003a: 170-71), it is clear from the manner in which they were composed that they were not simply written as preparatory notes for 'higher-level' documents (Bennet 2001). It would, for example, have been eminently possible for the tablets to be written in a far more abbreviated form, more reminiscent of the Linear A tablets for example. Neither would there have been any particular need to maintain the ordered format so characteristic of the tablets. The existence of other document types is therefore an entirely separate issue; tablets were undoubtedly composed to serve a precise role in themselves, even though the possibility that the information was subsequently transcribed remains. Yet the very features that mark the tablets out as significant in and of themselves also present several problems in understanding how they would have been used and, in particular, they make the documents less than easy to read for anyone other than the scribes who composed them. In attempting to explain the absence of earlier records, it was suggested by Finley (1957: 132) that once the activities referred to by the texts were complete or that the necessary modifications had been completed, there was no longer any need to keep the tablets. However, these seemingly reasonable and innocuous observations, when considered in relation to the readership, create a fundamental contradiction. It would seem that the tablets were being produced for no readership at all; they were simply being written by the scribes in advance of various activities and requirements, checked and amended by the scribes and then destroyed. Put simply, why bother? If there is no audience, there is no need for documentation. We cannot explain this with recourse to records required by dedicated palatial administrators simply because we have no evidence that the tablet writers were dedicated to this task.

In discussing the many problems surrounding Tn 316, Palaima (1999: 438, 449-50) rightly pointed out that many of the issues that we find puzzling surrounding unknown vocabulary, spelling inconsistencies, formatting and the associations between various words and phrases would have been perfectly understandable to those who composed the texts. Yet it was also suggested that the likely audience for the text would have constituted the original author and possibly other scribes. Certainly, the less than perfect presentation evident in this document makes it difficult to imagine who may be reading these texts other than the scribes themselves, but then we are faced with the basic contradiction presented by the documents that there is apparently no audience beyond their authors. This dichotomy prompts the fundamental question of why was it necessary even to bother writing the tablets and, if they were the only readers, why take so much trouble over the presentation? Unless the

information contained within moves beyond the scribal framework, they have no obvious purpose. Only one other possibility remains and that is simply that in the longer term it was the act of writing this information in an oral context that was of greatest significance.

Checking and destroying records in an oral context

It would be a mistake to apply the interpretation of oral presentation to each and every tablet or dossier that we possess for, although this may be surmised for groups such as the Sa and Sh series, other documents suggest a two-way process of communication. The checked documents (table IV-1), particularly of the Cn series, have not only clearly been composed in several stages, where the basic statements of the number of animals from named individuals have been supplemented some time later with the addition of what have been described as checkmarks, but also are themselves evidently incomplete since not all entries have these marks. There appears to be no reason to doubt that they represent the confirmation of information, but this in itself poses several questions. Why indeed should these entries warrant a process of confirmation if the tablets were written from information gathered by the scribes themselves? Evidently certain transactions were monitored to the extent that compliance was noted with the addition of a checkmark, but what reasoning determined that these particular documents required checking compared to the majority that are not? In what context could the information be checked when the time between the original composition of the texts and the addition of the checkmarks while apparently significant in several instances is nevertheless unlikely to have been too substantial? If the scribes were controlling the process of confirmation by direct observation why should there be omissions from this checking? Finally, if the scribes were expecting to have to check the entries on these documents, why did they not provide for space in the formatting of the texts for such checking? Cn 131, for example, incorporates checkmarks in many different positions in relation to the respective entries, whilst on Jo 438 they appear largely on the right-hand edge.

Tablet	Scribe	Room Location	No of entries. (<i>recto/verso</i>)	No of entries checked	Checked on
Ab 585	S186-H21	8	?	1?	?
An 39	Ci, Ciii	8	12/10	9/0	?
An 594	S39v-Ciii, Ciii	8	4/1	3/0	?
Cn 131	S131-H1	8	25	24	Dry clay
Cn 155	S155-Ci	8 / Chasm	3	1	Dry clay
Cn 200	S155-Ci	8	2	1	Dry clay
Cn 328	S131-H1	8	14	12	Dry clay
Cn 436	S131-H1, Cii?	8	8	3 checked, 1 unchecked, 3 uncertain	Dry clay
Cn 437	S719-H1	8	4	1	Dry clay
Cn 485	S719-H1	8	10?	1	Dry clay
Cn 491	S131-H1	8	6	1	Dry clay
Cn 1059	S719-H1	8 / Chasm	5	1	Dry clay
Eb 339	H41	8	1	1	?
Fr 1255	Cii	71, 72 (doorway)	1+?	1	Dry Clay
Jo 438	Ci	8	27 (+2 <i>latus sinistrum</i>)	10 <i>latus dextrum</i> (+2 <i>latus sinistrum</i>)	Wet clay
Vn 851	H12	8 / Chasm	24+/3	22/0	Wet clay

Table IV-1: Pylia documents containing checkmarks.

With only 16 documents bearing these marks, there is no obvious reason why these in particular should have been singled out for this process over and above other groups of tablets concerned with contributions of goods or the deployment of personnel. There is no consistency in the scribes involved, subject matter or, significantly, in the directionality of the recorded commodity flow. They refer to people, livestock and commodities and detail both incomings and outgoings in respect of the palace, and in each of the respective 'dossiers' the checked examples represent only a fraction of those documents apparently forming each dossier. The majority derive from the Cn series where the checkmarks appear to be lightly incised sometime after the original entries had been made and the clay substantially dry. By contrast, those on Jo 438 and Vn 851 are deeply incised with little visible difference between these and the main entries, indicating that little or no time had passed between the original composition and the verification of the information (Palaima 1988: 75, 118). Crucially though, not all appear to have been composed or verified by the same scribe; whilst several tablets have been composed by Hand 1, one of the most

significant and productive scribes at Pylos, and subsequently checked by the same scribe, this is evidently not a universal practice amongst either the entirety of checked tablets, the Cn series in particular, or those located within the AC. Whether or not Hand 1 represents a senior scribe or supervisor of others (Palaima 1988; Kyriakidis 1996-1997 [1998]: 205-07, 220-24), there appears to be no one individual responsible for verifying and marking the recorded information. In the case of An 39 and 594, for example, multiple scribal hands appear to be attested.

Despite the damaged and fragmentary nature of many of the tablets it is clear that not all entries have been given a checkmark. Only Cn 131 approaches completion with all but the last entry checked. Although it appears that Eb 339, the only instance of a check on a land-holdings document, is a contradictory example, this single-entry tablet reflects only one element of the land-holding dispute outlined on the larger document Ep 704, which has received considerable attention as perhaps the only example of a document with legal overtones (Thomas 1984; Shelmerdine 1998: 92-93; Palaima 2004a: 288, 297-98; Jasink 2006: 41). In essence it records a dispute over the land-holdings of the priestess *e-ri-ta at pa-ki-ja-na* that appears to have been ongoing at the time of the destruction of the palace, of which one of the listed holdings is mirrored in Eb 339. Discussions of this dispute, however, rarely mention the parallel text Eb 339 and its checkmark. Yet this would appear to confirm that there is no dispute over the initial landholding claim – that this has been confirmed and registered as such. Not only do these two documents point to an ongoing issue but they are also highly suggestive of why certain documents received these marks and why no tablets with fully confirmed information are present. These suggest an oral context or environment in which information is being verified verbally directly with the individuals named in the documents or through official judgement and proclamation; the omissions from this procedure are a result of the absence of those individuals, the need for independent verification or the need to await future decisions.

Whether marks were made on wet or dry clay, tablets were evidently retained because not all of the transactions or decisions had taken place. These absences may also suggest that these documents fulfilled an instantaneous need. Short of re-writing the entire document, editing to include late information after the tablet had completely dried, although feasible, would seem overly problematic to be standard practice. Cn 328 is also particularly revealing in this respect. Close analysis has shown that the majority of lines do include a checkmark, but that they are extremely faint and likely to have been made after the tablet had dried (Bennett

1973: 78; Palaima 1988: 42). Yet there remains one line where a mark cannot be discerned.²⁵ It would seem then that the act of verification was sufficiently important for the entries to have been checked on dry clay but the timing of this in relation to the original composition is uncertain. Clearly no effort was made to moisten the tablet to aid in making the marks more visible which may also indicate that in the long term, their visibility was less important than the act of checking itself. Furthermore their placement is often inconsistent; on Cn 131 marks are squeezed into any available space alongside particular entries whether above or below the concluding numbers.

Chadwick's (1998-1999: 33) view with regard to Jo 438 that these marks 'do not appear to have any special significance' cannot therefore be supported. To the contrary, it would seem likely that they are highly significant for in themselves they at least demonstrate compliance with the obligation to supply amounts of gold. When we consider the characteristics of this tablet it reveals a process of composition far removed from the ordered environment of a 'scribal office'. The tablet itself is unusual in being of a relatively narrow page-shape form but remarkably long. Nevertheless, it proved inadequate for recording all of the information and the scribe had to utilize both edges to complete the document. The line ruling apparent here is also exceptionally irregular. The scored lines are rarely parallel and slant both up and down. The intervening writing areas vary considerably in width and in the case of Jo 438.21 tail off into an exceptionally narrow space. The scribe was however sufficiently aware of this problem that the third word, *ko-re-te*, was inscribed using tiny characters in order to maintain a columnar arrangement. Indeed, this columnar characteristic demonstrates that we cannot suggest that the writing is shabby or poorly executed, but it does reveal expedient decisions being made during composition. Yet despite this seemingly orderly approach to the textual layout, there is a noticeable inconsistency in the descriptions of the people involved. The common use of the titles *po-ro-ko-re-te* and *ko-re-te* demonstrates that the transactions relate to district officials, but their identification is highly variable. In some cases we are presented with a name alone, others provide a name and a title while a few have no personal name but simply a title in connection with a place. Finally, in two cases all that the scribe has provided is a place-name. Why should such variability of data exist in the compositional process? Whether quantities of gold were expected to be delivered from particular officials or the

²⁵ The issue of understanding the purpose of these checkmarks underlines a methodological problem in analysing the tablets themselves. There is a temptation in reconstructing the original texts to view the omission of checkmarks simply as a function either of preservation or of the condition of the tablet when the scribe attempted to check each entry. In the apparatus to the tablet transcriptions (Bennett *et al.* n.d.), such considerations are all too evident. With the previously discussed Cn 328, the apparatus to lines .6 and .9 includes a note to the effect that an x would be expected and that the omission of this check may be more to do with the inscribing of the marks after the tablets had dried and were therefore very faint. This is a dangerous supposition, and one that I suggest is entirely invalid.

palace was to gift certain individuals, this data should have been available to a scribe. This inconsistency would suggest instead that the environment in which the scribe wrote the tablet was fluid. There would seem to be little foreknowledge of the specific information to be included but rather an immediate response to the provision of information. As Palaima (1998-1999: 209) described this: ‘the lack of systematic arrangement in the listing of toponyms and official titles, appear to be the end result of some kind of *simultaneous recording*’.

Unfortunately, although this document clearly concerns the movement of variable quantities of gold between the palace and individuals, groups and places, it remains uncertain in which direction the gold was moving. Comparisons with Jn 829 do suggest that we should view it as comprising receipts by the palace but the number of individuals and places does not correspond with the notion of receipts from officials of the major centres of the two provinces as is seen on Jn 829. We cannot therefore dismiss entirely the possibility that these are gifts to local officials provided by the palace. Whichever is the case, the checkmarks reveal that not all transactions were completed and a promise of provision in several cases still remains. It would seem entirely likely, though, that with the information fully confirmed, the documents would effectively become redundant and be destroyed. Indeed, it is one of the curious features of the tablets generally that they seemingly have an extremely short lifespan that fulfilled a temporally specific need and it is these checked documents that I suggest indicate the reasoning behind this. They suggest upon compliance with obligations or verification of information, any need for retention was gone.

Given these observations, we are entirely justified in posing the question: what advantage did writing provide and was its role simply economic? The exclusion of so many themes of social and political life from the records cannot simply be reduced to issues of preservation or quirks of timing in relation to the administrative cycle and the destruction of the palace. Communication between neighbouring polities or the politics of international diplomacy, for example, do not stop simply because an end point in the administrative cycle has been reached, yet there are no indications of this type of record ever having been consigned to the tablets. Even in the case of the one ‘legal’ text known to us, no full resolution of the dispute is apparent. The tablet appears only to serve notice of claim and counter claim, with the added inference from the parallel text that a part of the dispute had been checked and agreed upon. Conventional studies present inconsistent models of the palatial use of writing. On the one hand, they argue for a complex administrative structure utilizing writing to track and manage the acquisition and distribution of commodities important to the palace and its operation. On the other, the numerous omissions and inconsistencies are often reduced to

considerations of preservation and the polarized debate on the existence or not of another layer of documentation now lost to us.

The issue of preservation is all too easy to fall back on as an explanation for interpretive problems, but is little more than excuse, since there is no way to demonstrate the extent of the problem. Indeed, while it is possible to surmise from the extant corpus that a proportion of documents are missing, to over-promote this problem may disguise the reality of this system of documentation. Certainly in reference to the basic question of why there are apparently documents missing, three basic answers are available:

- ❖ that they were not preserved in the archaeological record,
- ❖ that they were never written (this inevitably involves two further possibilities in that there may never have been any intention for such documents to be written or that the information relevant to missing documents was not yet available for composition to take place),
- ❖ Or, that they were deliberately destroyed.

The first two of these are both reliant upon negative evidence and while we may surmise that they play a role in conditioning the nature of the archive bequeathed to us, there is no means of demonstrating the extent of either problem. Surprisingly perhaps, the third alternative, the deliberate destruction of tablets, has received little consideration. Although the general paucity of earlier records has led to the conclusion that once the need for these records was gone the tablets were deliberately pulped, is this all that can be inferred? This conclusion certainly appears inevitable but it begs the question: at what stage were the clay records deemed to have fulfilled their purpose? Unlike the Hittite examples, where there is evidence for the continual renewal of clay records – documents were copied and recopied providing a form of historical record – no such evidence exists for the Linear B records.²⁶

²⁶ Originally only five tablets were identified as possibly belonging to an earlier writing tradition (e.g. Palaima 1988: 171-72; 2003a: 162-64). The find-spots appeared to confirm palaeographic observations of inconsistent handwriting styles. However recently, one (Xn 1449) was found to join with Vn 1339 leaving only four tablets (La 994, Ae 995, Xa 1419 and Xa 1420) as potentially earlier than the majority. Crucially, the identification of the tablet join reveals the problematic nature of conclusions drawn solely from palaeographic observation. Indeed, Ae 995 and Xa 1419 are now the sole survivors of Hand 91, whilst La 994 and Xa 1420 have been tentatively assigned to a unique class (iv). It remains to be seen how these attributions will stand up to further scrutiny and whether these too will be found to be contemporaneous with the majority. Furthermore, the latter class are so fragmentary that any palaeographic observations and interpretations must be regarded with extreme caution. Nevertheless this leaves only four potential candidates for an extended retention (accidental or otherwise) on site, if we assume that they are not rare survivors accidentally preserved from an earlier year.

The logical conclusion therefore is that at some point tablets were deliberately destroyed. There is certainly no current evidence to suggest that substantial numbers found their way to areas beyond the palace. Nor is there any indication that the preserved tablets constitute fresh copies of earlier documents in a process comparable to the Hittite scribes who regularly made copies and re-drafted documents. The timing of this must remain uncertain since the notion that records were produced on an annual administrative cycle remains unproven. Although analyses have suggested that the majority of tablets belong to a single year and perhaps refer to as little as two to five months of that year (Palaima 1995a; 2003a: 169-70), there is no means of establishing definitively whether they were produced over an extended period or rapidly for a particular purpose. The evidence I presented in the previous chapters would suggest the latter is more likely but without clear examples of tablets written over a range of times the search for a regular cycle of tablet production is little more than speculation. The question though is whether there was any significance to this act beyond the practical disposal of out-of-date records?

The evidence of the checked documents suggests that destruction may well have been a significant act in itself. With no evidence for the existence of documents in which all entries have associated verification marks suggests that these were retained only because outstanding issues remained to be resolved. Yet the marks themselves show that it was important to distinguish precisely which issues were outstanding, those checked having effectively become redundant. This scenario appears analogous to later Classical situations in which the deliberate obliteration of documents was a significant event and one that contradicts the entire notion of document archives (Thomas 1989: 51-54). Despite existing within a monetary economy, one system in particular merits comparison with the Linear B tablets. Documents concerning the recording of contracts and taxes levied by the Athenian *apodektai* were kept until such time as the expected payments were made. When a particular payment was made, and a debt settled, the document would be amended with the complete deletion of that specific debt and the document returned to storage until such time as the entire debt was repaid (*ibid*: 53-54). As such, documents were records of debt alone and the destruction of a particular record was a significant act both practically and symbolically; as Thomas (*ibid*: 54, my emphasis) describes this, ‘proof of repayment lay in the *absence* of written records, not more documents...’. I suggest that a similar situation can be plausibly argued to exist in relation to the Linear B records and that we cannot ignore the possibility that the public deletion of entries and destruction of tablets was a significant practical and

symbolic act.²⁷ Although an oral context for tablet production and usage provides the most parsimonious explanation of all the peculiarities of composition and potentially the destruction of tablets, it does not alone explain why the documents were required in the first place. The conventional economic administration model is, I have suggested, considerably flawed. The form and detail of these records provides no obvious, discernible advantage to the palace unless we reconsider the role of the tablets as relating not to general economic matters but to a very specific role and an underlying philosophy far removed from modern bureaucratic concerns.

Administering patronage?

I would argue that the dominant motif of the Linear B tablets is not one of economics but of relationships, be they social, political or religious. A substantial proportion not only of the vocabulary, but also of the content as a whole, is taken up with establishing identities, the relationships of those identities to the palace as an entity or to those controlling the palatial system, as well as to the obligations, duties and benefits assigned to those identities. The economics of transactions and redistribution so evident in the documents are embedded within the relationships of individuals and groups to the corporate entity that is the palace. To attribute these documents to simple bureaucracy or to the administration of the political manifestation of the palace is, I suggest, to ignore the complex dynamics of maintaining and manipulating the interrelationships of the central palatial authority with the wider, dispersed communities of the hinterlands that would have been required to produce the relative prosperity and stability so characteristic of the late palatial period. Nevertheless, it is one thing to propose a change in our understanding of these documents, but to establish the nature and purpose of these concerns we also have to determine what form of relationship is being addressed.

One of the major features of the groups categorized by the filing labels is the notion of individual and group responsibilities, particularly with regard to craft activities. Not only is it evidently incumbent on certain 'trades' to undertake repair and maintenance work as we have seen with the Sa and Sh series documents in the previous chapter, but the identifiers used are often specific to individuals. The details of these tablets reveal the specialized

²⁷ As John Bennet (pers. comm) pointed out, the pithos in Room 7 (chapter II) may have played a role in such an event. If this was a *temporary* receptacle for destroyed tablets, it may explain the odd example of Fn 867 in which one of the fragments of the tablet was found beneath the smashed remains of the pithos, alongside a small cluster of mixed subject tablets, whilst the others were recovered from the Inner Propylon. Furthermore, as was noted in the discussion of this vessel, there is a case for regarding this as a 'special' or symbolic object in its own right from the possible archaic nature of the fabric and form.

nature of the crafts involved and that the number of individuals concerned is markedly low. By being named, however, these craftsmen clearly possess some importance, for, as Palaima (2004b: 105) rightly observed, ‘... it is often forgotten that the very mention of an individual by personal name within Linear B palatial records is an indication of significant status. Any clear linkage to the power and prestige of the central palatial authority would have conferred distinction’. In these cases the linkages suggested by Palaima are clear: craftsmen are obliged to practice their trades in relation to the production and maintenance of items required by the palatial authorities. The additional implication that these objects are to be presented as gifts or favours to significant members of Mycenaean society from the palace authority further imparts a degree of trust and favour upon these named craftsmen. Such favour implies a relationship between the palaces and particular individuals based upon concepts of patronage.

Patronage

Patronage and related mechanisms of socio-political interaction have received little consideration in archaeological literature, with the notable exception of several studies of patron-client relationships under the Roman Republic and Empire (e.g. Saller 1982; Garnsey & Woolf 1989; Wallace-Hadrill 1989; Chow 1992). But beyond a few oblique references, it receives almost no attention in the field of prehistory generally or the Aegean Bronze Age in particular. Yet the historical and contemporary attestations of patronage in the Mediterranean are well known and have been widely studied particularly in Spain (e.g. Gilmore 1977), Italy (e.g. Boissevain 1966; Weingrod 1968; Galt 1974) and Greece (e.g. Campbell 1964; Millett 1989; Gallant 1989; 1991: 143-96; Marantzidis & Mavrommatis 1999). Patronage in many forms has played, and continues to play, a significant role in the social, political and economic life of this region.

The content of the Linear B tablets suggests that it may also have played a significant role in the Bronze Age Aegean. The most obvious and recognizable indication of patronage occurs in the many compound titles applied to individuals and groups, whether identified by name or by trade. Of these, probably the most well-known refer to three individuals described by their trade as the ‘king’s potter’, the ‘king’s fuller’ and the ‘king’s armourer’ (Carlier 1984: 68-72; Hooker 1987: 260; 1995: 13; Gregersen 1997: 44-46; Palaima 1997). Remarkably, despite the suggestion of trades ‘by royal appointment’ the concept of patronage has largely gone unnoticed, but is one that has the potential to explain a great many of the peculiarities surrounding the tablets, from the characteristics of syntactic construction through to apparent subject lacunae and even ultimately to the very purpose for which these documents were

composed. Perhaps most significantly, by concentrating on tools of social analysis rather than economics, the possibility exists to explain the preponderance of personal names and associated identifying traits that appear to be of such concern in the composition of the texts.

The majority of scholarly attention and theoretical discussion into this phenomenon has come from the fields of anthropology, sociology and political science, concentrating on historical societies and particularly contemporary communities. Defining the concept, though, has consistently proved problematic and while certain general characteristics do find cross-disciplinary agreement, a definitive statement characterizing patronage remains elusive. Indeed it is undoubtedly easier to establish the elements that typify such relationships than it is to provide an all-encompassing definition, despite the suggestion that distinct theoretical divisions exist. As Weingrod (1968: 380) observed, 'to the anthropologist patronage refers to a type of social relationship, while to the political scientist patronage is a feature of government'. In the latter case, the particular relationship referred to is effectively one of political machination and manoeuvring in which the standing of the patron can bring about advancement of position for the client or the provision of favours in return for political support and promotion. It is doubtful whether these conceptual divisions can be rigidly applied in archaeological studies, though, for patronage as a social phenomenon cuts across many boundaries; as Saller (1982: 1) observed, 'patronage is as difficult to define precisely as are other types of complex behaviour, because it shares characteristics with other categories of relations into which it merges'. Not unsurprisingly then, each discipline has developed its own definitions and emphases such that any consensus is highly generalized. Nonetheless, patronage describes a particular form of dyadic social relationship between a patron and a client generally characterized by three features (e.g. Boissevain 1966: 18; Scott 1977: 22-23; Saller 1982: 1; Chow 1992: 31-32; Lyon 2002: 7):

- ❖ Exchange and reciprocity. The fundamental basis of patron-client relations is that of exchange; both parties enter into partnership to gain certain advantages, which may ultimately be viewed as exchange for personal benefit. By virtue of their social role and position, a patron is able to provide resources, usually material, that a client is in need of and, in exchange, the client is expected to reciprocate. Reciprocity in such a situation is usually less likely to be material in nature and may include the provision of services and support and as such should not be confused with the parallel concepts of gift exchange and balanced reciprocity that have long been of interest in anthropological and archaeological literature.

- ❖ **Asymmetry.** A fundamental criterion of the exchange relationship is that it is unbalanced. Whilst the ideal of exchange may be balanced reciprocity, patronage is dependant upon the inability of the client to respond in kind to the favours of the patron; one party must have access to substantial resources relative to the other, although these need not be material in nature, but may be political, spiritual, or even psychological. Ultimately a patron requires that the client be locked into a cycle of asymmetrical exchange and hence continual obligation, which in many cases is extremely difficult for the client to break. This feature has tended to be reduced to a simplistic statement of power. Yet whilst power may reside overtly in the hands of the patron, the client too has a notable, if indirect, power. The degree of asymmetry can also be seen to have an affect upon the nature of the relationship in terms of whether it is essentially collaborative or exploitative (Scott 1977: 25) and is what distinguishes this form of relationship from that of friendship.

- ❖ **Personal and individual.** As a dyadic relationship patronage is most often associated with an arrangement between two individuals or occasionally among a group of individuals, but not applied universally through society. Moreover, the association is conducted directly between the two parties; the involvement of a mediating agent would create the closely related brokerage relationship. It has been suggested that contact between patron and client is also private (Kaufman 1974: 285), but although particular transactions or arrangements may be conducted in private, the existence of the relationship is not.

With these characteristics there is broad agreement, although none are without problems and the latter particularly so. To these basic criteria have been added several, more contentious elements as summarized by Chow (1992: 31-32):

- ❖ The association is voluntary, a multi-layered characteristic that encompasses the notion of a relationship that is entered into voluntarily through personal choice and one that can be exited or discarded in a similar manner.

- ❖ The relationship exists outside of formal legal structures. In this way, patronage is a legally unenforceable association with the conditions of each relationship being agreed by the parties involved.

- ❖ A long-term association. With the imbalance in power to mobilize resources, the nature of the exchange relationship will inevitably be long-standing. A client is unlikely to be able to reciprocate immediately in equivalent terms to the patron's original support creating strong and lengthy obligations.

- ❖ It is a vertical relationship. As with the previous characteristic, this is a function of the basic requirement that the patron must have the power of access to resources that the client needs, which usually entails them having a social standing significantly greater than the client. The personal nature of the relationship also works to exclude other patrons from having access to a client, restricting additional horizontal relationships. However, it is also worth noting that Chow (*ibid*: 32; also, Wolf 1966: 16-17) further suggests that, 'in times of crisis a client may have to serve his leader and become a member of a faction'.

Rather than strengthening the definition of patronage these emendations instead serve to illustrate the problems inherent in determining strict terms of classification (Kaufman 1974: 285, n. 3); for while patronage defines a particular form of social relationship, it is simply a mechanism through which individual agency operates to construct, maintain and manage particular relationships but at the same time, one that can only exist through the agency of individuals. These characteristics tend to create the impression of a self-sustaining and self-perpetuating phenomenon not reliant upon the motivations and actions of the individuals involved, and are essentially reductive in describing the complexity of social interactions. Indeed, the tendency is to ascribe systemic principles viewing patronage relationships as elements within a much larger web (Boissevain 1966; Abercrombie & Hill 1976; Saller 1982: 1-6; Johnson and Dandeker 1989; Lyon 2002: 7). This, whilst acknowledging that the individual parties are themselves part of a network of social relationships, associations and allegiances, nevertheless disguises the subtle behaviour and actions at an individual level that characterize, maintain and modify each relationship and as such basically contradicts many of the defining elements. Since a basic motif of patronage relationships is that they are particularized, agreed individually between patron and client, no two relationships can be expected to incorporate precisely the same characteristics. Treating patronage as a system disguises the basic feature that the wider network is the product of a series of individual relationships, of individual motivation, choice and action. Even were we to suppose a scenario in which patronage was a dominant political tool and considered a system of government this does not transpose into considering it as the amalgamation of identical patronal relationships. The systemic approach can only ever hope to examine generalized

trends and characteristics at the expense of the complexities of individual behaviour and of increasing the danger of creating an illusory political ethic of patronage in which it becomes the dominant motif of political relationships and ideology.

Scholars have long debated the types of society, and the conditions under which patronage can emerge, prosper, and become prevalent. Weingrod (1968: 381) suggested that such relationships emerge in state structures exhibiting particular characteristics of segmentation where authority is not fully centralized but disseminated, where significant vertical differentiation exists between the constituent elements of the state, and where the activities of the state are not extensive. Abercrombie and Hill (1976: 421) suggest that the phenomenon is widespread and extensive both contemporarily and historically but that the particular nature of agrarian society in which the protection afforded by patronage provides mitigation for a lack of security makes it particularly conducive. In addition, they emphasize social conditions in which the relative ability to access and mobilize resources is significantly divided. Such views are also promoted by Waterbury (1977: 336): 'One may posit that resort to patronage mechanisms will be the more pronounced where the weak are disproportionately weak, the strong disproportionately strong, and formal, alternative mechanisms for protecting citizens – laws, court systems, police, procedural rules of the game, etc. – remain embryonic, manipulable or perhaps imbued with little or no legitimacy'. Similarly, Gellner (1977: 6) posits that: '...it is plausible to suspect that patronage is only avoidable when relations are anonymous and specific, in a mass society; and that in an inevitably more intimate elite, where relations cannot be anonymous nor criteria universalistic, patronage must be endemic'. These suggest that the social and political environment of Mycenaean Greece would have been entirely suited to the emergence of such relationships. However, it is important to acknowledge that the majority of discussions into this phenomenon have occurred in relation to the Roman and later periods with market-based or monetary economies and complex socio-political landscapes with, for example, formalized legal structures and complex civil administration. These elements either did not exist, or cannot be shown to have had any significant place within Mycenaean society.

Historically, patronage studies have tended to present a bipolar debate concerning the underlying ideology; from the functionalist perspective of patronage offering one important survival strategy to the client, to the Marxist view that patronage is a mechanism for maintaining the position of the patronal class (e.g. Saller 1982: 37-38; Wallace-Hadrill 1989). Both positions accept that the relationships are founded upon a basic ethic of reciprocity and that they cross social divides but whereas the former views patronage as inherently cohesive and one that can be advantageously manipulated by potential clients, the

latter views it as creating instability and serving only the interests of the powerful. These essentially represent the two extremes of the debate and are, I suggest, both inherently problematic. Each posits only one knowledgeable and active participant in the relationship or at the least downplay the intentions and actions of the other. But as Wallace-Hadrill (1989: 72-73) observed, power for the patron lies in the ability not only to grant favours but also to withhold or refuse them. Combined with the ability to bestow patronage selectively, it becomes a potentially powerful mechanism of social control and power. Nevertheless, at the level of the state, endemic or widespread patronage does not necessarily equate to a mode of government but simply to one informal mechanism for manipulating administrative concerns and potentially serving as a means of reproducing socio-political power and of identifying and selecting personnel that will support the *status quo* (Saller 1982: 205; Wallace-Hadrill 1989: 72). Neither does the power differential, or indeed a patronal intention of instituting some form of social control or manipulation necessarily make this a tool of oppression or exploitation.

Yet although this form of relationship brings benefits to both patron and client as individuals, it may not be benign or beneficial to wider society (Scott 1977). One of the basic criteria by which patron-client relationships are measured is through the asymmetry of the association and while this can vary greatly, a high degree of asymmetry can provide the opportunity for exploitation. At the very least, in such situations, resources will be provided preferentially; some of those in need will receive aid but not all (Garnsey & Woolf 1989: 157). Similarly the power wielded by patrons is such that clients can become utterly dependent upon the relationship. Indeed, as Gallant (1989: 405) posits in the context of subsistence crises and the subsequent large-scale indebtedness of the client: 'such practices become iniquitous unless the community as a whole is able to exert effective political and moral pressure on its elite class. Nevertheless, the result of these patron/client ties was often chronic rural indebtedness, peasant expropriation, the formation of a widening gulf between the rich and the poor, and a concentration of power in the hands of the rich.' Moreover, although the relationship itself is individualistic, knowledge that patronage exists is universal in that the society at large knows that patronage is available; in this sense Gellner (1977: 3) was correct to describe it as an 'ethos'. With this knowledge though comes the potential for social division and conflict; there are elements of the community that have gained an advantage over others creating the potential for a degree of envy. Even without direct conflict, if patronage is seen as advantageous by the wider community it will create competition at that level to gain patronage. This potential can be exploited, as Gilsenan (1977: 182) suggests, where those in a position to provide patronage maintain ties with each other creating an environment in

which dependency is strengthened and patrons collectively are seen to bridge the structural gaps in society.

Ultimately then, patronage can be divisive and provide a mechanism for social fragmentation. It can, for example, act structurally to affect the normal hierarchy and stratification of social networks and as such become a dynamic function of individual and group identity. The potential presence of patronage and associated mechanisms forces us to move beyond the simple structuralist views of status and rank since they militate against a single linear progression from the lowest to the highest rank in society by introducing fluidity and flexibility. A craftsman, for example, who is patronized, will undoubtedly achieve greater prominence than one who is not, but with the possibility of the removal of patronage and conferment elsewhere this prominence is unstable. Conversely, whilst promoting social positions, such mechanisms may act to constrain the actions of those individuals; they become subject to the conditions of the relationship. Nevertheless, within the community of their nominal peers the conferment of patronage sets them apart; it changes the dynamic not only between the parties involved but also amongst their own peer groups. While for the patron, there is unlikely to be significant benefit from a single relationship, for the client the advantages gained can be socially significant.

To suggest then that the provision or search for patronage is an entirely voluntary action ignores the wider context in which it operates for both parties. A client may be obliged to accept patronage if it is offered in a social environment in which such a relationship is strongly embedded and advantageous within the community. In other situations, since patronage from the perspective of a client acts 'as a 'down-to-earth' insurance against uncertainty' providing a risk-buffering mechanism which through networks of such relationships can provide a degree of social storage against subsistence crises (Abercrombie & Hill 1976: 421; also, Gallant 1989; 1991: 159-68; Garnsey & Woolf 1989), in situations of great need an individual may be forced into accepting the role of a client simply to survive. Despite any personal misgivings or desire to avoid the inherent obligations involved, the existence of other social mechanisms including friendship, kinship allegiances and even community support are generally capable only of providing limited and immediate aid. (Gallant 1991: 159-68). Thus patronage can become a practical necessity in the latter context and a social necessity in the former.

Conversely, a potential patron may be forced into that role through personal political considerations and the influence of other patrons or simply from political expediency in managing internal affairs. Similarly, whilst each relationship in which patronage is the

mediating mechanism is individual, patronage can only exist within a knowledgeable community. Thus patronage is an agency driven mechanism operating at the level of individuals but contextualized and made meaningful in its application within a group. Without community knowledge of the possibility of patronage existing at the individual level for each member of that group, patronage becomes socially and politically impotent. It would seem likely too that it is this knowledge that enables the obligatory nature of the relationship to be maintained and how patrons can in part guarantee the fulfilment of that obligation.

Patronage then is a mechanism of possibilities, both positive and negative, for the patron and client and also for the society in which it exists. It can, for example, be a facilitator to communication allowing individuals outside of any formalized political institutions or structures to circumvent the limitations imposed by hierarchical distance and gain limited admittance to such circles through personal contact (Boissevain 1966: 29). Conversely it can be used to support the inaccessibility of a political centre by making communication available only to those within a closed circle of patronal connections. Thus communication between a centre and its periphery can be controlled by the imposition of intermediaries in the form of patrons or select clients. Patronage is both an enabling device utilized by individuals and groups perhaps strategically, but it also retains a passive voice. The very fact of its existence is understood consciously by people either through direct advertisement of its provision or indirectly through the effects that its employment results in.

Clearly the power of a patron can be wielded in many ways with the ultimate sanction of force being available to the most powerful, but in the context of everyday relations this is obviously not a practical solution. Conventional discussions include the notion of honour, particularly given that patronage usually exists outside of any legal structure but, although there is little doubt that honour played a significant role within some societies, it is nonetheless a nebulous concept. For honour requires community knowledge to become meaningful. It is the threat of social rejection, should it be made widely known that an individual lacks honour in their dealings with others, that reinforces the concept. It is this knowledge that I suggest lies at the heart of any social relationship and provides the leverage against which obligations are structured. Patrons are reliant upon this community knowledge reinforcing the ties binding the client within relationships of obligation. With outside knowledge that the possibility of patronage being conferred elsewhere exists, should the client fail to meet the standards or obligations imposed by the patron, the onus on maintaining the advantages of patronage is placed squarely on the shoulders of each respective client. In other words individual patronage ensures a certain degree of group

competition and even conflict amongst the group vying for individual recognition and patronage. Similarly, patronage can be a tool for the aggrandizement of a patron (Waterbury 1977: 332), emphasizing to both the wider community and their own peer group that they have the power and position necessary to undertake such responsibilities. This aggrandizement, however, is only possible through the clear and effective projection or advertisement of the existence of patronage, a feature that I suggest is often overlooked and that has significant implications with regard to the existence of Mycenaean patronage.

Paternalism

A closely related socio-political relationship often confused with patronage is that of paternalism. As with the former, substantial difficulties arise in defining the precise nature of paternalism. According to Abercrombie and Hill (1976: 413) it is defined as ‘primarily an economic institution concerned with the manner of organizing a productive unit and regulating relationships between subordinates and the owners of the means of production or their agents’. Yet as Goodell (1985: 252-53) points out the very term, related as it is to parental concerns with their children, carries implied connotations of the relative maturity of the participants and is reflected in the definition she adopts: ‘paternalism is interference with others’ autonomy justified by reasons referring exclusively to their welfare, good, happiness, needs, interests, or values’ (*ibid*: 247). Both studies attempted to provide distinguishing characteristics based upon their respective definitions, which, although appearing to differ markedly in terms of definition, characterize the relationship in much the same way (table IV-2).

	Paternalism	Patronage
<i>Form</i>	Economic institution	Non-economic institution
<i>Patron/Client Relationship</i>	Collective	Individualistic
<i>Organization</i>	Tends toward systematization and institutionalization	Interstitial
<i>Relationship Extent</i>	All encompassing (total involvement)	Activity specific (partial involvement)
<i>Benefited</i>	Provider benefits	Client benefits

Table IV-2: A comparative summary of the general defining characteristics of patronage and paternalism (adapted from Abercrombie & Hill 1976: 413-16).

The temptation is simply to view these as polar opposites along a continuum of social relationships (*ibid*: 252-53) but whether at the level of a paternalistic familial relationship or that of a state to its citizens, a paternal act is one that is essentially imposed by virtue of the extreme power differential and the notion that the provider ‘knows best’. Whereas patronage

operates through the principle of obligation to fulfil the benefits provided by a patron, the scale at which paternalism operates means that a beneficiary could never hope to reciprocate in kind. This relationship in terms of the 'benefits' provided is also effectively autonomous; unlike the client seeking patronage, the recipient of paternalism does not enter into an agreement or contract to reciprocate. Benefits accrue entirely at the behest, both in terms of timing and nature, of the provider; in effect paternalism is imposed. As such, the differences between the two cannot be reduced to simple scalar characteristics either in terms of provisions or those acting as benefactors since both individuals and corporate entities can be paternalistic. However, these two relationships also clearly illustrate the problems inherent in social studies that attempt to neatly categorize human behaviour and interrelationships for there are few neat boundaries between these two phenomena. Not only are there many overlaps in their underlying motivation and operation, but also these basic characteristics do not define mutually exclusive phenomena.

At the contextual scale of a state for example, one can function within the other and, as such, cannot be considered to simply sit poles apart on a single continuum of social relations. A state may indeed be essentially paternalistic, but that does not preclude the existence of individual relationships based upon patronage. Conversely, a political system in which patronage is endemic does not exclude the possibility of paternalism operating on a smaller scale, particularly within kinship groups. More recently, such concepts have been encapsulated and subsumed within the study of clientage relations amongst the gypsy population of Sofades, where the co-existence of more than one form of relationship, including paternalism, was demonstrated (Marantzidis & Mavrommatis 1999). Here these relationships work to reduce conflict between the gypsy and non-gypsy population, whilst maintaining competition between gypsy families to obtain patronage. Yet there also exists within the client community a hierarchy of relationships in which the effectively socially excluded lower caste cannot enter into relationships with more than one patron. Inevitably the result of this is to maintain and reinforce the position of those in positions of power and simultaneously emphasize the difference between the gypsy and non-gypsy groups (*ibid*: 453-54).

Although many of the defining characteristics of paternalism give the impression that it is altruistic this is clearly not the case. It is, like patronage, a subtle tool of social manipulation in which the provider ultimately benefits. The straightforward opposition of the beneficiaries of the two relationships proposed by Abercrombie and Hill is fundamentally misleading. While paternalism is initiated solely at the behest of the provider, patronage emerges initially to fulfill the needs of the recipient (otherwise there would be no need for such a relationship

to be entered into), but at a price. Hypothetically, a truly altruistic patron may choose to derive little benefit relative to the client and release them from the inherent obligations with a minimum of fuss, but then they would be acting rather as a friend than a patron. Therefore we should not be misled by this phenomenon and imagine that the client benefits most from this association. From an archaeological perspective, though, the clearest distinction is perhaps provided by the motivation and intent behind acts of beneficence: acts that are outwardly directed towards benefiting the entire community at large being generally paternalistic, whilst restrictive practices aimed at specific individuals or groups reflect more fully the elements of patronage. There is however little evidence from the documentary sources for paternalistic acts directed at wider society in general or for globally orientated policies of beneficence.

Nevertheless we cannot dismiss the possibility that paternalism was a tool employed by the palaces. The archaeological evidence for large-scale infrastructure projects, from the 'Cyclopean' walls of Mycenae and Tiryns, through to the monumental tholos tombs that appear central to the definition of social identities, may be direct indicators of this. Perhaps more significant are the major building projects that could be portrayed by the palaces more definitively as socially beneficial, such as the construction of road networks and the major hydraulic engineering projects from cisterns, dams and harbours that were evidently of considerable concern in the Pylian region (Zangger 1994; Zangger *et al.* 1997: 613-23, 626; Hope Simpson & Hagel 2006; Showleh 2007).²⁸ Such projects would inevitably have required a significant expenditure of materials but, more importantly, also time and labour which, although widely recognized, remains largely unexplained. What made communities come together in support of the political institutions in these labour intensive projects? It is difficult to imagine that the dedication required would have been obtained through coercion or from direct employment with associated remunerations. Instead, I suggest that the motivation or incentive for the wider community was provided through a mix of paternalism and patronage by the palace. These acts we can see reflected in both the secular and sacred interests shown by the palatial institution in the tablets.

Identifying patronage and paternalism in the Linear B texts

One of the many problems encountered in examining the concept of patronage in the pre-classical world is the scarcity of direct accounts of the phenomenon. Unfortunately, in a situation similar to that observed by Millett (1989: 1) in relation to patronage studies in

²⁸ It is important to note here that I am not suggesting that these projects were conceived of as beneficial to the wider communities, simply that it would have been possible for the palaces to justify certain projects as being of benefit and hence obtain the support required.

Classical Greece, none of the terminology related to patronage visible in Roman or Biblical sources exists in these documents. As such, we cannot follow the methodology adopted by Saller (1982) and analyze direct linguistic associations or search for vocabulary that reflects the modern terminology surrounding such relationships. Patronage however is not defined by any written language but by the social relationship in which it operates, and the criteria by which such relationships can be defined provide a model against which both the archaeological and the textual evidence can be analyzed. Indeed, if we consider the much generalized definition of 'an exchange relationship between men of unequal status' that Saller (1982: 8) adopts in exploring the language of patronage, then similar principles can be applied to the Linear B tablets. Identifying patronage requires an identification of the underlying elements that typify the relationship. In particular the three main characteristics of exchange, asymmetry and dyadic relationships are directly observable in the tablet contents.

However, no single element can demonstrate the functioning of patronage either at an individual or a community level. The exchange relationship, for example, is essentially negative in archaeological terms; there is no reciprocity of exchange that may be identified in the archaeological record through the movement of goods. Instead we have to identify, on the one hand, the movement of resources and, on the other, the imposition of long-term debt and the possible long-term 'payment' of that debt through the movement of goods or the provision of service. Similarly, the argument that a primary determining feature for the existence of patronage should be the ability to demonstrate the control or government of resources is an overstatement (Johnson & Dandeker 1989: 226). Such absolute government only really occurs in what could be described as totalitarian regimes. It is the ability to command access to resources that is most significant. In other words, although materials may be circulating naturally throughout society, it is the ability of a potential patron to either have easy or preferential access to such resources or to facilitate access that is most important. Of the other characteristics mentioned above if we were dependent solely upon the archaeological record without recourse to textual sources, the dyadic and personal nature of the relationships would be difficult to establish, as would the asymmetry of any relationship in terms of the vertical differentiation within society. Fortunately, the tablets provide us with substantial evidence for social, economic, and political interrelations in both the secular and spiritual spheres that are also relatively explicit in defining the nature of particular obligations, the role or status of individuals and groups involved, and the variety of material objects and services that are at the heart of the relationships. From these we can observe the operational elements of all the major characteristics that combine to make up patron-client relations as well as being able to examine the effects on the socio-political interactions

between the palace and the hinterlands. Indeed, there can be little doubt that patronage in its broadest sense existed in Mycenaean society, but simply demonstrating the existence of patronage, although a necessary first step, would only be of minimal interest as it is common in one form or another in many or most societies (Saller 1982: 3).

Whether documents pertain to individuals, groups or communities, the majority seemingly address issues with respect to the palatial institution or with senior members of that institution, including the *wanax*. Indeed, it is arguably a '*leitmotif*' of the tablets that they consistently reference individuals to another entity whether an individual, a corporate body or to deities. Individual titles, or descriptions of responsibilities demonstrate the fundamental tenet of asymmetry and some documents such as Er 312, apparently reflect this asymmetry directly in their format, in that the 'order of business' is entered in a hierarchical sequence beginning, in this case, with the *wanax* and moving down through the social structure. However, the clearest expression of asymmetry and dyadic relationships is found in the compound titles used to identify individuals and groups, in which clear associations are made between people of rank and subordinate entities. Although many such compound identifiers are utilized, it is those that distinguish relationships between the highest members of society that are of particular interest.

As has been established, three named individuals are identified by their craft and by an additional epithet associating them with the *wanax*, suggestive of trades 'by royal appointment'. Thus we apparently have the king's potter, the king's fuller and the king's armourer. Yet it is noticeable that such designations are few in number compared to the number of trades, work titles and exotic materials associated with the palace. When, for example, we consider the high degree of craftsmanship involved in the production of furniture for use in or by the palace that are inlaid with exotic materials such as *kyanos* and ivory listed in the Ta series, no equivalent 'royal' craft titles are found (cf. Bennet 2008). Similarly, we might expect to learn of 'royal' gold workers, jewellers and other specialized craft trades, yet no such titles exist. By contrast, though, the Theban tablet Of 36.1 refers to 'royal' seamstresses, an occupation that in the Pylian A-series has been conventionally described as menial labour for the attention of slaves.

The presence of a king's potter however, apparently contradicts the majority of the documentary evidence where the palatial interest in pottery production and consumption is minimal. Yet we are left in no doubt from the archaeological evidence that this was a significant Mycenaean industry (Knappett 2001; Whitelaw 2001).²⁹ Similar titles also exist that suggest associations between individuals/groups and the *lāwāgetās* and other senior officials, including the *telestai* and the *heq'etai*. But beyond the confines of the secular elite, such associations can also be seen to exist with religious officials and deities. Thus we must be aware that patronage need not be restricted to the secular sphere of Mycenaean society but may have included divine patronage, a feature that has indeed been of significance in more general studies of this phenomenon (e.g. Boissevain 1977; Chow 1992; Neyrey 2005). As will become apparent in subsequent chapters, many of these tablets clearly show similar features to the Sa and Sh series discussed previously. Within these relationships the tablets record both the obligations and debts of individuals and groups within such relationships, as well as their provisioning by those in positions of authority. In other words, the tablets routinely document the three major defining elements of patron-client relationships, despite their extreme abbreviation. As such I will argue that whilst paternalism may have been a strong socio-political mechanism employed by the palace in achieving specific goals, the tablets reveal that patronage was an essential tool for the palace in procuring the resources it required and for maintaining and controlling socio-political relationships with the wider, dispersed communities. The tablets themselves, I suggest, are therefore both a physical manifestation of the process by which such relationships were maintained and manipulated, but also were an active tool in the process itself.

Performance, patronage and social identity in the Pylos archives

The problem of how we can definitively prove that a document was written to be spoken, or that it was composed or modified from within an oral context is inherently problematic and I reluctantly admit that there can be no one conclusive indicator of such a function. However,

²⁹ This absence of ceramic production and consumption from the tablets as well as their obvious stylistic homogeneity has long been a topic of discussion, particularly as to the question of the level of palatial interest in the control of production. The concept of patronage, however, introduces a further possibility in approaching this question and of addressing the stylistic development of ceramics. Although somewhat speculative, it is possible that by having a potter patronized by the palace that not only is a particular style of production maintained (consequently restricting innovation), but also that a degree of indirect control is exercised over the industry. We can certainly envisage competition amongst potters to achieve such a position, given the obvious advantages and security that patronage brings. Of necessity, however, individuals would have to demonstrate their ability to produce goods that would satisfy the palace. It may be then, that this, and perhaps the desire to mimic palatial products for other consumers, had a limiting effect on stylistic innovation. From the perspective of the palace, patronage would enable it to satisfy its own requirements with little difficulty. The palace had no *need* to control production or to monitor the material itself only to ensure the effective acquisition of the product through the individual.

the accumulation of evidence including the manner in which the tablets were written, the obvious lacunae in subject matters, the clear emphasis on relationships of obligation and conferment, the attention to characterizing the identities of individuals and groups in these relationships, through to the problems of identifying any potential readership or consultees of such a written archive and more global considerations of the nature and extent of the tablet inventories across the Mycenaean world, all indicate a purpose that is far from straightforwardly bureaucratic. These are not 'the day-to-day records of the working of a vast administrative machine' (Chadwick 1959: 9). It is, however, the issue of the intended readership that I suggest provides the clinching argument. There is no denying the care with which many of the tablets were produced but, unlike other contemporary writing systems, there is no clear standardization of textual composition; even within identified dossiers such as the Sa series, not all texts conform to a standard format despite being composed by the same scribe. As is shown in the following chapter, the same can be said of the Jn series, where there is considerable evidence that the process of composition was in a far from controlled environment and must have been written within the context of an oral performance.

Within such a context the peculiarities of these documents are revealed as necessities. The disproportionate appearance of personal names becomes a logical requirement of readings that place relationships of obligation, patronage and allegiance within the public domain. This is not to suggest that all of the documents are used in the same manner. The evidence of the checked documents indicates that the public role of these documents is manifold. Clearly not all documents were produced for announcement or proclamation, but also for the public recording of the status of obligation and debt. As I proposed in the previous chapter, the Sh and Sa series appear to be primarily involved with gift giving. Yet the Sa series apparently performs two tasks simultaneously, with the primary purpose being the referencing of gifts but with a secondary purpose of reaffirming ties of obligation on the named craftsmen to produce equipment to the specification of the palace. I suggest then that these documents, rather than representing a passive tool of bureaucracy and administration, were in reality an active device in the operation of socio-political mechanisms defining the relationships between the palace and the general population in terms of both individuals and specialist groups. The presentation or recording of the conferment of patronage and the associated obligations within the tablets suggests that their function was far more symbolic than has previously been accepted. They were mnemonic aids to a politically motivated process of accruing allegiances and social commitment to the palatial authority as well as the material goods that the palace required to maintain its position. As such, the tablets form an essential

part of maintaining the network of social interaction, patronage and support vital to the stability of the elites.

These conclusions are in direct contrast to those of Whittaker (2005: 36) who claimed ‘...that Mycenaean society placed an emphasis on immediate oral performance which did not allow for the use of writing in symbolic and social contexts.’ It is my contention that this is precisely the role that writing, in the form of the Linear B tablets, played. The mobilization of restricted knowledge in a symbolic act (re)affirming ties of obligation was a powerful tool both for ensuring the effective operation of palatial or elite patronage and as a means of advertising the power, influence and magnanimity of the palatial institution. Not only does the public recital of such documents promote a positive image of those acting as patrons and advertise the benefits and advantages of becoming involved in such a partnership, it reinforces ties of obligation by involving the wider community. It becomes known through this process who the patrons and who the clients are, who is considered trustworthy, even honourable, and the consequences of behaving otherwise. As such, it is possible to argue that the tablets form part of a complex propaganda machine employed by the palace, utilizing an elaborate performance - a display in which patronage is expressed publicly promoting the positive nature of the state/king as well as reinforcing the nature of the obligation and debt. Unlike the imagery surrounding the wall-paintings, which is arguably aimed at creating a diplomatic message for external consumption (Peters in prep (a)), the tablets are undoubtedly for internal consumption. These documents are not wholly symbolic but also eminently practical and functional tools, but neither do they represent a wholly written tradition. They function at an interface between written and oral traditions. It is even possible to argue that, rather than simply existing in an oral-written interface, these tablets mediate a more complex, oral-aural-visual-written relationship between the political heart of the polity and the wider communities.

Furthermore, the existence of the checked documents indirectly suggests that we should not dismiss the notion of the deliberate destruction of tablets being utilized in similar ways. With no extant tablets containing all entries checked off, it would seem possible that similar documents once verified were destroyed. Deliberate destruction may well have been entirely practical in that the obligations recorded therein had been fulfilled and the document was consequently redundant, but later traditions recognise the power inherent in the written word, and the obliteration of that word could be held to be a highly significant and ritualized event in itself. Perhaps the greatest implication of this interpretation is the potential for examining the associations that emerged in relation to the use of this restricted ‘technology’ by the palatial elite. As will become apparent, these associations and the model of Linear B use that

I am proposing provide the basis for an alternative explanation of why with the disappearance of the Mycenaean political system at the end of the Bronze Age, writing too disappeared and was not replaced. We should not forget that, to the uninitiated, writing was often thought to have magical properties (e.g. Harbsmeier 1988; Whittaker 2005: 29), a feature that is most famously known from the Homeric reference to Bellerophon's tablet (Bellamy 1989). It is not inconceivable then, as analogies with writing in later periods suggest, that for the majority it would have been viewed with suspicion and not a little mistrust, with memory and the oral word considered to hold more significance (Thomas 1989: 36).

Chapter V

‘Forging identities’: a case study in the oral/written interface.

Found in and around the AC, the 20 tablets that constitute the Jn series bring together many of the issues discussed thus far.³⁰ Commonly understood to refer to the Pylian bronze-working industry, they concern three interrelated subjects: the allocation of a commodity, known by the logogram AES, to named smiths, the issuing of specific work responsibilities and finally the collection of AES from officials of the 16 major towns of the two provinces. The precise identification of the commodity AES remains problematic since it could refer either to copper or bronze, or even a combination (Smith 1992-1993: 172-75; Gillis 1997: 506-09; Nosch 2006: 162). Despite this, the traditional focus for analyses has been upon the evidence they provide for the bronze-working industry in particular, and modes of craft production more generally (Lejeune 1961; Lang 1966; Killen 1987; Uchitel 1990-1991; Smith 1992-1993; Gillis 1997; Dialismas 2001; Michailidou 2001). The first and most substantial group of documents utilize a highly formulaic composition in describing allocations. Documents begin with a statement of the area or town to which allocations refer, followed by the common phrase *ka-ke-we ta-ra-si-ja e-ko-te* introducing a list of named smiths to whom allocations were made. These are listed individually by name with sometimes variable amounts issued, or by a totalling line following the name list probably indicating a uniform distribution. Subsequent entries detail those who are designated as *a-ta-ra-si-jo* – individuals who are without an allocation. Finally, on tablets Jn 310 and 431, comes a section concerning Potnian smiths, introduced in the same manner as the smiths in the initial section and utilizing the same format, naming those who are *ta-ra-si-ja e-ko-te* and subsequently those who are *a-ta-ra-si-jo*.

All but two of the tablets (by Hand 21) were written by the prominent scribe identified as Hand 2, who is also responsible for the writing of tablets of the Ma, Ta and Fr series among others. Analysis of the work of this scribe revealed numerous similarities with the work of Hand 1, generally considered to be the most productive and important of the Pylian scribes. Hand 2 in a similar vein, was concerned with a wide variety of records and although the overall number of tablets produced is significantly less than those of Hand 1, this scribe was nevertheless clearly of some importance (Shelmerdine 1985: 71-81, 112-13; Palaima 1988:

³⁰ Full transcriptions and illustrations of these tablets are provided in Appendix B.

66-68; Kyriakidis 1996-1997 [1998]: 207). Yet the involvement of such an apparently experienced scribe did not prevent these tablets from displaying many features incongruous to a model of careful tablet production and information recording in the service of a palatial administration. On the one hand, the tablets appear to form a coherent administrative dossier detailing the distribution of AES allotments and work responsibilities to named smiths, apparently drawn up in the context of a scribal office. On the other, they present numerous unexpected and inconsistent physical and textual characteristics. Many documents are written on blanks created from the cutting and breaking of larger tablets; smudging, fingerprints and tablet deformations from handling are visible on several, and erasures and corrections are commonplace. This contrasts noticeably with the syntactic construction of the recorded information, which contains several repeated formulas consistent with the notion that the scribes were fully aware of the type of information that was to be recorded, and the ultimate purpose of the documents. This awareness, it has been suggested, may have derived in part from the use of draft, or preliminary texts since several tablets superficially appear to contain similar information, but these 'draft' documents also contain unique records of allocations.

As I will show in the following analysis such a conclusion is not supported by their content, compositional characteristics or by comparisons with the other tablets in this series. Indeed, the purpose of this analysis is to show that these and other features are symptomatic of a process of information gathering and recording far removed from the confines of the AC, in arenas of oral negotiation and the public granting of favours and patronage. The omission of any mention of products expected to be produced from the distributed raw materials illustrates that the significance of the *ta-ra-si-ja* allocations lay not in defining a monitorable system of industrial production, but imposing an obligation to reciprocate for favours granted. As such, the tablets do not represent administrative documents in the conventional sense but are a visible manifestation of palatial patronage and a powerful tool for demonstrating and enforcing these obligations. Moreover, it is the process of composition within these public arenas and the inherent fluidity of the situation that, I suggest, accounts for many of the apparent scribal inconsistencies and the general idiosyncrasies so evident in this series. With this in mind, the starting point for this analysis is the process of composition and, in particular, one of the most idiosyncratic and characteristic features of the Jn series – the cutting of large page-shape tablets to create individual documents.

Examining the process of composition

Even a cursory examination shows that the documents were all originally formed from large, page-shaped tablet blanks. A few were retained in this form but the majority of shorter documents appear on tablets cut from these larger originals. Analysis of the physical characteristics of these has enabled several 'pairs' of documents to be identified; documents written on tablets that were originally part of the same blank. However, the paired examples of Jn 389/415 and possibly also Jn 320/478 (with reservations, see Smith 1992-1993: 188, 220, n. 130) and Jn 750/832 present a distinct problem if we are to understand the composition of the tablets within the context of an ordered scribal office, with the scribes patiently transposing gathered information onto the tablets. Jn 389 and 415 are certainly complementary in purpose and present their information in comparable forms but are nevertheless distinct in subject matter, pertaining as they do to different places. Both originate however from the same tablet blank with, as Smith (*ibid*: 188) suggests, Jn 389 probably having been written first. The scribe evidently decided at a later time that the information concerning each region should be retained separately and so the tablet was severed along the rule line dividing the two texts. The most significant question though surrounds the timing of this separation. Why was the tablet not cut before the second set of information was recorded and the second tablet provided with a margin separating the first line from the cut? If Smith's (*ibid*: 187-88) observations concerning the attempts to modify the format of the text into a columnar arrangement are correct then it is at the least inconsistent, if not counter-intuitive, for the scribe not to apply similar concerns to the physical construction and layout of the tablet and to have become used to a procedure involving the production of separate documents from a single blank. At the very least, we might expect the scribe to have taken the time to separate the tablet prior to the writing of a second text rather than risk damaging both at a later stage.

A closer examination of the pattern of tablet cutting reveals that a standardized, or at least preferred, method was used by the scribes in separating the tablets, but one that was not applied to Jn 415 suggesting that this was written sometime later and in a less than controlled environment than previously thought. Generally tablets show a consistent pattern of having been cut at the base but left uncut at the top where, apart from what appears to be the shaving of surplus clay in a few cases, the rounded edges of the original tablet blank are preserved (table V-1). Less convincingly, but still markedly more common, is the practice of cutting the tablet between rule lines rather than along a line thus providing a thin margin between the edge of the tablet and the final rule line.

Condition	Complete Tablets		Separated Tablets			
			Top edge		Bottom edge	
	Tablets	Quantity	Tablets	Quantity	Tablets	Quantity
Uncut	Jn 310, 431, 706, 829	4	Jn 389, 410, 658, 692, 750, 845, 881, 937	8	Jn 415, 693	2
Shaved	Jn 601, 725	2	Jn 320, 478, 832	3		
Cut along rule line			Jn 415, 605	2	Jn 320, 389, 478, 845, 881	5
Cut between rule lines			Jn 693	1	Jn 410, 605, 658, 692, 750, 832	6
Broken					Jn 937	1

Table V-1: Methods of tablet separation amongst the Jn series.³¹

We can be less sure that this was part of the preferred method of separating documents as there appears to be an equal proportion cut between rule lines and on the line. However, if we recognize that two of those cut along a rule line (Jn 389 and 415) were separated after the two texts had already been written and the scribe therefore had no choice in the matter, then this method appears to be more prominent. Furthermore, the complete tablet Jn 431 was originally cut between rule lines (with, interestingly, a preceding blank line) before the scribe decided that the document was not in fact complete and repaired the scoring (Palaima 1998: 59; Smith 1992-1993: 189, n. 15). The tabulated entries above are therefore slightly misleading in this respect and it does, indeed, appear that the preferred method was to break tablets between lines leaving a margin of separation. Jn 389 and 415 are therefore significant as they reveal a pattern of composition substantially at variance with the normal practice, which tends to see tablets cut after a single text has been written and the remainder, if it is of a useable size, turned around so that the original base becomes the top of the new document.

Jn 431, which is exceptional in showing the deliberate mending of a deep scoring intended to allow for the separation of the two halves, provides some confirmation of this pattern. Significantly, the last two lines of the basal half of the tablet have been written over a substantial erasure originally encompassing both lines. With no evidence for any underlying text it seems entirely possible that the erasure is linked to the aborted attempt to separate the tablet. Following the pattern discussed above, if the tablet had then been cut, it is likely that

³¹ No distinction has been made between the two scribes since with only two examples by Hand 24, and one of those being a complete tablet, the additional data would be uninformative.

the basal half would have been rotated prior to the writing of the second text, but it is also this part of the tablet which would most likely have suffered some surface marking from the scribe grasping both ends of the tablet to snap the clay at the scoring point. Indeed, it has been noted that the first line is somewhat smudged (Smith 1992-1993: 228, n. 156; Bennett *et al.* nd), suggesting that the scribe had been grasping the tablet at both ends as if to break it. Since we can be reasonably certain that such separations usually occurred after at least one text had been written, the scribe would inevitably have had to take care in performing this procedure that the already inscribed surface was not overly damaged from handling. As such the majority of pressure brought to bear on breaking the clay would have been to the base, increasing the likelihood that this part of the tablet would have been appreciably smudged. Damage to areas intended for writing would therefore have required prior smoothing; a process that would resemble the erasure of unwanted text. Here it would appear that the scribe, realizing that more data needed to be recorded, repaired the separation cut and smoothed over areas damaged in this aborted process. However, this attempt apparently introduced a further concern, for the second half of the document concerning the *ka-ke-we-po-ti-ni-ja-we-jo*, although still referring to the same area of *a-pe-ke-e*, was introduced with a reinforcement of this fact by a reiteration of the place-name. No other Jn tablet contains such a reiteration suggesting that the scribe having repaired the impromptu scoring commenced the second half of the document using the standard introductory formula in order to forestall a potential problem should the repair fail. Such an apparently hasty procedure is also reflected in the ruling of the basal half, which is considerably more poorly executed than in the upper half, mirroring features on Jn 832.

A degree of confirmation for the cutting process can also be found on two pairs of tablets that have been suggested to have originally formed single tablet blanks. Jn 389 and 415 were clearly separated after both texts had been written, but in contrast to Jn 431 the tablets show little surface disturbance from handling. Undoubtedly the scribe had to take additional care when breaking the tablet because of the potential risk to the texts. The break is however somewhat ragged suggesting that the tablet was held along the edges and pulled apart from the cut rather than snapped along it. This pairing unsurprisingly then presents opposing characteristics to Jn 431, but the possible pairing of Jn 750 and 832 shows a much greater degree of similarity. Here, the break appears to have occurred before the writing of the second text, although I would disagree with Smith's (1992-1993: 199) suggestion that Jn 832 was the first text to be written. This was based on no more than the cramped nature of the signs on Jn 750.12, as if the scribe had to squeeze the remaining information onto the limited area remaining following the writing and separation of Jn 832. However, the following line is noticeably devoid of such concerns with the sign spacing inordinately large and beneath

this a substantial margin preceding the break. Furthermore, the ruling of this document is neat and well executed in comparison to that of Jn 832, which is contrastingly irregular and untidy; characteristics that are replicated in the writing itself, which on Jn 750 is extremely regular with individual signs (in particular the combination of the AES sign and metrical units) consistently utilizing the entire height of a ruled line. The signs on Jn 832, by contrast, are noticeably more variable in terms of height, spacing and form; a useful example in this respect is the variability in the form of *ka-ke-we* on Jn 832.1,4,,9,13 and, in particular, the form of the individual sign *ka* throughout. Indeed, in many instances characters extend over the ruling lines, a characteristic that rarely occurs on Jn 750. While it would be wrong to suggest that this is representative of carelessness on the part of the scribe, it does indicate that the writing took place more hurriedly than that on Jn 750.

When we compare the surface conditions of the two tablets there is again a clear differentiation. While Jn 750 is largely devoid of damage and erasures Jn 832 is, by contrast, significantly affected and I therefore suggest that it is more likely that Jn 750 was the first to be written. Jn 832 has clearly been handled excessively, with the edges squashed and deformed and the writing surface smudged and thumbbed. These features are precisely what we would expect to see if Jn 832 had been created and composed in relative haste and mirror closely the basal characteristics of Jn 431. The physical condition of Jn 832 shows that it was separated from Jn 750 with a degree of roughness that is not so apparent on many of the other tablets, including the pairing of Jn 389 and 415. The cut ends of both tablets are ragged and on Jn 750 the basal edge appears to have been trimmed of excess clay after the break occurred, suggesting that the condition of the cut cannot be dismissed as a function of poor preservation. All of these characteristics indicate that sometime following the writing of Jn 750 a further tablet blank was required by the scribe and, as a result, a quick separation of the basal half of this tablet was undertaken. Following the scoring of the line, the scribe evidently grasped the basal half in such a way that, during the process of breaking, both the edges and the upper surface were damaged. We may assume that, since the writing process itself was largely unaffected by this damage, the scribe only had to smooth over areas where the text would have been affected, thus giving the impression of several erasures.

These examples show that the scribe usually separated tablets before composing a second text but, apparently in haste, was unable to apply this principle to Jn 389 and 415. The many, and varied, physical characteristics summarized here reveal a less than systematic attention to the tablet form itself, but certain intentions are nevertheless clear. As is well established, the scribe utilized a basic formula in the textual composition that was based upon defining the place at which identified smiths were operating, proceeding through lists of those

allocated quantities of AES to others who were designated as *a-ta-ra-si-jo*. Documents were intended to be defined by this place and, as such, tablets detailing two or more places had to be separated. It would seem that separation usually occurred only after one document had been written, with the cut aligned between a rule line providing a margin between the already written text and the supplementary tablet. Ideally, the second tablet was then rotated with the basal curve of the tablet providing the top of the new document. None of this process however, appears to correlate with the idea of the regulated or regimented production of administrative documents in the controlled environment of a scribal office. Indeed given that many of the tablets appear to have been trimmed of excess clay, suggestive of a desire on the part of the scribe to produce as orderly a set of documents as possible, this characteristic poses a basic dichotomy in the standard models of document production. In particular, some fundamental, and seemingly naïve, questions arise relating to the choice of tablet blank. Why, for example, did the scribe simply not use, or create fresh tablet blanks for each document rather than attempting to fit two texts to a tablet? Moreover, why did the scribe simply not make use of individual tablet blanks produced to a scale more compatible with the intended information? It would then have been a simple task, when a document was complete, to cut and trim them to the required standard without risk to the inscribed texts or without having to sacrifice textual organization and content because a blank was insufficient in size to accommodate all of the necessary, or possible, information. In defence of these questions, there certainly does not appear to be any underlying requirement to produce tablets of a uniform size given that the cut tablets vary considerably and that we also possess complete, page-shape documents such as Jn 431 and 829. These measures suggest that, in the particular circumstances of production, tablet blanks were either at a premium or that pressures of time did not allow for the production of new blanks.

Only one answer appears to satisfy this conundrum and that is, simply, that the scribe did not have an extensive foreknowledge of the contents of these documents. The context in which these were apparently written was one in which the scribe had a stock of large page-shaped tablet blanks pre-prepared and some knowledge of the eventual contents as suggested by the use of a formulaic prose style. Yet it is equally clear that the scribe had no prior knowledge of the length of each document or even the overall number that were to be produced. As such, the scribe made use of the largest form of tablet blank and used the remainder of a tablet after a text had been written as the basis for future documents. In addition, the physical characteristics surrounding the separation of documents suggest that the scribe was often placed under considerable pressure to complete the recording and cutting processes rapidly. As will become apparent this pattern is similarly reflected in the organization of recorded

information and the idiosyncrasies of the writing, including the pattern of erasures and corrections.

The organization of information

Here, the detailed analysis of Smith (1992-1993) is invaluable in examining the physical characteristics of the compositional procedure and for highlighting problematic issues in relation to this process and apparent irregularities in the organization of the written word. Fundamental to this analysis was the proposed identification of organized groups of documents that could be related both chronologically and geographically (table V-2).

Group	Subset	Scribe	Tablets	Purpose
A	1	H2	Headed by Jn 601 Jn 310, 410	Allotments to smiths of the HP (southern) – Room 8
	2	H2	Headed by Jn 431 Jn 389, 415, 320, 478	Allotments to smiths of the HP (northern) – Room 8
B		H2	Jn 605, 692, 693, 725, 927 Headed by Jn 845	Allotment tablets – Room 7
C		H2	Jn 750, 832	Allotments to specialized smiths
D		H21	Jn 658, 706	Allotments to smiths by Hand 21
E		H2	Jn 829, 881	Collection tablets

Table V-2: The proposed categorization of document groups within the Jn series (Smith 1992-1993).³²

Several criteria were used in proposing these groups, from the subject matter (defining Groups C and E in particular), differences in scribal hands (Group D) through to the spatial arrangement of tablet clusters in the AC (defining the separation of the allotment tablets of Groups A and B). Combined with the allotment figures themselves, it was further suggested that the allotment tablets were separated according to differences in the material allocated and their geographical location within the Pylian territory. As will become apparent these interpretations are extremely problematic, but of immediate interest is the pattern of compositional characteristics that led to the subdivision of tablets in Group A and the proposed writing order of the tablets in both Groups A and B. At the heart of this observation

³² Although this summary draws directly upon Smith's (1992-1993) study, since publication new fragment joins and a degree of renumbering has occurred moderately affecting the reading of her analysis. The affected tablets were Jn 937, which has been subsumed within the newly reconstructed Jn 927 and Jn 413 now subsumed within Jn 410 (Bennett *et al.* n.d.). This table, and the subsequent analysis, incorporate all such amendments.

was the pattern of find-spots argued to fundamentally delineate the two groups (figure V-1). However, as I have previously argued, there is persuasive evidence for a more fluid use of the space in Rooms 7 and 8 and that the deposition of all tablets in this area appears to have been temporary. The rooms do not constitute a focus for daily scribal activities and the pattern I established for tablet groups such as the Sa and Sh series and, more generally, for the placement of groups in both rooms is reproduced for this series. So while I would agree that the identifiable clusters relate to the chronological placement of tablets within the room, this cannot be reasoned to be a function of the writing process alone. Indeed, the basic find-spot distribution used is problematic in and of itself, particularly with regard to Group B. These were found widely distributed across the floor of Room 7, but not only are they mixed with components of other groups but also specific tablets do not precisely fit with this proposed pattern. Jn 605 for example, was located in Room 8 close to the interconnecting doorway rather than amongst those of Room 7 and a few, including Jn 845, although likely to have originated from Room 7 were found exclusively in the Chasm. As was apparent in the analysis of the Sa series, it is unlikely that the destruction of the room affected the natural clustering of tablets to the degree necessary for this scenario to be correct. Nevertheless, it is the organization of the written data and the proposal that this revealed a particular sequence of tablet composition that is of immediate interest.

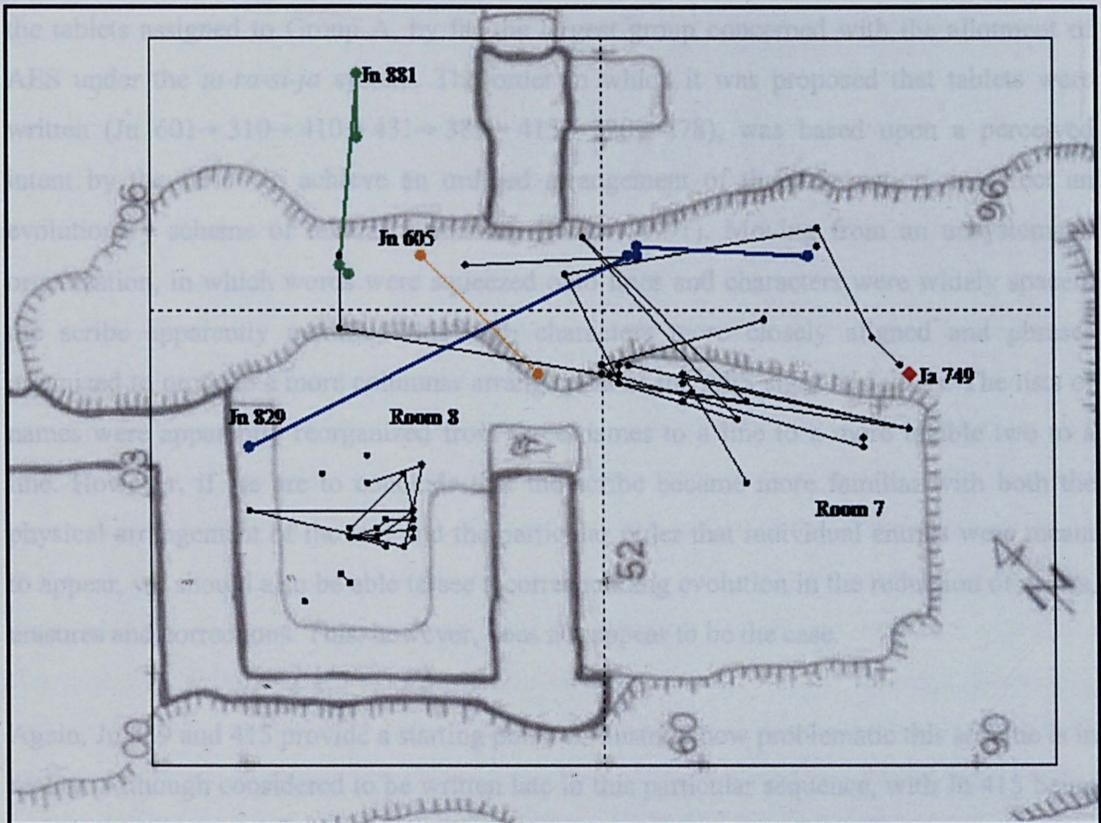


Figure V-1: The distribution of Jn series tablets in the Archives Complex.

Smith identified a series of compositional characteristics, including an apparent change in the number of entries written on a line and the seeming adoption of a columnar format, suggestive of an evolution in scribal writing technique. This, it was proposed, could be used to establish a writing sequence and hence reveal an underlying logic to the purpose of the documents. However, in doing so, a model was proposed that contained many inconsistencies and contradictions where tablets that possessed an ordered approach to information recording were placed in the same subset alongside others that were considerably less systematic in presentation, but also where the proposed writing order contradicted this evolutionary hypothesis. Jn 310, for example, which it was proposed was the second tablet to be written, not only possesses a degree of columnar formatting in the AES signs of lines 2-4 (contra Smith 1992-1993: 189) but is also notably limited in the number of erasures and corrections. By contrast Jn 431, thought to be the fourth in order of composition, is relatively chaotic, with several substantial erasures and, more significantly, the repaired cut intended to separate the tablet.

The issue of textual arrangement is undoubtedly significant, yet I suggest that the tablet and textual characteristics reveal a far more complex pattern of scribal motivations and actions that cannot be reduced to a straightforward sequence of compositional improvements and that, as a result, the conclusions drawn are not entirely correct. Here I shall concentrate on the tablets assigned to Group A, by far the largest group concerned with the allotment of AES under the *ta-ra-si-ja* system. The order in which it was proposed that tablets were written (Jn 601→310→410→431→389→415→320→478), was based upon a perceived intent by the scribe to achieve an ordered arrangement of the information, in effect an evolutionary scheme of textual formatting (*ibid*: 189-91). Moving from an unsystematic organization, in which words were squeezed onto lines and characters were widely spaced, the scribe apparently modified this with characters more closely aligned and phrases organized to produce a more columnar arrangement of the AES signs and totals. The lists of names were apparently reorganized from three names to a line to a more legible two to a line. However, if we are to conclude that the scribe became more familiar with both the physical arrangement of the text and the particular order that individual entries were meant to appear, we should also be able to see a corresponding evolution in the reduction of errors, erasures and corrections. This, however, does not appear to be the case.

Again, Jn 389 and 415 provide a starting point to illustrate how problematic this scheme is in reality. Although considered to be written late in this particular sequence, with Jn 415 being composed immediately following Jn 389, there is, in truth, no evidence to support the notion that these were consecutively written. To infer that because the two documents were derived

from the same tablet blank the scribe automatically followed from one to the next is merely an assumption, one in which I suggest the evidence shows otherwise. Not only are there the problems associated with the cutting of the tablet that suggest a late addition to the end of Jn 389 but also, as Smith (1992-1993: 190) herself noted, the organization of data on Jn 389 is somewhat less than that of Jn 415 with three names squeezed to a line rather than two. If we are to accept that the scribe progressively changed the formatting to something more satisfactory then in this particular regard the scribe moves between two names to a line on the second tablet, Jn 310, then back to three on Jn 431 and the subsequent Jn 389, before reverting to two again on Jn 415. This, alone, suggests that the text of Jn 415 was a much later addition to the tablet. It is also noticeable, however, that the ruling in the bottom half of the tablet is less parallel and shows considerable variation in the angles and width of the lines than in the upper half. In other words, it is unlikely that the tablet was carefully ruled completely prior to commencing writing but rulings were added when necessary; the apparently careless nature of the ruling is again suggestive of a hasty process. Combined with the evidence of the cut itself, where evidently every care was taken to preserve the final word on Jn 389.13 but that the condition of the remainder of that line mattered little, these characteristics strongly suggest a context in which speed was required in the writing of Jn 415.

It would seem then that the composition of the tablets occurred in a less than controlled environment. The substantial number of corrections indicates that the information cannot have been directly to hand prior to the composition of the tablets, since the scribe would have foreknowledge not only of how to organize information on the tablets but also of the details themselves. Whilst small errors are imaginable in any composition, the nature and extent of these cannot be explained simply by a 'slip of the stylus'. Neither can we imagine the information being dictated by another for transcription since once again, it would have been possible to establish beforehand the procedure by which the information would be conveyed. These characteristics instead suggest a degree of unavoidable haste in writing and, in the case of the cut tablets, a dynamic process where the scribe was occasionally obliged to make use of tablets already inscribed with one text and record a second, concerning a different area of the polity, before the documents could be separated. Similarly the suggestion of a natural evolution to the formatting of information must be considered doubtful within the context of a controlled environment. If we are to accept the writing sequence outlined previously, which I suggest cannot be considered wholly reliable, then it clearly took a substantial period of time before a columnar methodology was adopted by the scribe, despite the consistent use of a formulaic style in recording the different aspects of the allocations. Information was evidently being relayed or provided at a rate where this

experienced scribe was unable to establish the overall spatial requirements of the text or to have a mental picture of the optimum arrangement of individual entries. Moreover, corrections and alterations to the individual allotments (e.g. Jn 415.5, 601.3 and possibly Jn 605.2) are indicative of decisions in flux. Rather than previously fixed and known allocations being recorded, the negotiation of quantities appears to be occurring, as is also evident in the apparently incorrect totalling of certain documents or the modification of totals according to particular removals or inclusions.

Similar characteristics can be observed when considering the numerous erasures and the apparent re-ordering of names on many of the documents. Jn 389.3, for example, was observed to have the name *pi-we-ri-ja-ta* apparently written over an erasure corresponding to the subsequent name entered on the tablet. Similarly, on Jn 605.2 the initial two names were erased to include, instead, the name *to-ri-jo* at the head of the list, with the erased names subsequently re-entered. From this Smith (1992-1993: 191, 196) suggested that the order of the names was significant and that the scribe was obliged to erase the initial entry, replace it with the correct name and subsequently rewrite the original entry. No indication is given as to what this 'significance' may be, but I suggest that it would be entirely wrong to interpret these acts as an implicit statement of the relative importance of these individuals, or that the precise ordering of the names was in some way inherently important to the understanding of the document. On Jn 431 the initial entry concerns the smith *wi-ja-ni-jo*, allocated AES M5, who from prosopographical studies is unknown outside of this document (Nakassis 2006: 578). By contrast, the sixth entry refers to an allocation of AES M4 to *qe-ta-ko* who can be identified in other documents and appears to be a relatively prominent figure. This same document (Jn 431.11) shows that *qe-ta-ko* possesses a *do-e-ro*, whilst tablets of the Cn series (Cn 45 *lat inf*; Cn 570.3; Cn 600.14; Cn 1287.4) assign to him respectively the descriptions of being in possession of 11 boars, an unknown number of ewes, a further 30 boars and finally as providing one female goat in his role as a potter (*ibid*: 531-32).

It is of course dangerous to make any assumptions concerning the relative status of individuals based upon even the most detailed and critical prosopographical studies, but similar patterns can be discerned in other documents. For example, Jn 658.7 contains the entry for *o-tu-wo-we*, a smith allocated AES M5, who has been identified as the same individual listed in other tablets (An 261.2-5, v.7; Un 616 v.4) responsible for a *ke-ro-si-ja* composed of up to 14 men (*ibid*: 505). Conversely, preceding this name is the entry for *po-ro-u-jo* (Jn 658.5), and following is that of *pe-re-ta* (Jn 658.8); both of whom are unknown

outside of this and the parallel document Jn 725.³³ These cases do at least illustrate how certain smiths were known by the palace to hold responsibilities outside of their craft specialty. A measure of prosopographical visibility, whilst admittedly not conclusive, does suggest that the relative prominence of individual smiths varied greatly but this does not appear to be reflected in either the order in which names appear in the texts or the amounts allocated. Indeed, as we shall see below, this observation applies equally to those described as *a-ta-ra-si-jo*.

Whilst some motivation obviously existed for the scribe to deliberately amend the order of particular entries, it cannot be explained with recourse to simplified explanations of the relative rank of the smiths or the quantities of AES allocated. As with the many physical characteristics of the tablets and texts, including that provided by the cutting of the tablets, I suggest that the answer lies in a consideration of the context in which tablets were produced and the means by which information was obtained, gathered, or, as I suggest was actually the case, negotiated. Indeed, the question of the arena of composition must constitute the most fundamental issue to be answered, for the conventionally understood context of production within the AC does not explain why these errors and corrections should occur in relative abundance if the tablets are merely representative of the ordered accumulation of administrative data by skilled practitioners in a controlled environment. It seems more likely that the changing order of entries is related to the order in which information was presented to the scribe to record. Similarly, if this context is not as controlled as previously thought, the basic hypothesis of an evolution from disorganized to organized writing may be the reverse of what should be considered. It is just as feasible that textual organization was an ideal, undertaken where possible but was otherwise sacrificed for speed. As such, it is possible that the more disorganized documents are in fact the later ones written. The central question, therefore, is in what context did the writing of the tablets take place?

³³ One further example worth noting concerns the very document that was identified as being deliberately corrected by the scribe because of the significance of the order in which entries were meant to appear (Smith 1992-1993: 194). The corrected initial entry of Jn 605.2 placing *to-ri-jo* at the head of the list, for whom no other information can be discerned, installs him immediately ahead of the appreciably more prominent figure of *e-do-mo-ne-u* who has variously been identified as a smith (with two allocations), a herder of pigs, a landowner and, perhaps surprisingly, given the conventional interpretation of the title *do-e-ro*, a *te-o-jo do-e-ro* (Nakassis 2006: 423-24). He is then followed by two smiths for which further information is also available; a third smith (*u-wa-ta*) who cannot be identified elsewhere; and the fourth and final entry, *ka-ta-wa*, who owns a flock of 80 ewes in the name of the collector *a-ko-so-ta* (Cn 40.13).

Preliminary, working tablets or late additions of allocations?

Jn 693 and 725, which uncharacteristically contain records for more than one location, are generally considered to be preliminary or working tablets from which Hand 2 drew up, or was about to draw up, final version allocation documents (Chadwick 1973: 511; Lindgren 1973b: 62-63; Smith 1992-1993: 191, 194-97; Nakassis 2006: 161, 169). Superficially, they appear to contain information common to other tablets, including the only two written by Hand 21, Jn 658 and 706, as well as Jn 692 by Hand 2. It is the erasure of a substantial section from Jn 725 and the apparent replication of this section on Jn 692 that has driven the hypothesis that Jn 725 was a preliminary text from which Jn 692, considered to be the final version, was drawn. However, this hypothesis is not as straightforward as it first appears and considerable confusion exists across the published studies of these documents. Lang (1966: 411-12), for example, concluded that Jn 725 was partly drawn up from an interim document, Jn 658, the text of which closely resembles the first section on Jn 725. While Uchitel (1990-1991: 198) confusingly described Jn 725 as a 'recapitulative record'. Smith (1992-1993: 204), seemingly contradicting her original hypothesis, suggests that the tablets written by Hand 21, Jn 658 and 706, are likely to be older documents than those drawn up by Hand 2, including Jn 725; the argument for which was based upon the outwardly worn appearance of the writing surface and that these are the only exceptions to an entire dossier being created by Hand 2. By extension, then, at least a three stage process was proposed in which the 'old' tablet (Jn 658) was partially used to create the working tablet Jn 725, which in turn formed the basis for final version tablets that largely remained to be composed but included Jn 692. Others, contrastingly, have suggested that Jn 658 is a final document produced from Jn 725 (e.g. Lindgren 1973b: 62-63; Shelmerdine 1985: 112; Palaima 1988: 84).

Such is the obvious confusion and contradiction surrounding these documents that we must look at more fundamental questions surrounding the working tablets hypothesis in order to solve the problem. Even if we accept the basic model proposed in the most recent detailed study, numerous inconsistencies exist (Smith 1992-1993). Why, for example, if Jn 725 was simply a working tablet, did the scribe feel it necessary to produce another modified copy of the Jn 658 text only to have to rewrite it again when Jn 658 itself could have served the purpose of a working copy? If Jn 725 was indeed a working text, why was the initial text of this document concerning allocations to smiths at *e-ni-pa-te-we* not erased after the writing of the supposed final document Jn 658? Or, conversely, why was Jn 658 not simply destroyed if it was the precursor to Jn 725? More fundamentally, though, is there any evidence other than the similarity of texts to show that scribes commonly made use of

working notes, especially notes that themselves exhibit the same basic characteristics as the supposed final documents? In the documents previously discussed, erasures, corrections and adjustments are commonplace throughout, which we should not expect to see if the scribe had been producing them from rough drafts. From the outset, I suggest that there is no justification whatsoever for these traditional interpretations and that a careful analysis of the individual tablets and of the document set as a whole demonstrates this.

Undoubtedly the basic premise derives from the characteristics of Jn 725 and the presence of several, substantial erasures, including the previously mentioned paragraph Jn 725.18-22 replicated on Jn 692, which Smith (*ibid*: 194) suggests confirms the conclusion that this is effectively a working 'notepad'. In this single instance, it is possible to suggest that the erased text on Jn 725 was indeed a precursor to that of Jn 692, but even here the interpretation is not straightforward. It is noticeable though that analyses focus upon this feature in isolation without considering the context in which the paragraph was written and then erased. The lack of similar substantial erasures above these lines shows that this section, concerning the smiths of *e-ni-pa-te-we*, had already been composed prior to the writing of the erased paragraph and furthermore since the surface of the tablet was evidently hardened before the erasure occurred (*ibid*: 246 n. 230), some considerable time had elapsed after both sections, and indeed subsequent sections, had been written. If we examine the text that was erased, compared to what has been described as the final version on Jn 692, significant differences are apparent that bring into question the notion that Jn 725 is, in its entirety, a working document.

It is certainly not sufficient to base the concept of a draft text on the fact that the names of the smiths are, without exception, replicated on Jn 692 for the context in which each appears is significantly different. Jn 725 names all eight individuals as being in possession of a *ta-ra-si-ja* allocation and that the total amount of that allocation is AES M12. In contrast, Jn 692 explicitly states that four of these smiths are *a-ta-ra-si-jo* and that the two smiths, *ri-ja-ko* and *a-pe-te-u*, will each receive AES M6. Both tablets were written by Hand 2 and yet the differences in detail cannot be explained with reference to either document since both are explicit in how the allocations are to be made. Something must have occurred that drastically altered the information concerning six of the smiths that led to the necessary erasure of the Jn 725 variant. However, there is absolutely no evidence or logical reasoning to suppose that this was simply a result of a redrafting of Jn 725. If this were the case then where did these changes originate from and why was the preceding section concerning the smiths at *e-ni-pa-te-we* not also erased? This too, it has been suggested, is replicated on another tablet, Jn 658 by Hand 21, and so following the logic applied to Jn 692 the earlier version of this data

should also have been erased. In addition, the final line of the erased text, Jn 725.22, does not appear to relate to the preceding 4 lines; it is a totalling line referring to an amount of AES M30 which does not tally with either the erased text or with any of the remaining, extant entries. It is possible that this was originally a cumulative total of the erased section and the immediately preceding allotments to smiths of]-*nu-we-jo* (Jn 725.14-16), perhaps indicating that the allocations for these two areas were made together or from the same source, but unfortunately the evidence is too slight to be conclusive.³⁴

The change in information between Jn 725 and 692 is highly significant especially since the overall allocation remains the same. The principles governing the reallocation of individual allotments is virtually impossible to determine but, nevertheless, six smiths were redesignated as *a-ta-ra-si-jo*. Whether their eligibility was brought into question or they, as individuals, could not fulfil the obligations inherent in the *ta-ra-si-ja* system, it is unlikely that the scribe was responsible for the decision to revoke these allocations. Once again, the tablets evoke a scenario of negotiation in which the original decisions had, at a slightly later time, to be amended and were subsequently rewritten. Such an event is almost inconceivable if the tablets are merely the product of a passive bureaucracy, recording information gathered from various quarters for some unspecified future consulting need. I suggest that a more plausible scenario is highlighted by the nature of the layout of Jn 725. Immediately preceding the erased text is a blank line delineating this from a three-line text concerning allocations made to individuals at]-*nu-we-jo* (Jn 725.14-16), itself an unquestionably unique text. Preceding this is a three-line blank section separating it from the initial text concerning the smiths of *e-ni-pa-te-we*. Following the pattern observed above, it appears that the purpose of these blank lines was to provide the necessary space in which the tablet could be cut and individual documents created, but that this ambition was never realized. Once again, the impression given is that the scribe was in haste to record these details but that no time was available to separate individual texts or to make use of fresh tablet blanks. Instead, as each was completed a gap was provided to visibly demarcate them and possibly allow for the later separation of individual documents. Only in one instance, where the status of individual allocations was substantially changed, did the scribe make use of another tablet blank, either started completely afresh, or from the base of one already written, to produce Jn 692. This revised version was provided with exceptionally wide and uneven ruling and the individual signs written so large and widely spaced that even the relatively short introductory line could not be fitted into the entire width. Indeed, the initial three signs of *na-i-se-wi-jo* are of

³⁴ It is also possible that, given the total of AES M30 (i.e. AES L1), this is unconnected with the preceding entries and reflects the measurement of a single copper oxide ingot used for the late additions (see discussion below).

inordinate size which the scribe substantially reduces, possibly foreseeing the need to inscribe *e-ko-te* above *ta-ra-si-ja*. Nevertheless, it is noticeable that this revised version, uniquely amongst the Jn series, contains no erasures or other corrections apparently confirming that this is indeed a revision.

The initial text on Jn 725.1-10 presents many of the same features as the erased text, but here the differences with the proposed final draft document, Jn 658, are even more extensive. The two documents refer to the allocation of AES to named smiths at *e-ni-pa-te-we* but are written by different scribes, utilizing dissimilar formats. Jn 658.2-10 lists 16 names each with an associated allocation of AES M5 with the subsequent line providing a total of these AES allocations. Of these, 12 are duplicated on Jn 725.2-8 but curiously one name, *ko-ma-do-ro*, has been deliberately erased from the final position in the list (Jn 658.10) and the totalling line adjusted accordingly. Other than the entry on Jn 725.8 this name does not appear elsewhere but, if we are to accept that working tablet model, there is no obvious reason why this name should have been removed from the list of allocations following its transcription onto Jn 658. Indeed, as with the previous discussion, we are faced with significant rearrangements of the allocations and revisions of the recipients. Three names on Jn 658 have been claimed to be the same as those on Jn 725 but have apparently been transcribed wrongly (*ma-ka-wo* on Jn 658.3 for *ma-ka-ta* on Jn 725.4, *wa-ka-ta* on Jn 658.7 for *wa-tu-ta* on Jn 725.5 and finally *po-ru-e-ro* on Jn 658.9 for *o-ru-we-ro* on Jn 725.6), but this is both counter-intuitive and circular argumentation. As Nakassis (2006: 162-63) admits, these differences are more than simple spelling variations between two scribes and the only reason to suppose that this is the case, is that it has been assumed that the two documents are effectively the same and that consequently all of the names on the second document must correspond to those on the preliminary text. Yet why should transcription errors occur so abundantly when the scribe is supposed to be copying from text to text? And why, if the name of *ko-ma-do-ro* could be recognized as a recording error, were these not? Many different names in the Pylian tablets contain only a single sign difference between them (*ibid*: 375-585) and, I suggest, that to deliberately read three names as having significant and problematic 'spelling errors' to match them with names appearing on Jn 725 is simply forcing the data to fit a preferred model. Furthermore, one name, *pi-ro-ne-ta* (Jn 658.3), is unique to this document and only special pleading would see this equated with *i-*65-qe* on Jn 725.8 (*ibid*: 518).

It is not just the names that are problematic in this case but the allocations themselves. The final line of the supposed final document (Jn 658.12) provides the standard introductory phrase concerning those who are *a-ta-ra-si-jo*, but no names are given under this heading.

Evidently the scribe expected to have to list individuals with this designation but no names were forthcoming. Jn 725 by contrast is unequivocal: no individuals were considered in this way and no introductory statement was made. The text of Jn 725.1-10 simply records 27 names as having allocations but with no individual amounts assigned. In this respect we can only assume that the total noted in the following line (Jn 725.10) was to have been distributed evenly between these smiths. The amount specified is however slightly less than that of Jn 658; AES L2 M18 as opposed to AES L2 M20. Distributed evenly the smiths of Jn 725.2-8 would receive an amount less than AES M3 compared to the AES M5 allocations of Jn 658 enumerated individually.³⁵

Ordinarily such differences would be sufficient to distinguish these as different texts. However, a degree of confusion exists since many of the names appear to overlap. Whether or not these names can ever be shown to truly correspond, it is significant that none that can be equated with any degree of certainty appear in anything approaching the same order in either text. Lang's (1966: 407-12) attempt to establish juxtapositions of paired names pointing to an exceedingly complex but systematic pattern of transcription by the scribe is highly questionable. Not only do the suggested pairs not conform to the basic pattern suggested by Lang, such as with the pairing of *pa-qa-ta* and *au-ta-mo* (Jn 658.4) which it was suggested appear side-by-side on both texts but are in fact interrupted by the unique name *e-ka-no* on Jn 725.2, but also the three names with inconsistent spellings are assumed to be the same, and the deliberately erased name from Jn 658.10 is compared with the corresponding extant name on Jn 725.8. I would argue that any attempt to see a structured pattern in the respective positioning of the names on each document requires special pleading. If the scribe was transcribing from one tablet to another (whichever way round this is taken to occur), the positioning of the unique names interspersed with the common names would require explanation. In a straightforward transcription there would be no reason to ignore the basic structure of the original in adding new names (which themselves require an

³⁵ Although I have conducted the analysis here according to the latest readings and transcriptions of the tablets, one of these names, *po-so-ra-ko*, on Jn 725.8 I suggest remains problematic. Originally this was thought to be a genuine erasure, but Smith (1992-1993: 245, n. 225) suggested that the name was more smudged than erased and the latest edition of the tablet transcriptions (Bennett *et al.* n.d.) maintains this position. However, I suggest that this is incorrect. The total (Jn 725.10) of AES L2 M18 is also recorded over an erasure; an erasure that apparently reduced the total by AES M3 from AES L2 M21. These figures are entirely consistent with 27 individual allocations of AES M3, reduced by one to 26 allocations of AES M3. If we maintain the position that *po-so-ra-ko* was not erased but simply smudged then the reduction in the total line is problematic (if we are to assume that the total was being divided equally amongst these smiths) since the individual allocations assume abnormal amounts. It seems far more likely that the name of *po-so-ra-ko* was indeed a deliberate erasure with the total adjusted by the necessary amount and that the individual allocations were of AES M3. If the scribe was indeed working at speed any necessary erasures may not have been executed as thoroughly or diligently as normal but merely sufficiently for the scribe to recognize them as such. The intense scrutiny that we subject the tablets to could, in such cases, be misleading.

explanation as to their origin) while also making three substantial spelling mistakes and other errors requiring erasures.

Further confusion arises from the claim that Hand 21's tablets are older than those of Hand 2 and that, consequently, Jn 658 is the precursor to Jn 725 (Smith 1992-1993: 203-04). The suggestion was based upon three criteria: the first being the scribal hand itself, the second that the surface characteristics of the two tablets by this hand were generally quite worn, and the third being the find-spots locating Hand 21's tablets within Room 7. However, I suggest that none of these arguments has any validity. As I have previously demonstrated, the use of Room 7 does not correspond to a scribal working area and the notion that tablets located there were those being worked upon is unsupportable. The second element of this reasoning, I suggest, has far too many variables present in the assorted processes that led to the preservation of these tablets to be considered as anything other than entirely subjective. Variability is to be expected not only in the make-up of the clay used for the tablets, but also in the surface finishing (the degree of smoothing applied to the writing surface). Furthermore we cannot deduce the precise circumstances of firing that led to their preservation. In the chaos of the fire that destroyed the rooms, some tablets would have received greater protection from objects covering them, while others would have been dried and fired at a significantly faster rate and, finally, we have no way of determining any post-depositional effects on individual tablets. Perhaps most damning of all is that this characteristic is also visible on Jn 692 by Hand 2. Finally, the most basic reason that Jn 658, along with Jn 706, are the only examples written by Hand 21 and are therefore representative of a different period of composition simply admits of no imaginable scenario in which this series could have been composed by two different scribes.

One of the most noticeable features of the analysis of scribal hands at Pylos though is the sheer number of individual scribes apparently writing these documents, which is itself surprising given the relatively small numbers involved. If, however, we consider this in the context of multiple arenas of composition, of records that are not simply part of administrative files kept for future reference, then the presence of multiple authors is not only unsurprising but also necessary. As is discussed below, there is every reason to suppose that the collection tablets were written by Hand 2 in a different arena from that of the allocation tablets. If these as a whole were being written concurrently then we face the fundamental situation of a scribe unable to be in two places at once. It was inevitable that another scribe would have to take over the responsibility of recording these allocations. Indeed, as I have been arguing, these so-called working tablets are more likely to represent

late additions to the allocations of AES and so I suggest that rather than being 'old' tablets, those written by Hand 21 are likely to be very late in the sequence.

Whether this is generally accepted, these texts present a fundamental dichotomy; on the one hand, a working text produced by one of the most significant and experienced scribes at Pylos supposedly providing the basis for a new document by Hand 21, but one in which some names are apparently transcribed incorrectly, one name included that did not previously appear, another replicated from the working document but then erased and, finally, 11 other names removed without trace. Or, on the other, Hand 2 uses the working text of the other scribe to produce a document that retains none of the name order of the first, apparently randomly inserts new names while at the same time reduces the overall allotment and finally uses the same tablet to record three new texts subsequent to the rewritten text. Both scenarios are highly illogical and certainly less than credible. The time is, I suggest, long overdue for it to be acknowledged that these are not effectively the same documents but are complementary. We should not be misled into attempting to assign inconsistencies between such documents to scribal errors simply to support unsubstantiated hypotheses and in doing so effectively become Mycenaean scribes ourselves correcting documents to how we think they should read. Rather, we should accept that the scribes consistently made, discovered, erased and corrected their own mistakes and that as such the majority of inconsistencies point to modern interpretive problems. Furthermore, we must acknowledge that for skilled practitioners writing errors and erasures should be minimal in the controlled environment of a scribal 'office'; where we can identify substantial levels of corrections we should ask whether this is indicative of the context in which the tablets were written.

In the case of Jn 725, not only must any interpretation account for all of the issues raised thus far but also of the final section of text on Jn 725 concerning allocations to smiths at *a-ke-re-wa*. This record, invariably ignored in analyses of the working tablet model, is particularly interesting given that another substantial document exists concerning allocations to this area (Jn 310) and further allocations are also given on Jn 693, also considered to be a working document. However, none of the names in these documents overlap, nor is there any correspondence in the numbers of smiths involved; four in Jn 725, eight in Jn 310 and three in Jn 693. Differences also exist in the quantities of AES and the descriptions of individual allocations. In Jn 310, a total of AES M12 is divided equally in AES M1 N2 allotments but in Jn 725 the same total has not been specified by individual allocations (we can only surmise that it was divided equally in AES M3 shares), whilst on Jn 693 a total of AES M16 is split into two allocations of AES M4 and one of AES M8.

Once again we are faced with texts that display similarities but none that support the notion that these represent different stages in scribal process of document creation. In short, the claim that Jn 725 represents a preliminary or working document is unsubstantiated and unsupportable. All of the features that I have highlighted above indicate a far more subtle and complex intent that cannot be explained simply with recourse to a working copy made right through the writing of Jn 692, 658 and others awaiting production. Neither can Jn 693 be considered in this vein simply because it contains entries relating to two distinct regions. Indeed, the notion that we can apply an evolutionary scheme to the generation of documents, in terms of a more ordered format, as has been attempted in defining an order of composition is inherently problematic. There are distinct inconsistencies in the derived sequence for Group A tablets but more than this, the basic model is contradictory. It relies upon a pre-conceived idea that the tablets were being produced as records of transactions to be stored in Room 8 of the AC for future consultation and that the basis for the production of the office copies were working notes such as Jn 725 and 693. If this were the case though, why should such substantial variation exist at all in the formatting of the Jn series? Can we accept the idea of continual inexactitude in the efforts of the scribe to create organized documents for the purposes of record, in terms of the layout of information and the initial preparation of tablet blanks and the later cutting of the inscribed tablets? Moreover, why would the scribe, apparently, deliberately separate information concerning the same allocation district? If, alternatively, we accept that Jn 725 and 693 are not working notes or preliminary drafts then only one possibility remains: that these are additional allocations to documents that have already been created.

There can be little doubt that these documents were written late in the sequence and it is interesting to note, that with only a few exceptions, named smiths referred to in these 'late' tablets are difficult to identify in other contexts. As Nakassis (2006: 174-202) has convincingly shown, there are generally numerous correspondences between names in the Jn series and those in other tablet series, many of whom apparently hold positions of some responsibility, or have landholdings, substantial flocks and herds of animals, as well as attached *do-e-ro*. I suggest that it is no coincidence that with these later documents few such correspondences can be detected and the individuals concerned are relatively invisible. Although the specific writing order proposed by Smith is unlikely to be accurate, the overall group order based upon the tablet locations is almost certainly correct. From this, it seems that the earliest tablets concern smiths that are already well known in respect of such allocations and work responsibilities and attention is given to these first. The compositional arena was evidently dynamic with information being provided at a considerable rate and in a less than systematic form. Amendments to the tablets (particularly in the case of Jn 692/725)

indicate a process of negotiation in which particular allocations are altered and the documents amended accordingly. Such negotiation may also explain the curious addition on three tablets (Jn 431.6, 601.8 and 845.7) of a *basileus* following the list of allocations. Much debate has been lavished on these inclusions, particularly as to whether these officials act as overseers or supervisors to the smiths in respect of the *ta-ra-si-ja* obligations (e.g. Killen 1987; Smith 1992-1993: 182; Nakassis 2006: 172, 268, 280). However, if such a liberal interpretation is applied then it is surprising that not only do the officials appear on only three tablets, but also that two reappear in the Jn series as smiths without allocations. I suggest that the answer to their inclusion is considerably simpler: that these were the representatives of the smiths who were simply responsible for receiving the allocations on their behalf.

The ‘collection tablets’. Policy in the making?

The concepts of patronage, obligation and negotiation are also apparent in two tablets that do not conform to the usual pattern of allocations but were, instead, evidently concerned with the collection of so-called ‘temple bronze’ from officials of the 16 major districts of the Pylian territory for the manufacture of spear and arrowheads. Written by Hand 2, Jn 829 and 881 provide a complementary but contrasting picture to the allocation documents that, I suggest, is indicative of a different arena of scribal composition. No smiths are mentioned in either document, with attention being focussed upon local district officials alone. The contributory nature of the most well preserved document, Jn 829, is defined by a distinctive heading paragraph beginning, significantly, with the use of the future tense in describing the contributions (*jo-do-so-si*); a comparatively rare occurrence in the tablet corpus but one that, combined with other characteristics, provides a revealing glimpse of the political concerns of the palace. Unfortunately, there is no indication as to whether such collections were an exceptional occurrence and the complementary tablet, Jn 881, is too fragmentary to aid with this question. Indeed, Jn 881 contains no recognizably preserved verb forms making an unequivocal association with Jn 829 difficult to confirm, despite there being several indirect indicators of just such an association.

It is, however, the question of the derivation of the recorded information that is of immediate interest, for there can be no doubt that it is considerably different to that of the allocation documents. Unlike the latter, the structure of Jn 829 is rigid and well-defined and, in terms of the overall format, is arguably the most organized tablet of this series with the heading paragraph followed by a neat, columnar arrangement of the specific details of the contributions. These are ordered according to the relative status of the nominated officials

defined in the heading paragraph, with those of the *ko-re-te* appearing before those of the *po-ro-ko-re-te*, but crucially with an underlying structure based upon locality, suggesting that a fundamental aspect of Pylia identity was attachment to a specific place. In this respect, Jn 829 concurs with the pattern established in the allocation documents where the defining element is the place to which allocations are being distributed. Interestingly, rank does not appear to define the amounts expected to be provided by individual officials for although all of the *po-ro-ko-re-te* are expected to contribute AES N3, those of the *ko-re-te* vary between AES M2 for the first 10 listed, to a maximum of AES M3 N3 for the 12th, 15th and 16th individuals. As none of these officials appears by name, the principle governing these levies is most likely, instead, based upon the places each comes from. Moreover, as has been established through an examination of the provincial geography, the order in which the towns that these officials represent appears deliberately constructed, listing the nine major towns of the HP followed by the seven of the FP; an order that is repeated for the HP on Cn 608 and Vn 20 (Chadwick 1973: 142-44, 357-58, 511-14; 1976: 41-48; Bennet 1995; 1998; 1999).

Yet, the text neither explains why these particular officials are obligated in this manner, nor provides any details of how, by who, or where, the contributions by these officials will be worked into the specified objects. All that we can surmise from the quantities involved is that these do not represent the basis for the types of allocations that are addressed in the allocation documents, for all are relatively small and those of the *po-ro-ko-re-te*, at AES N3, are less than any of the individual allocations. Unfortunately, it is also clear that Jn 829 does not represent the only such statement of requisition, for the heading paragraph additionally states that contributions are expected from the *ka-ra-wi-po-ro*, *o-pi-su-ko*, and *o-pi-ka-pe-e-we*, but this document makes no further mention of them. Jn 881, by contrast, although poorly preserved, appears to concern the contributions of groups other than those in Jn 829, including the *o-pi-su-ko*. Here there is no indication of listing by place but simply by title, suggesting that perhaps there was no requirement to distinguish between individuals within an official role but that because of their rank they could be treated as a collective, a potentially highly significant characteristic in understanding the definition of identity across the Pylia tablets, particularly when we consider the various titles and means of identifying personnel in the A-series. Whether or not this interpretation is correct, Jn 881 does indeed appear to be an extension of, or a parallel document to, Jn 829 (Ruiperez 1963). How these contributions were organized in relation to those on Jn 829 remains problematic, but it is unlikely that we can simply consider these officials as the receiving officers responsible for ensuring the collection of the requisite amount of AES (Palaima 2004a: 291). If this were the case then we would justifiably have to question why several different ranking officials were

made responsible for the collection of variable amounts of AES from the same locality when the most straightforward means of ensuring collection would be to have one official responsible for collecting the entire quota from a particular place. Moreover, the lack of specific names is highly suggestive of a statement of policy made without knowledge of the individuals concerned.

As Palaima (*ibid*) highlighted, the overall quantity of contributions expected and the manner in which they are shared is undoubtedly significant; from the HP they amount to AES M24 N3 and from the FP, AES M23 N1. Both are markedly similar and fall within the range of equivalence to the mass of a single ingot of copper. Unfortunately, the incomplete nature of Jn 881 prevents us from determining how much the respective quantities should be adjusted by, but it appears that considerable care was taken in establishing the proportion of each provincial levy. As a dictate of future policy these demonstrate considerable foreknowledge of all of the allocations. Combined, these features indicate a significantly different source from which the scribe obtained the information. Unless we are to imagine that this is a list of contributions that officials are prepared to make themselves, the recorded details undoubtedly have their genesis within the palace and represent policy decisions that must have been determined and set by a political official of some responsibility, possibly even by the *wanax* himself. It is certainly difficult to imagine that such contributions would be set by anyone below the level of the *ko-re-te* mentioned in Jn 829, but in any case the context in which the scribe would have received the information is undoubtedly very different to the allocation documents.

This is also borne out by the location of the tablets themselves. It is, I suggest, no coincidence that the find-spots of both tablets are distinct from those of the remainder of the Jn series. While fragments of Jn 829 were found mainly in the Chasm, with one piece located on the bench in Room 8 just within the doorway connecting these two areas, those of Jn 881 were recovered from the Chasm and, importantly, from the area of the Inner Propylon (figure V-1). As I have previously argued, this tablet cluster cannot simply be dismissed as scatter from Room 8 but reflects a distinct group of tablets that were deliberately, if temporarily, placed outside of the entrance to Room 8. Their location, close to the doorway connecting these areas and, by extension, to the interior of the palace does, indeed, correspond with the hypothesis of an alternate arena of composition. In this instance, I suggest that these tablets were originally composed in the main megaron itself, in an arena of oral pronouncement and negotiation, before being removed to these outer rooms. The audience here, we may surmise, would have included the ranking officials responsible for the delivery of state policy and the policy makers themselves including the *wanax*, with the

actions of the scribe being a central element of the process of validating and confirming the obligation to comply with this policy.

The concepts of negotiation and obligation are not restricted to this particular situation but are also visible in the allocation documents. We have already seen how the scribe apparently amended specific entries in respect of particular individuals, removing or adding names or altering particular allotments, but the clearest illustration is provided by the *ta-ra-si-ja* system under which allocations are made. All allocations, whether of work or raw materials, are governed by this principle but, crucially, the documents also present details of an opposing situation in which individuals are explicitly referred to as *a-ta-ra-si-jo* – without an allocation. While this system remains the subject of some debate, the juxtaposition of these terms provides a clear opportunity to determine not only the principles governing the allocations but also the part played by the tablets themselves.

The *a-ta-ra-si-jo* entries and the removal of patronage?

The repeated denial of allocations and duties of work represented through the use of the oppositional term *a-ta-ra-si-jo* is probably the most revealing feature of this series. This obvious juxtaposition of information seldom receives attention, with conventional studies focussing almost exclusively upon on the positive *ta-ra-si-ja* regulation of allotments and work responsibilities and the comparison between this and the associated term *o-pa* (e.g. Chadwick 1973: 352-59; 1976: 140-43; Duhoux 1976: 69-115; Smith 1992-1993: 178-80; Gillis 1997: 509-11; Killen 1999; 2001; Nosch 2001; 2006). From the contexts in which *ta-ra-si-ja* appears (particularly in respect of textiles and bronze-working) there is broad agreement that the term carries the general sense of ‘an amount [of raw material] weighed out and issued for processing’ (Killen 2001: 161), and is thought to form the basis of a mode of industrial production controlled by the palace. However, it is in defining the function of the term *o-pa* and the relationship with *ta-ra-si-ja*, that I would argue that the most helpful definition of the latter has appeared: ‘...that it [*o-pa*] refers to work on a completed manufactured item or a unit of livestock, and is thus distinct from *ta-ra-si-ja*, which always refers to an issue of raw materials (or an object manufactured *with the help* of such an issue)’ (Killen 1999: 338, my emphasis).

A detailed analysis of this system is not possible within the confines of this thesis but one issue must be addressed in relation to the suggestion that *ta-ra-si-ja* designates a specific mode of production. While the sense of the term is relatively well understood, there is one assumption that is far from proven, for although in many ways the *ta-ra-si-ja* system can be

considered akin to a contract between the provider of goods and the recipient, it is not at all clear what restitution was expected in respect of this contract. Even though the term implies that raw material was issued for processing, it does not necessarily follow that the obligation had to be met with the return of objects created from the same material. The one consistent feature of all the tablets concerned with *ta-ra-si-ja* production is that not one document links all of the various stages of the production process in one record. While we can identify raw materials issued under the *ta-ra-si-ja* remit, there is rarely any indication of what is expected to be produced, or the delivery associated with that precise provisioning. Records of receipts are evident in the Knossos cloth tablets, as are records linking raw materials to supplying the production of specific goods, but these cannot with any certainty be linked directly to the records assigning materials to workers.³⁶ The Jn tablets are similarly explicit in the details of materials allocated under this system, but none specify the ultimate purpose of the AES allocations or give any indication that it was being distributed for the production of specific objects, and although the tablet corpus as a whole mentions many bronze objects, there is no way to link their production with the Jn series (Voutsas 2001: 153).

The omission of production targets, schedules or specific requirements for finished objects is often viewed as problematic since the basic assumption is that AES was allocated specifically to be returned in a finished form to the palace. Even were these allocations to refer specifically to bronze rather than copper this inference is questionable (Gillis 1997: 510). I suggest that we need to modify our understanding of the nature of this transaction by acknowledging that the basic element introduced by the use of the *ta-ra-si-ja* system is an obligation to reciprocate. Indeed, this is evident in the use of the term itself, where the subjects of *ta-ra-si-ja* in the introductory phrase are the smiths not the allocations. Whether or not we choose to view this system as working differently across the various craft industries or regions (Nosch 2001: 43), there are, nevertheless, significant differences between the textile and bronze industries that are the main focus of such studies.

³⁶ The Knossos tablets relating to textile production undoubtedly provide the most important information concerning the *ta-ra-si-ja* system (e.g. Killen 1984b: 49-51; 2001: 162-63, 172). However, even where documents have been identified in which specific amounts of raw material are issued to named groups for the production of a specific commodity (i.e. Kn Lc(1) 526) and a separate document exists suggesting the distribution of a quantity of the same commodity to an individual (i.e. Le 641 + fir.), we cannot definitively connect the two actions temporally. In this case, not only is the amount of finished cloth different to the amount specified in the 'production target', but also we cannot be certain that this would have derived from the same 'batch' (Killen 2001: 162-63). Furthermore, we are left wondering, as with the bronze production outlined below, where the textile workers are to obtain the other materials necessary for the production of these very particular items of cloth. No interest is demonstrated in the tablets for the various processes involved in production. Here we have wool provided but no indication of how or where the recipients are to obtain the materials for the fulling process, or the mordants and dyes for the finishing process, especially if the latter required the prized and 'expensive' murex purple.

Naturally the *modus operandi* of production in the bronze industry is far different to that of textiles utilizing very different knowledge, techniques and materials, with one of the more significant differences being that in order to produce bronze objects smiths required access not only to sources of copper but also tin and other raw materials. Even if we assume that the palatial requirements were fully known to all parties concerned, these allocations give no hint as to how, or where, the smiths were to obtain these other materials. If, as is argued below, the majority of AES allocations refer specifically to copper, then the situation becomes even more problematic, for the tablets contain no references to the tin required to produce the bronze objects apparently required. In such a situation, the *ta-ra-si-ja* system would be imposing a heavy debt upon those selected since the acquisition of another valuable commodity would seemingly have been their responsibility. Under these circumstances it is difficult to see how *ta-ra-si-ja* can work as a mode of production unless we understand it simply as a contract based upon the provision of raw materials by the palace. Although we can deduce a meaning to the word, this alone does not explain the underlying reasoning of the system or the motivation behind the palace employing such a system of production. Nor can we see directly how these records would have enabled or aided in the operation and maintenance of such a system. It is not possible, for example, to examine these records and observe that a particular individual was given a specific amount of raw material for the production of a precise quantity of a finished product and to then reference that directly to a receipt by the palace naming the individual(s) responsible for the fulfilment of that particular work responsibility. As a direct administrative tool the tablets appear to be largely inadequate.

The problem of understanding how this system may have worked, I suggest, can be alleviated by considering the concept of patronage. The provision of commodities within an official context would have constituted a form of patronage; the distribution of a valuable commodity amongst selected workers in a public display of power, wealth and influence. The public recording of these actions would not only have provided the basis of a 'contract' between the parties involved but also reinforced this display of beneficence and emphasized the obligation to engage in reciprocity. In effect, the production of tablets that for the majority would have been mysterious and unknown would have represented part of a highly stylized and ritualistic event of overt palatial patronage. The detailed, cross-referenced accounts that we might expect to see in a purely administrative recording system would not have been necessary in this context since the value of these records would be in their symbolic association with this act. Although, on occasion, the palace may well have stipulated its requirements, reciprocity in such a situation need not be immediate or like for like; in other words, we should not conclude that an obligation based upon the distribution of

materials through the *ta-ra-si-ja* system places a requirement on the recipient to reciprocate specifically with objects created from these materials. The *ta-ra-si-ja* allocations, therefore, represent a mechanism through which the palace can create, maintain and manipulate allegiances within particular social groups that are both necessary and advantageous to the political, social and economic operation of the territory as a whole.

Even with this proposed modification to our understanding of the *ta-ra-si-ja* system in mind, the inclusion of the direct oppositional term still appears superfluous to the motivation behind the production of these records. Certainly, within conventional studies, it represents a fundamental dichotomy, for in a purely administrative system it should only be necessary to account for what has happened to the AES, rather than what has not happened to it (Nosch 2006: 181). Why should it be necessary to record, often by name, those individuals who have not been presented or issued with AES? This additional information must have been viewed as being of similar worth, but the implications of these statements remain unclear. Was the information significant in itself or was it the act of recording people as *a-ta-ra-si-jo* that was of most importance? I suggest that the significance of these entries has been considerably underestimated, particularly given that the term *a-ta-ra-si-jo* appears only on the Jn series from Pylos. Conventionally, the interpretation of the term simply as ‘without an allocation’ has been deemed to be self-explanatory and is indeed likely to be accurate (e.g. Lejeune 1961: 419; Palmer 1963: 279; Ruijgh 1967: 118, 333; Chadwick 1973: 352; 1976: 140-41; Lindgren 1973b: 62-63; Duhoux 1976: 69-70, 103; Aura Jorro 1985: 113-14; Smith 1992-1993: 178-79; Killen 2001: 163), but the underlying meaning of this designation is far from clear. Duhoux’ (1976: 103) explanation, that the *a-ta-ra-si-jo* entries reflect smiths for whom no obligation to work has been imposed by the palace but whose names are included as a means of identifying those who will be subject to a future *ta-ra-si-ja* obligation after the smiths under *ta-ra-si-ja* have discharged their duties, similarly, does not appear logical when considering those who are designated in both categories. Chadwick (1976: 140-41) viewed these individuals as simply ‘unemployed’ and used this evidence along with the apparently small allocations being made to others to develop a thesis of a general bronze shortage and the rationing of raw materials indicative of an impending crisis.³⁷ Other scholars, while rejecting this hypothesis have been unable to provide alternative explanations (e.g. Uchitel 1990-1991: 196-98, 202; Smith 1992-1993: 178-79). In order to resolve this, it is necessary

³⁷ Unsurprisingly, estimates of what could be manufactured from the individual quantities of AES concentrated on items of a military character such as arrowheads, swords, spearheads and armour (Lejeune 1961: 433; Chadwick 1973: 356; Gillis 1997: 510 n. 26) rather than the many more utilitarian, ritual or even decorative objects that would have been in circulation (Michailidou 2001: 92-97). This automatically created the impression that the work of bronze-smiths was not only intimately connected with the palace but was also associated with the events that led to the destruction of the palace.

to examine more closely the individuals who carry this designation (table V-3) and for this we are fortunate that considerable progress has been made in prosopographical studies (Lindgren 1973a, b; Nakassis 2006).

Tablet (Jn)	Location (Room)	Number of smiths	Number of <i>do-e-ro</i>	Number of Potnian smiths	Number appearing in other Jn tablets	Individuals in other tablet series
310	8	4	4	1	1	1
320	8	5				1
389	8	5				1
410	8	2				
415	8	5				
431	8	15		55	3	6
478	8	1				
601	8	5			1	1
605	8	3	6		1	1
658	7					
692	7	6			6	2
693	7	2				1
706	7	4			1	1
725	7					
750	7	3?	1?			1
832	<i>Chasm</i>	13*			1	5
845	<i>Chasm</i>	7			1	2
927	7	4			1	
Totals		84	11	56	16	23

Table V-3: Individuals in the Jn series designated as *a-ta-ra-si-jo* (* denotes the non-allocation of work responsibilities rather than AES).³⁸

It is important to note the bias provided by the entries on Jn 431; the numbers of individuals recorded here as *a-ta-ra-si-jo* is, by far and way, the greatest of all the Jn series, with the vast majority described as Potnian smiths. However, 46 of these have no individual designations but are recorded as groups of men responsible to three named or titled individuals. In two of these cases the names of those responsible for them cannot be reliably identified outside of this document, whilst the third, *i-je-re* [(Jn 431.25) remains uncertain. Reconstructed as *i-je-re[-wo]*, this would be the genitive form of the title 'priest', but this could equally be reconstructed as the title of 'priestess' (*i-je-re-ja*); whichever is correct, these individuals are clearly responsible collectively to a priest(ess). There is a temptation to extrapolate from this

³⁸ In this study the strength or probability of correspondence between occurrences of the same names were considered to range from tenuous through to certain but for the purposes of this discussion all references to tenuous connections have been removed. A more detailed breakdown of this data is presented in Appendix A.

relationship and the lack of names that these individuals are essentially slaves, but this, I would argue, would be entirely unjustified. Their designation remains under the heading of ‘Potnian smiths’ and as such it would be more appropriate to consider these individuals as trainees or apprentices to the craft still requiring some form of supervision, sponsorship or patronage in which to practise.

This issue, alone, illustrates many of the contradictions inherent in conventional interpretations. The inclusion of *do-e-ro* in the lists presents a basic dichotomy if the interpretation of the term as ‘slave’ is maintained. Whether or not the *a-ta-ra-si-jo* designation denotes individuals that have simply been excluded from consideration on this occasion, evidently *do-e-ro* were considered eligible to receive allocations. Crucially, two (Jn 410.10 and 750.13) are described as smiths as well as *do-e-ro* (of *de-ko-to* and *e-u-we-to-ro* respectively). I suggest that this is not a mistake, since the scribe appears to consistently use the phrase *to-so-de do-e-ro* to introduce the majority of *do-e-ro* who are *a-ta-ra-si-jo* (Jn 310.11-12, 605.9-11 and, possibly, Jn 706 v.1-6). Why should slaves be accorded the privilege of palatial handouts of AES, a valuable commodity whatever its precise nature, and why would they be in a position to make use of this in a craft capacity? I would argue that, the very interpretation of *do-e-ro* as slaves is perhaps one of the most fundamental misinterpretations applied to these documents and in this particular context they appear to be apprentices to the trade rather than slaves.³⁹

If we accept that patronage was a tool of the political elites then it is just as likely that patronage also operated outside of this domain (Shear 2004: 55). Craft patronage would serve both as a means of establishing the next generation of skilled workers and function as a means of political control. Indeed, as Nosch (2006: 167) has suggested, the *ta-ra-si-ja* system itself may have served as a tool of ‘training and apprenticeship’ and have acted as a source for ‘the inheritance of knowledge’. Although I would slightly modify this statement in light of the suggestions made here, that if the *ta-ra-si-ja* allocations are viewed as direct palatial patronage, then the system allows the patron (in this case the palace) a modicum of influence and control in the development and maintenance of craft industries vital to their own needs. Leaving this argument aside, the impression that the vast majority of named smiths are unknown outside of these *a-ta-ra-si-jo* entries is, in fact, misleading; many are identifiable in the Jn series and across other series. It is these correlations of individuals across the various documents and dossiers that are central to understanding the juxtaposition of information. As table V-3 illustrates, many of the smiths denied allocations appear in

³⁹ See also chapter VI and, Peters in prep (b).

other guises in which it is possible to examine their respective occupations, titles, ranks and responsibilities as well as to observe the apparent contradiction of individuals who are listed under both the *ta-ra-si-ja* and *a-ta-ra-si-jo* headings.

No simple explanation based upon relative rank can explain the designation; smiths, Potnian smiths and *do-e-ro* are included and prosopographical studies reveal that these designations themselves conceal a complex network of individual identities with a large degree of variability in relative rank, visibility and responsibilities (Lindgren 1973a; 1973b; Nakassis 2006). Two documents, Jn 413.10 and 431.11, list unnamed individuals as both smiths and *do-e-ro*, whilst the smiths *pa-qo-si-jo* and *ke-we-to* (Jn 310.8) who are *a-ta-ra-si-jo* have attached *do-e-ro* who are also *a-ta-ra-si-jo* (Jn 310.12 and 310.11 respectively). *Pa-qo-si-jo* also appears on Jn 832.10 at a different centre of activity as a specialized bronze worker (a ‘finisher’) with an allocation of work, and on Jn 601.8 with the title of *basileus*. Many of the named smiths do not appear elsewhere either in the Jn series or other tablets, whilst others can be identified, particularly in the Cn series, as holding flocks or herds of animals. Still more appear to possess the fundamental contradiction of being designated *ta-ra-si-ja* and *a-ta-ra-si-jo*. Particularly noteworthy is the case of *po-so-ro* who is listed on Jn 601.5 and 750.3 as a smith with allocations of AES M8 and M1 N2 respectively and yet on Jn 845.11 is *a-ta-ra-si-jo*. The only other information that can be gleaned about this individual comes from An 1281.6 where he is assigned to the woman *me-ta-ka-wa* at the shrine of Potnia Hippeia. There is, unfortunately, no indication of how this relationship works even if we interpret the woman as a priestess or religious functionary, but as was noted above it is all too easy to fall into the trap of applying the label of slave to the attached individual (e.g. Nakassis 2006: 194). Nevertheless, there is evidently a contextually specific relationship of responsibility to *me-ta-ka-wa*, which has not been transferred to the context of the AES allocations.

One of the most intriguing individuals, though, is *a-tu-ko* (Jn 927.3), who also appears on Jn 658.10 and 725.7, where he is assigned AES M5 and an unspecified amount (AES L2 M18 shared between 26 smiths) respectively. He is also identified on En 609.5 and Eo 211.2 as holding a *ki-ti-me-na* plot of land at *pa-ki-ja-ne* from the *te-re-ta wa-na-ta-jo* as well as a *ke-ke-me-na* plot on Ep 301.5. So, in this instance we evidently have a smith of some prominence who is evidently a landholder with ties to senior officials but, more importantly, is also accorded the title of *e-te-do-mo* (armourer) with the additional epithet on En 609.5 of *wa-na-ka-te-ro*; *a-tu-ko* was evidently accorded the privileged title in some contexts of ‘the king’s armourer’ (Palaima 1997; Shelmerdine 1999: 23). There is no obvious reason, therefore, why he should be described as *a-ta-ra-si-jo*. He was clearly a high profile

practitioner of the craft with direct connections to the highest rank in society, who had already received AES allotments on other tablets. Yet it was deemed necessary to have a record that he received nothing in respect of the smiths at *Jme-no*. Since the correlation of identities of this individual in both the E and J-series is considered certain (Nakassis 2006: 198-200; 402), it is perhaps significant that the entries on the Jn series do not include either epithet used on the E-series. Such an omission may be coincidental, or it may suggest that the title was highly contextualized and that in respect of these allocations his status as the king's armourer was not a consideration.⁴⁰

There is, therefore, no straightforward explanation as to why particular individuals were listed as *a-ta-ra-si-jo*. Smiths of all abilities and rank received this designation and the only discernible distinction, based upon those who also hold allocations, is that it was centred upon the particular place concerned. Conventionally, explanations tend to be founded upon the notion that these are individuals who have performed their duties and are thus exempted or that it is somehow not 'their turn' to perform duties for the palace (Smith 1992-1993: 180). But this is based entirely upon the unsubstantiated interpretation that the allocation of AES or work responsibilities was a required service of the palace; in other words, there are negative connotations attached to the entire concept of *ta-ra-si-ja* in that the benefits are seen as being only to the palace. The examples of those who are recorded under both headings would appear to undermine these arguments and indicate a more complex underlying rationale. Neither can we simplify this with notions of rationing or shortages (Chadwick 1976: 140-41), for as has already been discussed, consideration of the so-called 'working' tablets suggests that extra allocations were being made subsequent to the original allocations.

The inclusion on Jn 389.7 and 601.7 of the term *e-pi-da-to*, associated with a single allocation of AES, creates similar problems. The precise sense of the phrase in which this appears remains uncertain since in both occurrences the phrasing is slightly different but, nevertheless, it is commonly understood to refer to a supplementary allotment distributed amongst all the named smiths (Lejeune 1961: 420; Palmer 1963: 286; Lang 1966: 400; Ruijgh 1967: 347; Chadwick 1973: 354, 510; Lindgren 1973b: 62, n. 9; Duhoux 1976: 70,

⁴⁰ There is a temptation to view this title as referring specifically to a craft position but the omission of this epithet from the Jn series may suggest otherwise. Shelmerdine (1999: 23) has suggested that such epithets were necessary or relevant only in the context of religious personnel holding land at *pa-ki-ja-ne*, and Palaima (1997: 411-12) that it would simply be understood within the context of the AES allotments and that the land allotments were effectively payment for the service provided. However, it is a distinct possibility that the interpretation of this title has simply been misunderstood. The armourer need not refer to a craft position but simply to the attendant responsible for aiding the *wanax* with his armour whether in a ceremonial or a military capacity. Certainly *a-tu-ko* is far from the most favoured smith in the Jn series, with this privilege being afforded to *o-na-se-u* and *we-we-si-jo* respectively.

75; Aura Jorro 1985: 223). The inclusion on Jn 389.7 of *pa-si* in conjunction with *e-pi-da-to* appears to confirm that this was indeed meant to be distributed amongst all of those previously named (Aura Jorro 1993: 63-64, 88) rather than as has been suggested that it refers to AES that has 'yet to be allotted' (Smith 1992-1993: 189). The amounts involved are relatively small (AES M6 and M7 respectively), yet in each case it would have been sufficient to represent at least one more individual allotment but for some unspecified reason was held back. Crucially, both tablets contain lists of smiths who are designated as *a-ta-ra-si-jo* so despite extra material being available for allocation these individuals remain deliberately excluded from such consideration (Uchitel 1990-1991: 199); an apparent anomaly for which there must be an underlying rationale.

This conundrum is most clearly answered by the apparent decline in the number of individuals classed in this manner between the tablets in Room 8 and 7. Although the precise writing order that Smith suggests is problematic, the suggestion that the tablets in Room 8 were the first to be deposited and hence composed appears valid. As table V-3 shows, all of the Room 8 tablets contain entries for individuals designated as *a-ta-ra-si-jo* while, by contrast, two documents in Room 7 contain no such entries. One of these, Jn 658.12, contains the standard introductory phrase *to-so-de a-ta-ra-si-jo* but does not include any names under this heading. Evidently the scribe expected to be recording a list of smiths who were *a-ta-ra-si-jo*, but for some reason no names were forthcoming. If these documents were created within the context traditionally assigned to them this would represent a clear, and apparently unrecognized, scribal error, but one which cannot simply be dismissed in the same fashion as supposed 'spelling mistakes' or 'arithmetical errors'. Moreover, the largest number of *a-ta-ra-si-jo* designations in the Room 7 tablets concerns the allocation of work responsibilities as opposed to the allocation of AES. Of the 71 smiths (not including Potnian smiths or *do-e-ro*) denied AES allocations, 45 appear on tablets located in Room 8 and only 26 on tablets in Room 7. If, however, we consider a situation where tablets are being read, or in this case produced, in a public arena of oral negotiation, pronouncements and proclamations such as the outer courts and are being taken from this arena to be temporarily stored in the AC, then a straightforward explanation emerges that not only explains these apparently contradictory features, but also helps to explain the few cases of multiple *a-ta-ra-si-jo* entries and the pattern of multiple allocations (table V-4).

Name	Allocations	Tablets	Allocations	Total Allocations	<i>a-ta-ra-si-jo</i> entry?
<i>a-ti-pa-mo</i>	2	Jn 320.6	M1	M2 N2	N
		Jn 750.4	M1 N2		
<i>a-tu-ko</i>	2	Jn 658.10	M5	M8?	Jn 927.11
		Jn 725.7	M3?		
<i>a₃-so-ni-jo</i>	2	Jn 310.3	M1 N2	M6 N2	N
		Jn 706.11	M5		
<i>e-do-mo-ne-u</i>	2	Jn 389.2	M3	M4 N2	N
		Jn 605.2	M1 N2		
<i>mo-re-u</i>	2	Jn 389.6	M1 N2	M1 N2+	Jn 431.13
		Jn 750.10	?		
<i>o-na-se-u</i>	3	Jn 601.6	M12	M20?	N
		Jn 658.5	M5		
		Jn 725.4	M3?		
<i>po-so-ro</i>	2	Jn 601.5	M8	M9 N2	Jn 845.11
		Jn 750.3	M1 N2		
<i>we-we-si-jo</i>	3	Jn 431.18	M3	M11?	N
		Jn 658.2	M5		
		Jn 725.3	M3?		

Table V-4: Smiths with multiple allocations of AES in the Jn series.

It is clearly counter-intuitive to suppose that it was a bureaucratic necessity to record all smiths who were not selected to receive materials, especially since no justification is provided for their rejection. Table V-4 similarly confirms that we cannot interpret *a-ta-ra-si-jo* simply as the unavailability of raw materials. Many of these multiple allocations derive from tablets that are evidently late allocations rather than duplicate entries, and with these come a definite reduction in the number of individuals designated as *a-ta-ra-si-jo*. Moreover, these are individuals for whom prosopographical visibility is considerably reduced and who therefore appear to be less well-known to the palace than the initial recipients. Since it appears most likely that the *ta-ra-si-ja* allocations represent palatial patronage then patronage is being conferred upon, or maintained with, those who were already favoured in some way followed by individuals newly favoured. In this context, the *a-ta-ra-si-jo* entries appear to be related specifically to this display of patronage, but it is something that can be both rendered or taken away. This at least explains the contrasting situation with *ma-no-u-ro* (Jn 605.7, 692.5 and Jn 725 r.[[19]]) and *a-ka-ma-wo* (Jn 431.12 and 706.18) who are the only individuals with multiple *a-ta-ra-si-jo* entries and do not receive allocations elsewhere.

There is little doubt from the preceding discussion that we cannot reduce interpretations of this phenomenon to a single underlying reason; the enormous variability in the personnel

carrying this designation indicates multiple considerations at work. Nevertheless, I suggest that these allocations are primarily a direct manifestation of palatial patronage towards favoured smiths in which the *ta-ra-si-ja* designation imparts a clear obligation upon those favoured to reciprocate. As such, the oppositional term represents a similarly clear statement of those who are not favoured and, significantly, those for whom patronage has been withdrawn. The *a-ta-ra-si-jo* designation in one respect then is a public affirmation of the penalty that can be incurred if the obligations inherent in the *ta-ra-si-ja* 'contracts' are not met. It also appears to serve the additional purpose of confirming the limits of visible patronage, so that in the case of *a-tu-ko* where he receives two allocations the denial of the third represents a limit placed upon visible patronage. In this situation, the recording of *a-ta-ra-si-jo* no longer appears to be an unnecessary qualification to the tablets, but a very necessary public display of the removal or limiting of such privileges.

What do these allocations amount to?

Although this analysis suggests a scribal process far removed from the confines of the AC, in an arena of the public negotiation of favours, patronage and reciprocal obligation, the question still remains as to precisely what these allocations represent. From the earliest studies of these tablets, scholars have attempted to discern patterns in the allocation totals that would explain how the allocations were determined and, also, reveal the precise nature of the material referred to. In other words, it was thought that there should be an underlying arithmetical pattern or uniformity in distribution that explained particular AES distribution totals, whether this was founded upon individuals documents, the locality or, the smiths themselves (e.g. Lejeune 1961; Lang 1966; Chadwick 1973: 352-56, 508-11; Smith 1992-1993). Poor preservation, in a few cases, prevents us from observing the precise nature of each and every allocation which, combined with a few seeming arithmetical errors, has led scholars to believe that a full reconstruction is unlikely to be forthcoming (Bennett *et al.* n.d. lxxi). However, in the following analysis I suggest that this is largely an interpretive problem rather than a problem of scribal errors or preservation deficiencies.

Central to this problem has been the identification of the metrical units associated with the allocations and their absolute mass values, particularly the absolute value of the full mass unit L.⁴¹ The ratios of the fractional quantities expressed in the tablets are now well understood, but their corresponding absolute values have been more difficult to define. Conventionally, the approximation of the full unit as 30 kg has been, and continues to be, the figure used in the majority of studies despite considerable evidence to the contrary (Palmer 1963: 15; Chadwick 1973: 57; 1976: 102-05; de Fidio 1998-1999: 41-44). The derivation of a mass value at Knossos from the discovery of a decorated stone claimed as a weight standard corresponding in general terms to that of a talent, does no more than suggest that a reference existed for the measurement of this scale of object at Knossos. Whether or not this was indeed a talent reference at Knossos, there is no justification in transferring this to other regions or in applying this to an analysis of the Jn series. Nevertheless, this approximation continues to be used in documentary analyses.⁴² On this basis Smith's analysis concluded that the tablets concerned the distribution of different types of source material (*ibid*: 182-83). Thus, in those assigned to Group A, it was suggested that the distributions originated from complete or fragmented copper oxhide ingots, while it was suggested that Group B tablets, based upon the totals involved, defined allocations either of ingot fragments, possibly of bronze rather than copper, or from metals not in ingot form. While I agree that the use of copper ingots as a source for these allocations is very likely, pattern recognition in the AES totals requires far more subtlety in analysis, recognizing that considerable variability in the characteristics of the source material may be contained within the absolute figures and that the approximation of Mycenaean metrical values merely adds to the confusion.

Indeed, I suggest that there is now sufficient archaeological and comparative textual evidence to overturn this overly complicated scenario and to demonstrate that virtually all allocations in these tablets were of copper from a palatial stockpile of copper oxhide ingots. This evidence also shows that the insistence on an absolute figure for the mass of an individual ingot is both incorrect and has been fundamentally misleading in understanding

⁴¹ Virtually all discussions of this issue refer inaccurately to weight rather than mass. Weight (W) defines the force operating on an object of a specific mass (m) by the gravitational field strength (g) in the relationship, $W = mg$ the unit of which is the Newton (N). Within this relationship the field strength varies according to where that object is with the result that weight, unlike mass, is variable. Mass however, is a relative measurement of the quantity of matter within an object with respect to a mass standard, the unit of which is the kg (cf. Fenna 2002: 310-12). All published analyses, despite the continual reference to weight measurements, are in fact measurements of mass. This is a crucial distinction that bears heavily on ancient metrology since the equivalent of the international mass standard used today could not have been replicated in the Bronze Age. It is fundamentally incorrect to adopt an Aegean-wide standard absolute value of mass when the mass standard available at one centre would have varied from the mass standard at another.

⁴² Although Smith (1992-1993: 175) confusingly adopts two stances: that the standard mass of a copper oxhide ingot was approximately 26 kg from which the allocations were made, whilst maintaining in her analysis the standard AES L1 measurement of 30 kg previously mentioned.

the allocations of the Jn series. Undoubtedly the most important comparative evidence comes from the excavations of the Uluburun shipwreck where amongst a considerable cargo, perhaps bound for the Aegean, were 354 copper oxhide ingots whose provenance, although disputed, is most likely to be Cypriot.⁴³ These were found to have widely varying masses, ranging from 20.1 to 29.5 kg with an average mass of 23.9 kg, a variation which, even accounting for the effects of corrosion, was undoubtedly the result of the manufacturing process itself (Bachhuber 2006: 348-49; Hauptmann *et al.* 2002: 18; Pulak 1998: 193-98; 2001: 18; Lin 2003: 95-125). Similar patterns of variability were found in the substantial cargo of bun-shaped ingots apparently manufactured in three general sizes with average masses of 3.83, 5.71 and 8.26 kg respectively. Again however, the individual mass range was considerable, with the smallest intact ingot measured at 2.62 kg and the largest 10.52 kg (Lin 2003: 125).

Compositional analyses of both types revealed an unusually high level of chemical purity showing that the raw material utilized in the production process undoubtedly comprised pure copper ores. However, these analyses also revealed a manufacturing process producing copper with a high porous volume and significant levels of impurities, resulting in a relatively brittle product and one of low quality (Hauptmann *et al.* 2002: 4-5, 13, 19). Structural analysis at both macroscopic and microscopic levels further revealed that the ingots possessed a layered matrix symptomatic of a casting process involving multiple stages of pouring molten copper into the production mould, supporting the results of experimental work that suggest that smelting anything approaching the 30 kg of copper in contemporary smelting furnaces would have been extremely difficult (Merkel 1986: 257; Hauptmann *et al.* 2002: 17).⁴⁴ The continued assertion for the application of a mass approximation is, therefore, entirely misleading when applied to the analysis of these documents.

Indeed, if the mass approximation of 30 kg (or any of the suggested variations) is applied to the Uluburun ingots then a significant production error is introduced that would be difficult to explain or countenance. Between the average individual mass of an Uluburun ingot and the 30 kg approximation is a differential of 20.3% and for the smallest of these ingots the differential increases to 33%. This pattern of variable ingot mass is also repeated in the much smaller cargo of copper oxhide ingots recovered from the Cape Gelidonya wreck. The

⁴³ It should be noted that the dating of this wreck to approximately 1300 B.C reflects a situation around a century before the Pylos tablets were written. By contrast, the Cape Gelidonya cargo, discussed below, is broadly contemporary with the documents (cf. Pulak 1998: 191, 213).

⁴⁴ Although the archaeological evidence for the types of moulds remains scarce, there is none that supports the notion of a standardized production environment that would have enabled sufficient repeatability for the heterogeneity in production that the continued stipulation of a standard mass implies.

condition of these varied considerably, with some badly affected from contact with tin ingots and the resultant natural electrolysis that occurred, while others that were largely unaffected by corrosion had smaller fragments broken away. Nevertheless, the published data provides a significant pattern of mass variability summarized in table V-5 (Bass 1967: 52-57). Excluding partial ingots that were evidently also a part of the cargo, and one ingot whose preservation makes a reasonable assessment of the original mass difficult, the mass range is from 16 to 25.9 kg with an average of 20.44 kg.

Ingot (In)	Recovered Mass (kg)	Estimated Original Mass (kg)	Ingot (In)	Recovered Mass (kg)	Estimated Original Mass (kg)
1	19.95	19.95	18	17.00	19.00
2	21.75	22.30	19	22.20	22.20
3	18.50	19.50	20	21.90	21.90
4	18.15	18.80	21	19.70	19.70
5	22.00	22.00	22	10.70	21.00
6	20.70	20.70	23	15.50	22.00
7	19.00	19.10	24	17.00	17.00
8	20.80	20.80	25	21.30	21.45
9	20.75	22.20	26	16.00	18.00
10	15.60	16.60	27	23.80	24.10
11	16.85	18.00	28	18.00	21.50
12	23.40	23.60	29	16.85	20.00
13	25.90	25.90	30	21.80	21.80
14	20.00	20.00	31	14.95	18.00
15	17.50	19.50	32	12.85	16.00
16	20.20	20.35	33	19.35	19.70
17	23.00	21.75 - 22.00	34	10.90	??
Average ingot mass (estimated original mass excluding In 34) = 20.44 kg					

Table V-5: The mass of copper oxide ingots recovered from the Cape Gelidonya shipwreck (Bass 1967: 53-57).

With too many variables present ranging from the relative quantities involved, problems of chronology, the precise location of manufacture and the intended destinations and purpose of the cargoes, direct comparisons between the two data sets cannot be considered. Nevertheless the data show that ingots were being manufactured with considerable inherent variability and not to an 'international standard' or specification of mass. In neither case does the average mass correspond to the conventional approximation of a talent, or to the approximated absolute value of a full mass unit in Linear B; only the upper mass range of the Uluburun ingots approach this figure. The importance of this evidence lies in showing the potential variability in ingot mass that might be expected in palatial acquisitions of copper.

Although we might imagine that cargo deliveries to particular Mediterranean ports would have been separated broadly according to the local conventions or expectations of the size and mass of a talent, the variability that is apparent would have necessitated individual weighing and assessment at the point of transaction (Pulak 1998: 194; 2000: 264).

This situation is also reflected in the evidence provided by the balance weights from the Uluburun site where several sets were identified representing at least three separate weighing standards. Although the evidence for a specific set relating to Mycenaean measurements is scanty, as is the evidence for sets in the talent range, they do reveal several closely allied, but nevertheless distinct systems operating in the Mediterranean region (Pulak 2000; Alberti & Parise 2005). Indeed, analyses of these regional systems have revealed that the absolute value for a talent varied considerably and that, while the concept was common, no 'international standard' existed fixing the absolute value across regions. From the Aegean to the Near East, the variability in the absolute value of a talent was considerable; at Ashdod 23.5 kg has been suggested, while at Ugarit a talent weighed 28.2 kg and, finally, in the Aegean, 31.3 kg (Alberti & Parise 2005: plate LXXXIVa). The latter represents a considerable revision of the conventionally applied figure of 29 to 30 kg to the mass of the full unit L (Chadwick 1973: 57) and to the later study of the balance weights from Ayia Irini which, in contradiction to the majority of archaeological and textual evidence, concluded that an 'Aegean standard' did exist with the mass of a talent set at 29.28 kg (Petrucci 1992: 60-68).⁴⁵ The conclusions of this latter study must, however, be considered with caution since the methodology relies upon a process of mathematical extrapolation to determine the larger mass ranges from the smaller, and from a dataset that was derived from the express intention of determining 'the mathematical substratum on which a system was based' (*ibid*: 8). In other words, the methodology itself, in attempting to define the absolute value for a fractional quantity, disguises any variability that undoubtedly existed between sites and regions. With no means of accurately replicating a mass standard from region to region, while the concept of a particular quantity may have been common, the actual measurement of that concept would have varied. Therefore, whilst the concept of a 'talent' can be considered as constant throughout the Mediterranean it possessed differing absolute measurements from one region to another.

⁴⁵ This work was broadly supported by a later study, where the absolute figure for an Aegean talent was modified to 28.188 kg (de Fidio 1998-1999: 61). However, in my opinion, all of these studies fail to acknowledge the fundamental issue with respect to imposing such systems upon the Mediterranean Bronze Age world. Unlike contemporary society, there were no international standards providing a common mass reference against which to produce balance weights, nor any process of calibrating the production of such objects.¹

With this in mind, if we accept the possibility that the allocations were made from a store of copper ingots of variable mass and apply that principle to an analysis of the tablets then an interesting pattern emerges. In table V-6, rather than attempting to define patterns by applying absolute mass values to the documented figures, the working hypothesis starts from the premise that each document represents a unified allocation of source material; in other words that each defines allocations made on the basis of whole ingots of **variable** mass. In transferring this principle to the Pylian records we must be mindful that we should not necessarily expect to find convenient patterns of AES totals. If measurements of allocations were made with reference to individual ingot characteristics, variations will exist in the total mass of allocations made from a single ingot. Indeed, such errors conceivably transfer to individual totals on the Jn tablets. The sizes of individual allocations are potentially all fractions of the mass of a single ingot that would have had to have been deliberately broken into these amounts before distribution. The precise method used to achieve this is unclear, but we may surmise that whatever process was used, fracturing would not have been sufficiently controlled to produce precise divisions, as experimental investigations of ingot production suggest (Van Lokeren 2000: 276, fig. 3). Thus, where we have a totalling line on a tablet such as Jn 478 recording AES M26 but where the individual allocations amount to only AES M25 there may in fact be no error, let alone a scribal error. Controlling the fracturing of the ingot would have been extremely difficult and it is likely that such divisions were very imprecise, so that while an individual may be assigned AES M3, it is entirely conceivable that they would receive a mass slightly above or below this figure. It is noticeable that the smallest fraction used in the recording of allocations is N2, not the smallest unit available, but it does present a figure for an acceptable error margin between fractions.⁴⁶ Thus, if the totalling line on Jn 478.7 represents the mass of the source from which allocations were made, the error of AES M1 between that and the total of individual allocations may simply reflect the accumulated error in producing the seven allotments.

⁴⁶ It is only on the collection tablets that a mass division smaller than N2 is used; by implication the totals of N3 appearing on both Jn 829 and Jn 881 indicate that AES was to be measured to a resolution of at least N1. It is possible that the greater degree of accuracy in these collections is indicative of an expectation of contributions in the form of scrap bronze; a material whose very nature would lend itself to more accurate measurement.

Tablet (Jn)	Smiths		'Potnian' smiths		Extra AES	AES total mass issued	Possible ingot quantity	Mean ingot mass
	allocations	Total line	allocations	Total line				
310	M12		M12			M24	1	M24
320	M41+	M56				M56	2	M28
389	M22 N2	M27			M6	M27	1	M27
410	M6 +	M30 +				M30 +	1	M30+
415	M34	M34				M34	1	M34
431	M54	M54	M27	M27		M81	3	M27
478	M25	M26				M26	1	M26
601	M101	M104			M7	M104	4	M26
605	M9					M9	0.4	M25
658	M80	M80				M80	3	M26.67
692	M12					M12	0.4	M28
693	M16					M16	0.6	M25
	M26					M26	1	M26
706	M18+	M30+				M30 +	1	M30+
725		M78				M78	3	M26
	M16					M16	0.6	M28
		M12				M12	0.5	M24
750	M24					M24	1	M24
832	M3					M3	??	??
845	M12	M12				M12	0.5	M24
927	M11 N2+					M11 N2+	0.5	M24?

Table V-6: AES allocations in the Jn series and their possible derivation from copper oxide ingots.

If we apply the same principle to a particularly awkward tablet, Jn 320, we observe a similar pattern. Here the difficulty lies in the preservation of the tablet itself since the allocation figures of the initial entries on Jn 320.5-7 are marred by poor preservation. Jn 320.11, however, provides a total of AES M56 against which the individual totals may be referenced. Of 12 preserved allocations, nine are clear and account for AES M37. The initial entry of Jn 320.5 has been reconstructed as AES M3 and that of Jn 320.6 as AES M1, but both may be disguised by the damage to the tablet that has also robbed us of any evidence of an entry on Jn 320.7. Of the clear totals, allocations range from AES M3 to M5. If we reconstruct the three unclear allocations to AES M3 then we have a cumulative total of AES M46 and, with AES M5, a total of AES M52. If the latter is correct then the error between this cumulative total and the totalling line would be AES M4, suggestive of a single allocation not preserved on this tablet. The line immediately following, Jn 320.8, is largely unpreserved but could certainly have contained the single entry that would account for the error in the totals. As

such, any remaining discrepancy between the individual allocations and the total line would be comparable with that of Jn 478.

With these observations, it is possible to reassess the entirety of AES allocations and show that in all likelihood these were made from palatial acquisitions of copper ingots. In table V-6, the totals of all AES allotments on the 18 allocation tablets are presented along with an estimate of the possible quantity of ingots used as a source. Inevitably, such assessments can only be conjectural in light of the evidence for the variability in the mass of copper ingots of all types and that consequently no straightforward formula can be applied to the tablet figures. Moreover, even if the basic hypothesis is correct, the cumulative of the individual allocations and the totals recorded on the tablets is insufficient to distinguish between the multitude of permutations of oxhide and bun ingots that could have been involved. For example, in the case of Jn 832, where an apparently erroneous quantity of AES M3 is recorded, this could have derived from the accumulation of residues from the breaking of oxhide ingots, or represent the allocation of a single bun-shape ingot. Indeed, given that the text is itself different to the standard allocation documents in that it specifically relates to work responsibilities and the finishing of products, it is not inconceivable that in this instance the material in question is not copper but scrap bronze. Nevertheless, among the estimates of ingots used in each case there is a degree of consistency in the average ingot mass and range that corresponds well with the archaeological examples from Uluburun.

If we follow the arguments that I have proposed here concerning the nature of the so-called working tablets in that they are themselves part of the same sequence of allocations and that there is no replication of data then an interesting pattern emerges. Table V-6 shows a minimum total AES allocation of M711 N2; on four tablets, however, partial preservation robs us of a complete tally of the allocations. The final entry of Jn 750.10 can probably be safely reconstructed as M1 N2 since every other named allocation is of this quantity, increasing the overall tally for this tablet to M25 N2. In a similar fashion, Jn 927 appears quite likely to contain 12 entries with the same M1 N2 allocations despite the poor preservation robbing of us of two individual allocations completely and providing only partially preserved figures for five others. Such a reconstruction would present a total allocation on Jn 927 of AES M18 (table V-7).

Tablet (Jn)	Preserved AES allocation	Suggested reconstruction of the AES allocations	Increase
410	M30	L1 M2	M2
706	M30	L1 – L1 M20	0 – M20
750	M24	M25 N2	M1 N2
927	M11 N2	M18	M6 N2
Total Increase			M10 – M30

Table V-7: The estimated revision of AES allocations on the four tablets with incomplete entries.

Jn 410 and 706 are considerably more problematic to reconstruct, although with the former we can be sure that at least some additional mass of AES can be assumed, as the totalling line (Jn 410.7) preserves AES [L]1 M[. The value of the missing figure is a matter of conjecture, but given that there are four lines on which individual allocations may have been inscribed and that the character spacing visible on the preserved writing surface suggests a maximum of only two names to a line, then the tablet probably referred to eight individual allocations. Even though the figure of AES M6 is the only one preserved from the individual allocations (Jn 410.5), this does allow us to see that the overall total was probably only a little higher than what has actually been preserved on the totalling line. Naturally, we cannot assume that AES M6 was replicated for each entry, but variations in the allocations on other tablets are generally small, so an estimation of the Jn 410 total of AES L1 M2, based upon eight allocations of AES M6, is unlikely to be far removed from reality. With Jn 706, the greatest difficulty is presented by a missing fragment that bore the individual allocations of six smiths, as well as the incomplete nature of the totalling line (Jn 706.14) recording AES L1. It is possible, although unlikely, that with only a small fragment of the totalling line missing that the total may need no revision. However, given that four entries account for M18, if the minimum total was L1 the remaining six entries would have to amount to only M12 at an average of M2 each. This is clearly far less than anything preserved, even if Jn 706.3 was indeed M3 rather than M5 as would appear to be far more probable. It seems far more likely that the missing allocations are relatively close to those preserved; even if M3 is taken as the lowest allocation and the six missing totals were of this value, then the overall total would have amounted to L1 M6. If we assume that the maximum allocation amounts to 10 entries of AES M5 then we can justifiably assume a maximum range for this document of AES L1 – AES L1 M20. With these amended totals, it is now possible to attempt to derive a model of how the distributions and collections may have worked.

Some speculative calculations

It has long been recognized that these figures relate in some way to Ja 749 (recovered from the Chasm), also written by Hand 2, that records a total of AES M1046. Although missing the AES ideogram, this has generally been interpreted as a totalling document for the Jn series and therefore a tally of all AES allotments made. The obvious discrepancy between this total and the allotments that can be accounted for have been viewed both as a direct measure of the quantity of missing documents, and, by extension the number of smiths that must therefore have existed throughout the polity (Lejeune 1961: 433; Palmer 1963: 286; Chadwick 1973: 356; 1976: 140-41; Smith 1992-1993: 171-72 n. 4; Dialismas 2001: 122-23). There is however no evidence to suggest that a substantial quantity of documents that could account for allocations approaching half the total on the extant documents is missing. Indeed, the evidence presented thus far suggests that, in all likelihood, we possess all of this series that were written. To understand then how Ja 749 relates to the figures that we possess, we have to recognise that three basic assumptions have been made:

- ❖ that there is a direct relationship between this and the Jn series in that it represents a totalling document specifically for this series;
- ❖ that the total relates to allocations rather than a previous receipt or stored material;
- ❖ that the total represents all AES that has been distributed rather than a total from which allocations are seen to be made.

It is this last that I suggest encompasses the actual function of this document. Rather than relating to the Jn series, it would be more accurate to say that the Jn series relates to it, in that this total represents a tally of AES held by the palace prior to the allocations made to the smiths. In the following analysis, I attempt to show that this tablet records the probable presence of copper oxhide ingots stored in the palace from which allocations are made. These calculations will also suggest a close association between the provincial distribution of copper and the proportion of AES expected to be obtained from the two provinces in the collection tablets.

As has already been mentioned, Smith's interpretation of the division of tablets in the AC and the nature of the tablet groups relies upon locating the place-names mentioned in the texts within the Pylian territory. But, as she herself admits, the place-names of five tablets of Group A cannot be located and that of Jn 410 is not preserved (Smith 1992-1993: 190, n. 62). Only Jn 310 and 478 can be identified directly as listing places in the HP. Similar

problems exist for tablets within Group B exacerbated by the assumption that place-names on a single tablet must refer to the same province (e.g. Jn 725). The result is that many claimed to be located in different areas of the HP cannot, with any certainty, be accepted as such, despite this having been maintained in later studies (e.g. Nakassis 2006). Whether or not this geographical model can ever be proven, it is similarly difficult to disprove and, despite this criticism, is nevertheless a plausible enough scenario from which to examine the distributions further. There are, however, a further five place-names that remain problematic: on Jn 410 and 845 the place is simply not preserved whilst Jn 658, 706, 725 and 927 refer to three toponyms that cannot be, definitively, located in either province (cf. Sainer 1972: 37, 49).

Without the provincial identification of these five places, definitive conclusions concerning the AES distributions cannot be determined. Nevertheless, the figures presented in table V-6, and the emendations of table V-7, present a pattern that cannot be dismissed as entirely circumstantial. It is particularly noteworthy that distributions to smiths of the FP are markedly fewer than those of the HP, with only the one tablet, Jn 750, referring to *a-si-ja-ti-ja* that can be firmly located within the former. This apparent bias in the attention paid between the provinces is difficult to imagine as simply coincidental and, indeed, similar patterns of attention can be seen among other tablets, most notably the personnel documents of the Aa series. Here the 'major' subset, concerned with women of the HP, far outnumbers that of the 'minor' subset, concerned with the FP, by a ratio of 3:1 and, like the Jn series, these also have a clear distribution between the two rooms of the AC (Chadwick 1988).

Tablet (Jn)	Town / Area	HP Allocations	FP Allocations	Unknown Province Allocations
310	<i>a-ke-re-wa</i>	M24		
320	<i>o-re-mo-a-ke-re-u</i>	M56		
389	<i>a-ka-si-jo-ne</i>	M27		
410	??			M32
415	<i>ru-ko-a₇-ke-re-u-te</i>	M34		
431	<i>a-pe-ke-e</i>	M81		
478	<i>wi-ja-we-ra₂</i>	M26		
601	<i>po-wi-te-ja</i>	M104		
605	<i>a-pi-no-e-wi-jo</i>	M9		
658	<i>e-ni-pa-te-we</i>			M80
692	<i>na-i-se-wi-jo</i>	M12		
693	<i>a-ke-re-wa</i>	M16		
	<i>a-pu₂</i>	M26		
706	<i>pa-to-wo-te</i>			M50
725	<i>e-ni-pa-te-we</i>			M78
	<i>]-nu-we-jo</i>	M16		
	<i>a-ke-re-wa</i>	M12		
750	<i>a-si-ja-ti-ja</i>		M25 N2	
832	<i>a-to-mo</i>	M3		
845	??			M12
927	<i>]me-no</i>			M18
Totals		M446	M25 N2	M270

Table V-8: AES allocations according to province (including suggested maximum amendments from table V-7).

These basic patterns are, I suggest, meaningful and relate directly to the context in which the Jn series was written. However, even with the emendations I have suggested, the degree of accuracy that can be expected from any analyses would inevitably be insufficient to provide definitive conclusions. It is clear from Jn 750 that we cannot suggest that all allocations were restricted to the HP and neither is it reasonable to suppose that Jn 750 is the only representative of the FP. Certainly the suggestion that *e-ni-pa-te-we* should be considered as part of the HP simply because it appears on Jn 725 alongside names that can be identified with this province is questionable and takes no account of how the document came to be written (Sainer 1972: 37; Smith 1992-1993: 198). Similarly, the supposed ethnic derivation of *u-de-wi-ni-jo* (Jn 410.5) from the toponym *u-de-wi-ne* does not provide any justification for the placement of the allocations of Jn 410 in the HP (contra Nakassis 2006: 567). Currently, there is no definitive evidence to locate any of these places in either province, other than the long-standing assumption that documents containing multiple locality

references must contain a degree of geographical relatedness. So, for the purposes of this discussion only, I tentatively suggest that all of the unknown tablets be considered as FP allocations.

In these calculations, the figure of M3 on Jn 832 will be excluded. Although it is possible that this too was a fragment of copper ingot, within the context of work allocation for 'finishers' it is more plausible to suppose that this was a separate issue, possibly derived from scrap bronze. Adjusting for this caveat, the potential ratio in allocations between the provinces, using the amended allotment totals, is approximately 3:2 in favour of the HP, from an amount of AES that is 70.6% of that recorded on Ja 749. So, from a total of AES M738.5 recorded on these allocations, M443 appears in allocations to the HP and M295.5 to the suggested FP locations. If the remainder from Ja 749 (M307.5) were distributed at a similar event but one in which the FP was favourably represented in contrast to the HP, then allocations made according to this same ration of 3:2 would produce a FP allocation of M184.5 and HP allocation of M123. The cumulative totals between the two events would then equate to M566 for the HP and M480 for the FP. These allocations would represent 54.1% and 45.9% of the Ja 749 total respectively, with a differential between allocations of 8.2%.

When we consider the differences in the collection targets between the provinces, there are distinct similarities between the differentials in respect of the collections and the pattern of allocations that I am tentatively proposing. Although it is almost certain that Jn 829, alone, does not account for the collections expected, the contributions expected from the two provinces in respect of the *ko-re-te* and *po-ro-ko-re-te* is unequal. The cumulative total expected from officials of the HP at AES M24 N3 is M1 N2 more than that expected from the FP at AES M23 N1. In total these collections amount to AES M48 from both provinces, with the collections representing 51.6% and 48.4% of the total respectively, with a differential of 3.2%. The difference is relatively small, and without the clarity that would be provided by the information originally recorded on Jn 881 the precise differential remains uncertain, and may have been either rectified or exacerbated by these additional contributions. Nevertheless, allowing for the errors I have already mentioned, the pattern of collections between the provinces closely resembles the pattern of allocations considered by the palace. In other words, I suggest that it is at least a possibility that the uneven collection amounts between the provinces were a deliberate measure to acknowledge and reflect the difference in allocations between them.

It is possible to go one step further with the allocation figures and attempt to find a potential pattern in the number of copper ingots involved. In table V-6, I estimated the number of copper ingots that may have formed the basis of the allocations, alongside which I estimated the average mass in terms of the fractional quantity M. In light of the amendments made to the allocation totals on some tablets (table V-7) a revision of these estimations is necessary. Excluding the odd allocation of AES M3 on Jn 832, I estimate that 28 oxhide ingots of varying mass could have provided the basis for these allocations.

Tablet (Jn)	AES total mass issued	Possible ingot quantity	Mean ingot mass
310	M24	1	M24
320	M56	2	M28
389	M27	1	M27
410	M32	1	M32
415	M34	1	M34
431	M81	3	M27
478	M26	1	M26
601	M104	4	M26
605	M9	0.4	M25
658	M80	3	M26.67
692	M12	0.4	M28
693	M16	0.6	M25
	M26	1	M26
706	M50	2	M25
725	M78	3	M26
	M16	0.6	M28
	M12	0.5	M24
750	M25 N2	1	M25 N2
832	M3		
845	M12	0.5	M24
927	M18	1	M18
Totals	M741.5	28	M738.5

Table V-9: An estimation of the number of copper oxhide ingots providing the source for the Jn series allocations (including table V-7 amendments).

From the estimated average mass of each, an overall mean ingot mass of M26.38 is obtained, well within the range of the archaeologically attested examples from the Uluburun wreck.⁴⁷ Of these 28 ingots, 16.5 ingots could have provided the basis for the HP allocations and 11.5 for the FP allocations. The remaining 12 ingots (representing the unallocated AES M304.5), if they were distributed according to the ratio of 3:2 in favour of the FP, as suggested above, would result in further allocations of 4.8 ingots for the HP and 7.2 ingots for the FP. In this scenario, the overall ingot distribution between the HP and FP would be 21.3 and 18.7 ingots respectively. This would represent a 53.25% allocation against a 46.75% allocation respectively with a differential of 6.5%. Once again, although the figures are largely speculative, the pattern of ingot allocations mirrors closely the mass allocations as well as the distribution of collections.

Despite the speculative nature of the preceding analysis, it is difficult to accept that this pattern is entirely coincidental and, I suggest, that it is therefore reasonable to interpret the allocations and the parallel collections as being carefully managed by the palace such that the obligation to reciprocate (the collections) closely mirrors acts of patronage (the allocations). This patronage appears to be monitored according to the place at which smiths were operating or with which they were socially connected and that, overall, patronage is monitored at a provincial level. However, this provincial breakdown does demonstrate that no simple plan to manage allocations one province at a time was operating, but without a precise understanding of the geography (both political and physical), it is unlikely that we can fully define the reasoning behind the precise selection of the places involved in this event. Nevertheless, I suggest that the tablets are the physical manifestation of a process of the public negotiation of patronage, in which the composition of records plays a pivotal and visible role in the conferment of patronage and the imposition of obligations. In this instance, allocations appear most likely to have been made from a palatial stock of copper oxhide ingots, probably totalling 40 in number, recorded collectively on Ja 749. This tablet, rather than simply acting as an accounting device such as an inventory tally may, within an oral context of distribution, have acted as an additional proof or demonstration of the actions of the palace in respect of each region. Indeed, unlike the Knossos examples, it is noticeable that this tablet records the quantity of copper according to mass rather than the number of

⁴⁷ It is worth noting here that KN Oa 730 apparently records 60 ingots with a combined mass of AES M1562 (Killen & Olivier 1989: 265). Here, the average mass of each ingot would be M26.03 (Chadwick 1976: 142), a figure that does not concur either with the conventionally assumed mass of a talent or that of the stone weight found at Knossos thought to be the reference against which the mass L was measured. Applying the lower suggested absolute figure for L (29 kg) the average absolute mass would be 25.16 kg, and with upper value (31.3 kg) the average mass value would be 27.13 kg. This average mass does however correspond closely to the figure that I have proposed for the Pylian source material.

ingots. With so much variability in mass between ingots, and the possibility of different ingot forms being present, one provincial distribution could appear to inordinately outnumber the other in the actual number of ingots, but not in terms of their mass. Ja 749 could then be used to demonstrate this alongside the allocation documents themselves as well as to act as a foil against which the distributions at this event could be monitored. In this respect, it is interesting to note that the find-spot of Ja 749 places it in the area of the Chasm towards the southern extremity of Room 7 which, I would argue, represents an area where some of the last tablets to be returned to this complex were placed following the event which saw the distribution of gifts of armour, chariot wheels and copper. As such, it is not surprising to note that this location places it in close proximity to some of the last allocation documents of the Jn series.

The public display of a stockpile of copper oxhide ingots in the kind of stylized and ritualistic event that I am proposing would have provided the palace with an unparalleled opportunity to display its power, wealth and prestige. These ingots would themselves have to have been broken up to produce the individual allotments and although the means by which this would have been achieved remains unclear, it is nevertheless evident that such a process would have made a memorable spectacle. The conferment of patronage appears to be a very public event in which the palace utilizes all possible tools at its disposal to reinforce its beneficence as well as the imposition of obligations on the recipients, from visual and oral/aural devices through to the 'mysterious' power of the written word. In the following chapter, the manipulation of such devices, the conferment of patronage and palatial involvement in the construction of Mycenaean social identity will be explored further in relation to individuals and groups in two contentious documents, An 607 and Tn 316.

Chapter VI

The public face of palatial patronage? The political sanctioning of social identities.

Endorsing appointments: a re-evaluation of An 607

One of the most enigmatic and contentious documents found in Room 8 of the AC is An 607. This page-shaped document was written by Hand 1 and ruled for 14 lines, but with the substance of the text confined to the first 8 lines. Whilst the text itself is markedly formulaic in content, the layout is noticeably disorganized with no single entries to a line but rather flowing from one line to the next, a feature that led Chadwick (1976: 83) to describe its writer as 'incompetent'. As we shall see, this essentially aesthetic judgement has little foundation, but the adopted layout has undeniably led to problems separating individual statements and created a degree of contention concerning their correct reading and interpretation. These difficulties have been compounded by the seemingly unique nature of the document, with scholars unable to unequivocally identify any obvious parallel. Moreover, with much of the vocabulary poorly understood or simply untranslated, and with few comparable documents containing these terms, An 607 remains the subject of continued controversy. As I hope to show, these difficulties are far from insurmountable and that through considered analysis and, in particular, through a reconsideration of vocabulary assumed to be understood, a coherent translation of this document is possible and that parallels do indeed exist.

However, it is difficult to immediately see the repetitious and formulaic nature of the composition from the standard transcription. By slightly re-arranging the text to show the individual entries line-by-line this feature becomes readily apparent and shows, in all likelihood, the intended structure (Bennett 1961: 8-9; Heubeck 1985: 62).⁴⁸

⁴⁸ References to lines of the document in this discussion will refer only to the standard tablet transcription, the text of which appears in Appendix B.

me-ta-pa, ke-ri-mi-ja, do-qe-ja, ki-ri-te-wi-ja
do-qe-ja, do-e-ro, pa-te, ma-te-de, ku-te-re-u-pi MUL 6
do-qe-ja, do-e-ro, pa-te, ma-te-de, di-wi-ja, do-e-ra MUL 3
do-qe-ja, do-e-ra, ma-te, pa-te-de, ka-ke-u MUL 1
do-qe-ja, do-e-ra, ma-te, pa-te-de, ka-ke-u MUL 3
do-qe-ja, do-e-ra, e-qe-ta-i, e-e-to, te-re-te-we MUL 13

An 607.1 appears to act as a header or title line in much the same way as the initial line of Jn 829. Contrary to the views of Tritsch (1958: 411, n.11), the inclusion of *ki-ri-te-wi-ja* in significantly smaller signs at the end of line 1 suggests a desire to squeeze the header into the first line, rather than an attempt to include a word belonging to another line. This form of composition with both variations in sign height and crowding is not unfamiliar amongst the corpus, whilst the use of punctuation appears to confirm that it is meant to complete line 1 (Bennett 1961: 6, 8-9). Further corroboration of the separation of statements between lines 1 and 2 is provided by the obviously formulaic pattern visible in the re-arranged text. Each entry begins with the phrase *do-qe-ja do-e-ra/o* and ends with a figure for the number of women being discussed. In four of these cases (lines 2-3, 5-6, 6-7 and 7-8), this phrase is followed by a statement of parentage indicated by the terms *ma-te* and *pa-te*. Confirmation that the figure in line 4 is the sum of the individual entries of the number of women was provided from a consideration of the compositional process (*ibid*: 7-8). Two stages of writing are visible with a clear separation between lines 4 and 5, and the total of 13 overwrites an earlier number corresponding to the 6 women listed in line 3. Evidently lines 5-8 were a later addition necessitating the alteration of the figure in the original concluding statement.⁴⁹

The text itself has proved just as difficult to interpret and controversy has surrounded much of the vocabulary with many words remaining untranslated. Yet, I suggest, these issues have themselves been driven by one underlying theme maintained in all interpretive schemes

⁴⁹ The use of the KA ideogram in the middle of the 11th line remains problematic. No parallels for such a usage exist and so any interpretation is largely speculative (*ibid*: 6; Bendall 2007: 249, 251). This sign appears as an adjunct to SUS on Un 6 and Un 853 and as an ideogram on Un 219 designating an unknown substance, possibly an aromatic, given to a group of seamstresses. On Vn 1314.3.A it has been tentatively identified as an ideogram, although it remains possible that in this case the sign is actually the figure for 100. There would appear to be no connection between these and An 607. The possibility that this is an abbreviation, however, cannot be excluded and the question of whether this may be the mark of the scribe must, therefore, also remain open. However, given the previous observations concerning the stages of information entry it is not inconceivable that this sign was added simply to make it clear that the document was complete and that despite the additional ruling, no further entries were expected or required. Certainly, if the scribe was expecting further information to be incorporated that subsequently became unavailable or unnecessary it would account for the cramped nature of the writing that led Chadwick (1976: 83) to the conclusion concerning the scribal abilities of the author.

proposed and that is the issue of slavery. This document has often been regarded as one of the most significant for discussions of Mycenaean slavery, if only it could be satisfactorily translated (e.g. Deger-Jalkotzy 1972; Chadwick 1976: 83). Superficially, the repetitive appearance of the words *do-e-ra/o* appears to support this conclusion. Of all the vocabulary in this text, this has been regarded as probably the most secure in terms of translation and semantics, but it is the assumed semantic equivalence with the Classical *δουλος* that creates innumerable problems in interpreting this document. Indeed, one of the major studies of this text was fundamentally centred on this term and how it may reveal the nature and dynamics of Mycenaean slavery through analogy with contemporary Near Eastern cases (Deger-Jalkotzy 1972). As I intimated in the previous chapter though, maintaining this semantic equivalence may well be untenable and that whilst the word undoubtedly reflects a social bond or relationship, it is not one of enforced servitude.⁵⁰ Here, I suggest, that within the context of religion and cult the relationship is probably more analogous to the concept of acolytes, neophytes or novices bound to the charge of the religious or cultic orders in which they are being trained.

This term, nevertheless, represents the starting point for analysis through its association with the controversial word *do-qe-ja*, which the rearranged transliteration shows is clearly a significant element in the overall composition. The debate over the meaning of *do-qe-ja* essentially falls into three camps, those who view it as referring to a deity (Adrados 1957; Palmer 1963: 128; Lindgren 1973b: 40; Billigmeier & Turner 1981: 8; Carlier 1999: 186), those who argue that it is a description of the women being documented (Tritsch 1958: 412, n.12; Deger-Jalkotzy 1972: 140, 153-55; Chadwick 1973: 167) and finally the suggestion that it refers to the mistress of the *do-e-ra/o* listed as parents (Bennett 1961: 10). That *do-qe-ja* is the name of a deity is the most commonly held view and it is this interpretation that I shall follow. Several arguments favour this reading, not least of which is the clearly

⁵⁰ The direct association of *do-e-ra/o* with the translation of 'slave' has not been without its detractors. Dissenting views have been expressed in several studies where the contextual use of the terms appears to contradict this translation (e.g. Billigmeier & Turner 1981; Uchitel 1984: 278-81; Hooker 1995: 9-12; Efkleidou 2004; Nakassis 2006: 91-96). The pervasiveness of the conventional translation is difficult to counter but the weight of evidence against this interpretation is nevertheless extreme. In my opinion, the equation *do-e-ra/o* = 'slave' has become an example of historical dogma. Whilst the equivalence with the Classical word *δουλος* is itself undeniable, semantic equivalence is far from proven. It is entirely feasible that the Classical meaning developed from a rejected socio-political environment in which this title was perceived as defining a subservient relationship between a patron and client. Similarly negative semantic developments are to be found with the Old English word *wealh*. Originally a noun that had the meaning 'Celt' or was perhaps more specifically a tribal name, the semantics gradually became corrupted such that it developed meanings that included 'foreigner' and 'slave'; eventually obtaining a pejorative gloss when applied in particular contexts (Faull 1975). Restrictions of space prevent the inclusion of a detailed study of this issue conducted in the course of this research, but there is a strong argument to be made that *do-e-ra/o* refers not to slaves or servants but effectively to patron-client relationships, whether secular or divine (Peters, in prep (b)).

formulaic lexical structure of the document and the consistent combination of *do-qe-ja* with other titles. We see it used once in combination with *ki-ri-te-wi-ja* and five times with *do-e-ra/o*, mirroring other well known formulae (e.g. *i-je-re-ja do-e-ra/o* and *te-o-jo do-e-ra/o*). This compound form suggests a similar relationship in which we see the *do-e-ra* of *do-qe-ja*. Indeed, this pattern has been noted by Carlier (1999: 187) who, although maintaining the interpretation of *do-e-ra/o* as slaves, observed that the *do-e-ra/o* are always specified with the genitive and thus they are *do-e-ra/o* of a named or titled individual, or a divinity.

If we accept this combination then the logic behind the composition of the text becomes clear. Five of the references to *do-qe-ja do-e-ra/o* relate specifically to the parentage of all of the women concerned. In each case a *do-qe-ja do-e-ra/o* parent is listed first, although no gender distinction appears to be involved; in two entries the father is described first, whilst in the other two instances it is the mother. The fact that the two entries recording the father as *do-qe-ja do-e-ro* appear before the two instances of the mother may suggest that some form of 'gender hierarchy' exists, but with only four entries to base an interpretation on, no significance can be determined. So, whilst the parental order may be meaningful, it is the identification of one parent as *do-qe-ja do-e-ra/o* that is the primary consideration, not as has been suggested that it is simply the *do-e-ra/o* designation determining the order (Deger-Jalkotzy 1972: 138).

Contextually, it does appear that status was inheritable through either parental line but the suggestion that this points to an overall conclusion that religious status was inheritable through either parental line (Billigmeier & Turner 1981: 8, 15) is, I suggest, an overstatement and over-generalization. As we shall see in relation to the *ki-ri-te-wi-ja*, gender appears to be a significant factor in this group's identity but without comparative cases of parental concerns, we cannot be certain that the conditions defining membership of other religious groups was not based upon gendered criteria. Moreover, we have no means of establishing how the relationship with *do-qe-ja* was originally established. After all, An 607 recalls the parentage of 13, presumably young, women and their association with *do-qe-ja*, yet from the parental descriptions we see both men and women associated with *do-qe-ja*. Despite An 607 clearly reflecting the selection of women alone, the relationship with this deity could not have involved any gender discrimination for there to be *do-qe-ja do-e-ro* at all. We may conclude, therefore, that whilst both men and women could become associated with *do-qe-ja*, qualification and acceptance of membership within the *ki-ri-te-wi-ja* was reserved for women alone.

Notwithstanding the problems of interpreting these gender issues, the key feature is the use of the compound title *do-qe-ja do-e-ra/o* as a description of the women. Significantly, we see this formula emphasized over important craft positions (the smiths of lines 6 and 7) and the well-known *di-wi-ja do-e-ra*, in the case of the mother in line 5. So, in this unique instance of parentage being included in a list of personnel, it is the bond or relationship between at least one parent and *do-qe-ja* that is the determining feature. This observation leads us to question the meaning and relative status of *do-e-ra/o* since it is clearly the relationship between one parent and *do-qe-ja* that is used to validate the inclusion of the women within the group described in line 3, not the social status of the parents (Carrier 1999: 187). Conversely, why should the *do-e-ra/o* be the significant partner if, as has been generally understood, this list reflects the mixed parentage of slaves and non-slaves? If the traditional translation of *do-e-ra/o* as slave (or even servant) is maintained, then there would be a social incongruity which could only be explained by arguing that all of the parents were in fact unfree (Deger-Jalkotzy 1972: 151-53). This of course, creates even more problems with notable craft positions becoming the province of the slave and, ultimately, we would have to question the very purpose of the palace having any interest in recording details that are a rarity even amongst those who could be considered part of the elite. The only conclusion that can be drawn is that An 607 concerns free persons and that *do-e-ra/o* does not refer to slaves. Furthermore, there is no indication from this document that relative social status is of any concern contextually, or that any straightforward hierarchy of social rank can be deduced. The deliberate inclusion of parentage unrelated to *do-qe-ja* may indicate that both parents were required to be of a certain status, but what that may have been is uncertain. I suggest, contrary to the established consensus that this shows that all of the people recorded are free but whether there is an underlying cultic or religious connection between them is far from clear.

Such an association undoubtedly exists for those described as *do-qe-ja* or *di-wi-ja do-e-ra/o*, and it is entirely possible that the smiths have a similar cult connection, but it is not at all obvious how this may be applicable to the mother described as *ku-te-re-u-pi*. This is conventionally interpreted as an ethnic or locational designation, possibly 'Kytheran', but a reference to a location within the Pylian region cannot be excluded and is perhaps more likely (Chadwick 1973: 167, 420; Deger-Jalkotzy 1972: 140; Heubeck 1985: 71-76). Overall, it is consistent with the pattern of specifying parental identities, but there is no corroborating evidence to suggest that this woman was ever a slave or, as has been described, an individual 'of no relevance' (Deger-Jalkotzy 1972: 140, 152). Within this context the identification may be considered unusual compared to the other descriptions, but it is a consistent means of identification amongst the tablets for individuals to be identified

either specifically to a place or through an ethnic adjective. In this instance, viewing the mother as a slave combines facets of the slavery tautology surrounding this document with the assumption that the place referred to was a flax producing region and that she was a flax worker (based upon a reference to *ku-te-re-u-pi* in Na 296). As such, it has been assumed that the meniality of such a task supports the slavery interpretation. This conclusion, based solely on a modern, value-based assessment is, in my opinion, unjustifiable. The alternative suggestion that the name derives from a place associated with ceramic production or pottery workshops, whilst interesting, does not aid in the overall interpretation of An 607 (Heubeck 1985: 76). There is, therefore, little evidence to suggest that this woman was anything other than free and that her status, whilst perhaps lowly with respect to the palatial elites, was significant enough contextually to be recorded and allow for the qualification of her daughters as members of the *ki-ri-te-wi-ja*.⁵¹

The remaining example of the use of *do-qe-ja* in An 607 sees it combined with the problematic term *ki-ri-te-wi-ja* in the heading line and it is this combination that has added to the interpretive confusion (Heubeck 1985: 65). At first glance it seems incongruous to have *do-e-ra/o* and *ki-ri-te-wi-ja* associated with *do-qe-ja*, as both seemingly apply to the women enumerated in this text, but as I shall argue, such a relationship is entirely possible if it reflects a change in the social position of these women. It has long been recognised that *ki-ri-te-wi-ja* refers both to a group of women and also has cult or religious connections, but contextual information is limited and does not allow for precise interpretations of their function or attributions, and the etymology is far from conclusive (Aura Jorro 1985: 363). Indeed, etymological studies have, at first glance, provided a perplexing array of possible meanings, from 'barley' to 'anointed' or 'chosen, exquisite' (Ruijgh 1967: 129; Lindgren 1973b: 81-82; Billigmeier & Turner 1981: 13, n 30; Aura Jorro 1985: 363). Whilst these could all be said to have religious or cultic connotations, no individual interpretation has

⁵¹ Neither can we infer much with regard to the relative positioning of the deities mentioned; with a lack of contextual information in which to situate the document within the religious concerns of the state, we cannot suggest any hierarchy or relative significance to them. The same observation, incidentally, applies to the argument that *do-qe-ja* cannot be the name of a deity since the name does not occur on any other documents (Deger-Jalkotzy 1972: 139). Without wider contextual information, particularly with regard to the timing of this and other religious events, we cannot assess whether this simply relates to a time of the year in which other deities are far more prominent in social and religious concerns. Neither should we forget that this problem also occurs with other well-known deities amongst the Pylos texts that are recognisably early forms of the Olympic pantheon; *e-ra* (Hera) occurs twice, *a-re* (Ares) appears twice on texts from Knossos but only as two/three instances of theophorics on tablets from Pylos (Gulizio 2001: 34), and *e-ma-a₂* (Hermes) only in three Pylian texts (Gulizio 2000). In any interpretive scheme we must accept, therefore, that the document concerns a process that is very specific spatially and temporally and that generalized statements concerning relative status are largely untenable. Moreover, in this very specific instance the document contextualizes the events precisely around the figure of *do-qe-ja*; all of the key relationships and concerns surround this personage and as such we should not expect any individual, human or divine, to appear in anything other than an apparently subordinate role.

unequivocally been accepted despite the inherently reasonable morphologies (Aura Jorro 1985: 363). Foremost amongst the possibilities though is the connection between *ki-ri-te-wi-ja* and barley (*ki-ri-ta*) potentially deriving from *Kritheus*, the ‘god of barley’ (Ruijgh 1967: 129; Deger-Jalkotzy 1972: 154; Chadwick 1973: 167; Palmer 1992: 485-86; Killen 2004: 167), which would in turn imply that *do-qe-ja* is an agricultural deity (Adrados 1957) and that the responsibilities of the *ki-ri-te-wi-ja* are in some way representing this divine interest or intervention in the agricultural cycle.

There is however a tendency to view the range of possibilities as mutually exclusive and to assume that, if the correspondence with barley is accepted, the other etymologies cannot work (Palmer 1992: 486). I suggest, however, that An 607 shows that this need not be the case and that the root of this controversial term actually combines facets of all of these potential meanings. After all, An 607 is explicit in showing a careful process of selection determining the members of the *ki-ri-te-wi-ja* and also, as I shall argue, it is likely that this document concerns a ceremony of investiture or inauguration of this group. In this context, we can see all of the aforementioned meanings potentially contained and subsumed within this particular group identity. Extending this interpretation, however, to argue that they are somehow involved in the processing of barley is, in part, derived from the base assumption that these women are of servile class (e.g. Palmer 1992: 485-86; Heubeck 1985: 70). There is no evidence to support this and in the context of this document it would appear to contradict the need to record the ceremony or event under consideration. Evidence from other documents indicates that this group, whilst not of elite status, nevertheless held a significant rank as well as responsibilities within the social system. Their relationship to other religious or political officials is particularly evident in documents such as Kn E777, Ep 704 and Un 1426 in which they appear in relation to the *wanax* and the results of this analysis of An 607 clearly reveal an important religious or cultic role that required a singular event establishing the group within the community. As such it is inappropriate to view them as a purely labour group.

However, if we accept that the *ki-ri-te-wi-ja* is connected to *do-qe-ja* and that *do-qe-ja* most likely refers to a divinity, then it is possible to offer a likely interpretation that potentially incorporates all of the suggested etymologies. Syntactically, it appears probable that the 13 *do-qe-ja do-e-ra* are themselves, collectively, the *do-qe-ja ki-ri-te-wi-ja*; the ‘religious corporation’ that has generally been accepted as defining the *ki-ri-te-wi-ja* (Tritsch 1958: 418; Palmer 1963: 128; Lindgren 1973b: 82; Billigmeier & Turner 1981: 7, 13, n. 30; Killen 2004: 166-67). As such An 607 reveals varying forms of identity expression: the difference between a collective entity (*do-qe-ja ki-ri-te-wi-ja*) and the individuals belonging to that

collectivity (*do-qe-ja do-e-ra*). But what conditions allow for this change in identity expression?

It seems most likely that the issue of parentage provides the explanation. These statements are seemingly included to establish or validate the suitability, and hence the qualification, of the 13 women as *do-qe-ja do-e-ra* to be, or become, collectively the *do-qe-ja ki-ri-te-wi-ja*. It is also clear that these women are to be brought together under the jurisdiction or responsibility of the *heq"etai* at *me-ta-pa*. The dative, plural form *e-qe-ta-i* indicates a group of *heq"etai* as 'recipients' of the 13 *do-qe-ja do-e-ra*, but in no way does this text indicate ownership (an interpretation that is commonly levelled but one which in no way relates to the syntactic construction but simply to semantic assumptions that the *do-e-ra/o* are slaves). It should be noted, however, that Bennett (1961: 12) suggested that functionally the form *e-qe-ta-i* could also be instrumental, which would clearly present several possibilities for interpreting this difficult line. Whatever the precise form, it does not provide any direct indication of how many *heq"etai* were to be gathered at *me-ta-pa*; we can only be sure that grammatically a group, rather than an individual, is referred to.

There is, however, one intriguing possibility concerning the numbers of *heq"etai* involved that relates to the total number of this group that have been previously identified (primarily through studies of the An series) for there appears to be a correspondence between their total number and the numbers of individuals forming the *ki-ri-te-wi-ja* on An 607. Although this remains the subject of some disagreement, analysis of the *o-ka* texts identified at least 12, and more likely 13, *heq"etai* (Lindgren 1973b: 46-47). With 13 members of the *do-qe-ja ki-ri-te-wi-ja* associated with them in An 607 we inevitably have to ask, what is the relationship between these groups and is there any significance in the correspondence between their numbers? Naturally, we cannot be sure how many of the identified *heq"etai* held this position at any one time; how accession was made to this post or for how long each individual retained this designation and this may explain the recent, tentative suggestion that 14 *heq"etai* can be identified (Nakassis 2006: 125-31). In itself, the argument to identify *ko-ma-we* in An 519 as a *heq"etas*, is unconvincing. Those listed in the An series are consistently described in a highly formulaic manner, which this individual is not accorded. Nevertheless, it is possible that the similarity in syntactic construction surrounding *ko-ma-we* reflects an individual that was previously credited with the title of *heq"etas* and the assertion that this individual was of some importance is undoubtedly correct (*ibid*: 131). The fact that the numbers of *heq"etai* and *ki-ri-te-wi-ja* may correlate could simply be an interesting coincidence, but one in which the precise significance of the number in either case is far from obvious. We cannot, for example, suggest that they correspond to simple geographical

issues such as the number of districts of the two provinces. However, the accumulation of evidence leads me to suggest that it is not simply coincidental, for the connections between these groups extend beyond the question of numbers.

Both groups are defined or identified in part by their parentage and would appear therefore to incur similar aspects of qualification and justification in membership of their respective group. The expression of parentage in the case of the *heq^uetai* concerns only the paternal line through the use of patronymics, but the general rarity of these and other indications of parentage suggest that the inclusion of this information is significant to their identity as *heq^uetai*. Coincidentally, the use of these patronymics suggests another point of similarity between the women of An 607 and the *heq^uetai*, with *a₃-ko-ta* and *di-ko-na-ro* both recorded with the same patronymic *a-da-ra-ti-jo*. It seems likely that these two are, indeed, brothers mirroring the apparent selection of groups of siblings in An 607 (Nakassis 2006: 343, 407, 416). It must be stressed, though, that with not all *heq^uetai* bearing a patronymic in the texts, there is a lack of surety as to the significance of these epithets. It may be that they simply reflect the high status that the *heq^uetai* enjoy (Chadwick 1973: 121) but it seems more likely that their use validates participation in certain activities or permits the individuals to assume the responsibilities that the role of *heq^uetas* entailed (Carlier 1999: 193; Nakassis 2006: 344-45).

Whatever the precise relevance of these identity traits is, I suggest that they betray a significant relationship between the *heq^uetai* and the women of An 607; a relationship reinforced by both groups appearing together in the land records (Lindgren 1973b: 48). A direct relationship in this respect is provided by Eb 321 and Ep 704.4, in which the lease of a substantial plot of land to the *ki-ri-te-wi-ja* from the *damos* appears. Alongside this are recorded the land holdings of the priestess *e-ri-ta*, and the keybearer, *ka-pa-ti-ja*, but it is the summation tablet, Ed 317, that establishes a contextual association between the *heq^uetai* and the *ki-ri-te-wi-ja*. Here, the *heq^uetai* appear in conjunction with both *e-ri-ta* and the keybearer as well as the priest *we-te-re-u*. It seems unlikely, as will be argued below, that the *ki-ri-te-wi-ja* appearing in these tablets are the same as those referred to in An 607, but nevertheless, these few tablets clearly place the *ki-ri-te-wi-ja* and the *heq^uetai* into a close association with key religious or cult personnel. It is, I suggest, not surprising therefore that we see the individual members of the *ki-ri-te-wi-ja* coming under the jurisdiction of a group of *heq^uetai*. Although doubt remains, I suggest that this group does indeed constitute all of the members of the *heq^uetai* and that their numbers have an important ceremonial correlation with the *ki-ri-te-wi-ja*. Even were this not the case, to have the presence of more than one *heq^uetai* attested at any one location is unusual, marking this out as a special event (Hooker

1987: 266). Why then, does this document appear to contradict this pattern? Not only would there have to be a close association between the two groups concerned but it would also require a unique circumstance, one in which their presence is essential either as witnesses or direct participants.

It seems likely then that the gathering of both the *heq^uetai* and the *ki-ri-te-wi-ja* at *me-ta-pa* is for a singular purpose, but in order to determine what this was or how it was organized the remainder of lines 1 and 3 must be considered. In doing so, it is worth recalling the situation described by Palaima (2004b: 103) for Un 718 in relation to the registration of contributions from various parts of the polity. In this case, the material contributions to a ceremony for Poseidon are relayed into the hands of a specific, controlling group – the *o-wi-de-ta*. This suggests that in the case of certain ceremonies or festivals the central components of that event are regulated and controlled by responsible groups, which we are further justified in suggesting is determined by their active engagement in those ceremonies. In the case of the *o-wi-de-ta*, their engagement is clear; as those performing the animal sacrifice to Poseidon, their importance cannot be doubted. Extending this principle to the *heq^uetai* of An 607, the lexical form is suggestive of their active engagement in an event concerning the 13 women for whom they are responsible. As I shall argue below, this event would appear to involve the ‘initiation’ or ‘inauguration’ of the 13 *do-qe-ja do-e-ra* by the *heq^uetai* (possibly acting as the ‘initiation priests’) as the *do-qe-ja ki-ri-te-wi-ja*. If we assume that the numbers in both groups are indeed no coincidence, it begs the question as to whether the *heq^uetai* were in some way effectively the patrons of these women, or the designated representatives of palatial patronage.

Unfortunately we are hindered in our interpretation of the remaining key lines by the words *e-e-to*, *te-re-te-we* and *ke-ri-mi-ja* all of which remain either untranslated or the subject of continued debate and controversy. Yet it is likely that the problematic *e-e-to te-re-te-we* holds the key to understanding the roles of all of these groups of people if it addresses some notional function for the women, or describes the role of the *heq^uetai* themselves, but it may also simply be a statement to the effect that jurisdiction or responsibility is placed on the *heq^uetai*. Several interpretations of *te-re-te-we* have been offered, but the problems surrounding this term have led scholars to maintain that it should remain untranslated (Lindgren 1973b: 145, n.5; Chadwick 1973: 168; Aura Jorro 1993: 340). Nevertheless, the suggested interpretations do offer possibilities of understanding this document. These are essentially restricted to personal designations (titles) in either a dative singular or nominative plural form, or to a place-name (Bennett 1961: 12; Palmer 1963: 128). The last would seem to be the least likely given that the heading line serves to locate these events. The former,

possibly linking it with *te-re-ta* and thus the *telestai* (Adrados 1957: 56), has also come in for criticism (Heubeck 1985: 81-83). Nonetheless, it is this that appears to offer the greatest possibilities syntactically. Deger-Jalkotzy's (1972: 157) argument that it should be considered as defining the purpose of the women going to *me-ta-pa*, whilst plausible, need not be necessary if the purpose is explicitly contained within a functional attribution for the *heq"etai*.

Given the undoubted cultic context, the interpretation based upon *te-re-ta* seems eminently logical but determining the case is problematic, although this is perhaps not entirely necessary to provide the sense being conveyed. If we accept that the word derives from the same root as the title *telestai*, then we may understand the word as being involved with 'initiation' or 'initiators'. If a passive reading of *te-re-te-we* were taken then it may be read simply as 'initiation' or 'for initiation', but if the nominative plural were taken then we may suppose that it applies to the *heq"etai*, and that they are acting as 'initiators'. In the absence of any direct evidence I suggest that the passive reading is taken, but either conveys the purpose behind the relationship between the individual *do-qe-ja do-e-ra* and the *heq"etai*. We can either understand it as the *heq"etai* overseeing the initiation of the women into a new role as the *ki-ri-te-wi-ja* or, more actively, as the *heq"etai* performing the initiation.

Probably the least controversial aspect of the problematic third line concerns the subject of the verb *e-e-to*, which undoubtedly refers to the 13 *do-qe-ja do-e-ra* (Adrados 1957: 55; Bennett 1961: 12; Ruijgh 1967: 365, n. 66; Aura Jorro 1985: 203-04; Heubeck 1985: 76; Carlier 1999: 186). There is, however, considerable disagreement when interpreting the form it takes. In essence, this debate revolves around the timing of the events portrayed and whether *e-e-to* signifies events that have happened or are yet to occur.⁵² Generally, there is a tendency when interpreting these documents to view them as records of events that have already occurred despite that a few, including Un 718, Jn 829 and possibly Tn 316, Cn 3 and Cn 608 are prospective. It is certainly the case that the majority appear to record events that have happened or situations that are ongoing, yet this can easily become an unsubstantiated assumption. Indeed, in this instance, this crucial word was not even considered by Deger-Jalkotzy (1972: 157); the meaning apparently assumed to be relating to past events when considering the construction and interpretation of lines 3-4. This generalization is also apparent in the rejection by Carlier (1999: 186, n. 9; also Chadwick 1973: 168, 420) of an

⁵² The majority of opinion has favoured a reading of, 'they were sent' (Ruijgh 1967: 365, n. 66; Heubeck 1985: 76-77). However, it has also been proposed that *e-e-to* could be understood as a 3rd person, future, 'will be' or 'will go' thus defining events that were yet to come (Adrados 1957: 55). This has generally been rejected, but the grounds upon which this rejection is based are themselves controversial.

imperative meaning on the basis that no others exist in the Mycenaean archives; as such it was argued that the perfect tense should be assumed. However, this cannot be considered as anything but circular logic. Since the majority of the documents are relating past or present occurrences the numbers of occasions in which an imperative may occur are inevitably going to be limited and so it can be no surprise that amongst the extant documents the evidence for these is anything other than scanty.

With no consensus achievable on the later Homeric or Classical Greek forms of the Mycenaean word, the argument becomes contextual and syntactic in nature. Unfortunately, again, there is no conclusive evidence to exclude an imperative from consideration. With the opening statement simply locating the events at *me-ta-pa*, syntactically the document could accommodate both interpretations of timing and with no other temporal indicators, either from comparable uses of this verbal form or from an established Mycenaean calendar, this debate is likely to continue. Nevertheless, I suggest that *e-e-to* can be taken to be the future form, as Adrados (1957: 55) proposed, and that An 607 is indeed a prospective text, similar to Un 718, Jn 829 and potentially Tn 316. Within the oral arena that I have argued for, it is possible to view this document then as an affirmation of qualification and the reinforcement of social ties of patronage, obligation and duty for these women in the context of an announcement of an event that was soon to occur.

But what is this event and what is the reason for this gathering at *me-ta-pa*? The discussion so far clearly places 13 *do-qe-ja do-e-ra* at *me-ta-pa* alongside the *heq"etai* for the purposes of initiation or inauguration as the *ki-ri-te-wi-ja*, but as yet there is no obvious reason for administering this assembly at *me-ta-pa*, or of determining the context in which this event takes place. To understand this, I suggest we need to consider the term *ke-ri-mi-ja* in the heading of An 607. As with much of the vocabulary, no explanation or interpretation has yet been agreed upon. The only other occurrence of this word is to be found on Kn Lc 535 + 538, in connection with a *ta-ra-si-ja* allocation of cloth. Accordingly, Tritsch (1958: 412) maintained that *ke-ri-mi-ja* must either be an adjective applied to *ta-ra-si-ja* or a noun in apposition, but also noted that it could, on An 607, be an independent noun (*ibid*: 412, n.13). In the context of An 607, *ke-ri-mi-ja* has been viewed as pertaining to the women (*do-qe-ja do-e-ra*); a synonym of *do-e-ra*; or a designation of a class of women (Lindgren 1973b: 78). Given the likelihood that *ki-ri-te-wi-ja* collectively describes the *do-qe-ja do-e-ra*, the first and third suggestions would seem nonsensical. Similarly, the suggestion that this is a merely a synonym of *do-e-ra* would be superfluous to An 607 and illogical for Lc 535 + 538.

I suggest that *ke-ri-mi-ja* is indeed a noun that can be seen in apposition to *ta-ra-si-ja* and simultaneously gives meaning to the grouping of *do-qe-ja do-e-ra* at *me-ta-pa*. It seems most likely that *ke-ri-mi-ja* should be interpreted, somehow, as reflecting both the timing and context of this congregation. Contextually, this would be syntactically correct but the form that it takes is less certain. Two possibilities present themselves; that this is the name of a month, or of a known festival in the Mycenaean calendar. Given the absence of a compound form incorporating the term *me-no*, it seems less certain that we should consider this to be a month name, despite the fact that month names need not incorporate this (as with Tn 316). Nonetheless, this would not directly provide a specific context or reasoning for the ceremony and so I would suggest this is less likely to be the case. It is entirely possible though that this is the name of a festival, official event, or indeed the ceremony itself, in which the time element is implied rather than directly referred to. Given the connections already discussed between *ki-ri-te-wi-ja* and *ki-ri-ta* and similarly the association of *do-qe-ja* with barley, it is not inconceivable that *ke-ri-mi-ja* may be referring to a festival linked to the agricultural cycle, perhaps a 'spring festival' in which a ceremony of inauguration or confirmation of the *do-qe-ja do-e-ra* collectively as the *ki-ri-te-wi-ja* would itself be symbolically pertinent. Interpreted in this way, the heading line provides a succinct abstract of the document describing the location, timing and nature of an event in which the *do-qe-ja do-e-ra* are required to attend either as, or more likely to become, the *ki-ri-te-wi-ja*.

Exploring the identity of the *ki-ri-te-wi-ja*

What emerges from this analysis is the importance of the group of women known as the *ki-ri-te-wi-ja*, but what more can be said about them? Despite the relative paucity of references, I suggest that these few documents can reveal a very great deal. However, one of the first questions to be answered is whether the group in An 607 is unique to the Pylos region or was there more than one group of individuals bearing this title? It has generally been supposed that a single group existed based upon analyses of An 607, and the additional references in the Pylos tablets have largely been assumed, rather than proven, to refer to the same group. As I have suggested though, An 607 may be a prospective text referring to a forthcoming inauguration or initiation of women to this role and so it is a mistake to automatically equate this group with the *ki-ri-te-wi-ja* mentioned in the land-holding tablets. Furthermore, although the term *ki-ri-te-wi-ja* itself may be singular or plural (Aura Jorro 1985: 363), two clear examples of a plural form do exist in the corpus of Linear B documents: in Un 1426 it appears as the instrumental *ki-ri-te-wi-ja-pi*, whilst Kn E777 incorporates the dative *ki-ri-te-wi-ja-i*. These alone suggest that more than one group existed in each region and although Un 1426 is poorly preserved, Kn E777 is, by contrast, extremely informative in this respect.

This has traditionally been translated as detailing the distribution of wheat to women of Knossos, Amnisos and Phaistos for a period of one month; in the case of Knossos, the women being *ki-ri-te-wi-ja*. The *verso* appears to document a similar, single transaction of wheat to a group of 'seamstresses'. The figures for the wheat are clearly problematic being so large; approximating to 9600 litres of wheat in a single month, which, based upon the ration scales from Pylos, has been estimated as sufficient for 500 people at each of the named sites (Chadwick 1973: 214-15; Palmer 1963: 234; Tritsch 1958: 418, n.27).⁵³ I suggest however, that this reading can be modified in light of the evidence of An 607. The early reading of this document suggested that the *ki-ri-te-wi-ja* entry referred only to the women of Knossos. Yet, it appears in the dative plural form (*ki-ri-te-wi-ja-i*) and is noticeably squeezed into the first line, in substantially smaller script (Killen 2004: 166-67).⁵⁴ Both of these features strongly suggest that this should, instead, be understood to refer to multiple groups of *ki-ri-te-wi-ja* at each of the named sites, and so this should effectively be read as the *ki-ri-te-wi-ja* of Knossos, of Amnisos and of Phaistos each receiving 9600 litres of wheat (Uchitel 1984: 277).

This document clearly demonstrates that more than one group with this designation existed although we cannot be sure whether the three groups mentioned constitute the entirety of *ki-ri-te-wi-ja* in Crete. It is also clear that these groups are associated with major centres of political and social importance and that, additionally, the order in which the toponyms occur is highly suggestive for the situation in the Pylian case. Here, the primary centre, Knossos, is followed by what has been described as a 'local specialized area' in the case of Amnisos and a 'second-order centre' in the case of Phaistos (Bennet 1985: 249). Of particular interest is the secondary placing of Amnisos, a district that has cult associations, which has led to comparisons with *pa-ki-ja-ne* in terms of both status and function (*ibid.*: 242-43). From the Pylian tablets An 607 and Ep 704, a clear association between the *ki-ri-te-wi-ja* and *pa-ki-ja-ne* exists and, by association with the administering of the inauguration of individuals at *me-ta-pa*, the same is true for Pylos itself. It is reasonable therefore to ask whether such a pattern exists for the Pylian region and whether groups of *ki-ri-te-wi-ja* are associated with the equivalent major centres of the Pylian region, in this case Pylos, *pa-ki-ja-ne* and possibly Leuktron as the secondary capital (Bennet 1998-1999).

⁵³ As Killen (2004: 167) observed, the incomplete nature of the figures on this tablet would suggest that this should be regarded as a minimum estimate only.

⁵⁴ Furthermore, the textual layout and spatial restrictions are such that the late inclusion of *ki-ri-te-wi-ja* is only really possible on lines 1 and 3. If this was indeed a late amendment to the text, it is entirely plausible to suppose that the scribe would only attempt to include it on the first line.

The three feminine 'ethnic' adjectives on Kn E777, again, recall the gender issue previously discussed for An 607 where an apparent dichotomy exists between the membership of the *ki-ri-te-wi-ja* clearly comprising 13 women, but their qualification as individual *do-qe-ja do-e-ra* defined by parentage that includes *do-qe-ja do-e-ro*. This tablet appears to confirm that membership of this group was indeed confined to women of a particular status, with at least part of this qualification including the status of *do-qe-ja do-e-ra* as defined by An 607. Unfortunately we have insufficient texts to be sure that this was the only qualification or whether other similar religious personnel may have been eligible. Nevertheless, we can be certain that a social distinction existed between the individual *do-qe-ja do-e-ra* and the *ki-ri-te-wi-ja* and hence that the contention that this group is the lowest rank of religious personnel is untenable (Deger-Jalkotzy 1972: 140; Palmer 1992: 485). It would seem, however, equally implausible and unjustified to label them as 'upper class' (Billigmeier & Turner 1981: 8). They clearly possess a noteworthy status involving significant responsibilities but this does not necessarily imply that they are high status individuals or members of the elite.

However, this interpretation does not, in itself, explain the quantities of what are generally characterized as rations, nor does it affect the basic problem of the numbers that this could supply. To understand this, we have to dismiss the notion that this represents a ration allocation specifically for the *ki-ri-te-wi-ja*. It would seem improbable to claim that the Cretan *ki-ri-te-wi-ja* should consist of approaching 40 times the number of individuals than their Pylian counterparts.⁵⁵ The key to this conundrum undoubtedly lies in their role as what has been described as religious functionaries and the concomitant status that such a group would possess (Tritsch 1958: 418; Lindgren 1973b: 81). The most likely scenario, previously explored by Tritsch (1958: 418, n.28), is that these allocations represent a delivery to the *ki-ri-te-wi-ja* as responsible 'officers' for future distribution. Given their potential cultic association with an 'agricultural deity' and, therefore, with significant religious events such as the harvest, it is worth recalling the previously discussed scenario encapsulated by the 'sheep-flayers' of Un 718. The consignment of goods for special events seems to be made to responsible groups and it is this that I suggest is occurring with E 777. Wheat is being delivered to the *ki-ri-te-wi-ja* of three districts for use in some ceremonial festival or feasting event. The widespread and synchronous deployment to multiple groups further suggests a universally important event, possibly a harvest or spring festival. Such an interpretation would also explain the *verso* inscription in which an apparently menial group of *a-ze-ti-ri-ja* (seamstresses) are similarly accorded a disproportionately large quantity of wheat. The wider

⁵⁵ This is based upon Kn E777 and the estimate that the wheat allocation in each entry would be sufficient for at least 500 people. If my reading of this document is correct, and these were not acting as intermediaries for an event or other individuals, it would imply a combined *ki-ri-te-wi-ja* population of 1500 personnel known by the Knossian authorities.

significance of this entry and the group it concerns will be discussed later, for as we have seen in relation to the term *ke-ri-mi-ja*, cloth and cloth-working is intimately connected to the events that surround the *ki-ri-te-wi-ja*.⁵⁶

This form of deployment of commodities and the religious role of the *ki-ri-te-wi-ja* is further emphasized by Kn Fp 363, in which we see that a quantity of oil is due to the *ki-ri-te-wi-ja* alongside a similar delivery to the shrine of *da-*83-ja*. Conversely, the badly damaged Un 1426 shows multiple groups of the *ki-ri-te-wi-ja* contributing or delivering a substantial quantity of figs to the *wanax*, although there is no indication as to whether the groups are simply mediators in this transaction. Nevertheless, the text provides further confirmation of their notable position in society, and more significantly would appear to confirm the presence of multiple groups in the Pylian region.

Returning to Kn E 777, one further feature deserves consideration and that is the equality of allocations to the three groups of *ki-ri-te-wi-ja*. The lack of comparative data makes definitive conclusions virtually impossible but the identical figures suggest that either the places involved are deserving of identical allocations or that the constitution of the three groups is identical. The latter is perhaps more likely but for the Cretan *ki-ri-te-wi-ja* this cannot be proven. Yet this does provide a working hypothesis by which the *ki-ri-te-wi-ja* of the Pylian region may be further explored, particularly given the indirect evidence of multiple groups provided by the analysis of An 607 and the direct evidence from Un 1426. Having established that there were, in all likelihood, multiple groups of *ki-ri-te-wi-ja* in the Pylian region and that An 607 represents the initiation of one of these groups, it is worth considering the remaining tablets mentioning them. These references are contained in the landholding tablets Eb 321 and Ep 704 and the previously mentioned Un 1426. Of these, Ep 704 is without doubt the most informative.

Cited between two entries recording land holdings of the priestess *e-ri-ta*, Ep 704.4 records the holding of a substantial plot of land from the *damos* by the *ki-ri-te-wi-ja*. Significantly, this landholding is five times that of *e-ri-ta* again indicating that the *ki-ri-te-wi-ja* must comprise a group of individuals, but at first glance it is not clear how many individuals are involved. If we work on the basis that there is some significance to the number in An 607 and that the Kn E777 implies (albeit tenuously) that the different groups have inherently the same structure, then it is possible to tentatively examine the nature of the individual membership further. Here, we need to note the preceding entry for *u-wa-mi-ja*, a *te-o-jo do-*

⁵⁶ This cannot, in my opinion, therefore be regarded as one of 'what seem to be 'ordinary' ration records' (Bendall 2007: 38).

e-ra who has a land allocation equivalent to T 1 v 3 of seed wheat, but as a gift from the priestess *e-ri-ta*. If the individual *do-qe-ja do-e-ra* of An 607 were to have similar rights, and possibly status to *u-wa-mi-ja*, then we might expect a landholding for the *ki-ri-te-wi-ja* to be 13 times that of *u-wa-mi-ja*; a holding of 1 T 9 v 3.

In fact, Ep 704.4 records a figure of 1 T 9 for the *ki-ri-te-wi-ja*. A discrepancy of v 3 is, I suggest, too close to be coincidental and does suggest that this group of *ki-ri-te-wi-ja* are also 13 in number and that their individual land rights are equivalent to a *te-o-jo do-e-ra* which, incidentally, negates the problem of an apparent status dichotomy between this group and the priestess *e-ri-ta*. Of course this calculation can only be considered an approximation and, as I shall expand on below, the magnitude of the error can be accommodated if the individual members of the *ki-ri-te-wi-ja* are accorded slightly different rights dependent upon their position within the group. Similarly, the comparison with *u-wa-mi-ja* may be considered reasonably safe because, although (as with the members of the *ki-ri-te-wi-ja*) she is described as a *te-o-jo do-e-ra*, we also know from Ep 704 that their landholdings derive directly from the *damos*, whilst that of *u-wa-mi-ja* is a gift from the priestess *e-ri-ta* and no other landholdings are recorded for her. It is therefore unlikely that *u-wa-mi-ja* is a member of the *ki-ri-te-wi-ja* herself. It may though be possible to identify the members of the *ki-ri-te-wi-ja* within the landholding tablets, but any search for these individuals must take into account the following criteria:

- ❖ that they are described individually as *te-o-jo do-e-ra*;
- ❖ that they possess land *ke-ke-me-na ko-to-na pa-ro da-mo*;
- ❖ that the total individual land holdings amount to GRA 1 T 9.

The identification of individual members of the *ki-ri-te-wi-ja* as *te-o-jo do-e-ra* would seem reasonable given that the initiation revealed in An 607 sees the individuals as *do-e-ra* of the deity *do-qe-ja*, as well as the *ki-ri-te-wi-ja* being 'of *do-qe-ja*'. Since the *ki-ri-te-wi-ja* appear several times in the Pylos tablets without a corresponding deity name, it seems likely that this qualification is meaningful only with regard to the initiation and may, in any case, be different between the various groups. Individual identification, when necessary, would logically therefore be as *do-e-ra* to the generalized term *te-o-jo*. Also, based on point (ii), the search for these individuals must be restricted to the Eb/Ep series describing land held from the *damos*, but Ep 704.4 does not of itself exclude the possibility of the individual members holding additional land that is *ki-ti-me-na ko-to-na*. Table VI-1 shows the *ke-ke-me-na ko-to-na* landholdings for all of the *te-o-jo do-e-ra* listed in the Ep series.

Ep series tablet	Parallel Eb series text	<i>te-o-jo do-e-ra</i> name	Lease From	Grain	Other tablets
Ep 212.1	Eb 916.A	<i>re-ka</i>	<i>damos</i>	T 6	Eb 886.A
Ep 212.4	Eb 347.1?	<i>ko-ri-si-ja</i>	<i>damos</i>	1 T 0 / 1 T 8 ?	En 74.18 En 74.24 En 160.4 En 247.3
Ep 212.5	-	<i>e-pa-sa-na-ti</i>	<i>damos</i>	T 3	En 74.13 Eo 247.4
Ep 212.6	Eb 858.1	<i>mu-ti-ri</i>	<i>damos</i>	T 1	-
Ep 212.9	Eb 498.1	<i>i-do-me-ne-ja</i>	<i>damos</i>	T 1 v 3	-
Ep 539.1	-	<i>pi-ro-na</i>	<i>damos</i>	??	-
Ep 539.2	Eb 1440.A	<i>e-ri-qi-ja</i>	<i>damos</i>	??	-
Ep 539.4	-	<i>po-so-re-ja</i>	<i>damos</i>	??	En 609.17 Eo 224.7
Ep 539.5	-	<i>po-so-re-ja</i>	private?	??	En 609.17 Eo 224.7
Ep 539.6	-	<i>te-qa-ja</i>	<i>damos</i>	T 2	-
Ep 704.2	Eb 416.1	<i>u-wa-mi-ja</i>	<i>e-ri-ta</i>	T 1 v 3	-
Ep 705.1	Eb 866.B	<i>ma-ra₃-wa</i>	<i>damos</i>	T 2	-
Ep 705.6	-	<i>to-ro-ja</i>	<i>damos</i>	T 1	-
Ep 705.10	Eb 464.A	<i>ta-ra-mi-ka</i>	<i>damos</i>	T 1 v ?	-
Ep 613.12	Eb 173.2	<i>po-so-re-ja</i>	<i>pa-ra-ko</i>	T 1 v 3	En 609.17 Eo 224.7
Ep 613.15	-	<i>ko-pi-na</i>	<i>damos</i>	T 2	-
Ep 613.16	Eb 905.1	<i>mi-ra</i>	<i>damos</i>	T 1	En 74.4 Eo 276.3
Ep 613.17	Eb 900.A	<i>qe-ri-ta</i>	<i>damos</i>	T 2	-
Ep 613.18	??	??	<i>damos</i>	v 3	??

Table VI-1: The *te-o-jo do-e-ra* landholdings of the Ep series.

The variation in the scale of landholdings amongst the *te-o-jo do-e-ra* is substantial and as a result there are no obvious candidates for membership of the *ki-ri-te-wi-ja*. Certainly there are no consistent land allocations immediately identifying 13 members of this group, but this alone is not necessarily problematic. The unequal division of community land may simply be indicative of an internal hierarchy, perhaps based upon the relative ages of the members of the group. The evidence from An 607 that individual members were derived from family groups suggests that we should not expect the *ki-ri-te-wi-ja* to consist of a single age group. However, the table also reveals that five of these individuals possess landholdings that are privately leased (shaded cells). Despite several *lacunae* on these tablets, it is possible to tentatively suggest the makeup of the *ki-ri-te-wi-ja* at *pa-ki-ja-ne*.

In the first instance, we need to recognise that the figures involved follow a strict pattern, such that in the case of *ta-ra-mi-ka* it is entirely likely that the grain figure should be

reconstructed as T 1 v 3. By contrast there are three entries for *re-ka*, *ko-ri-si-ja* and *e-pa-sa-na-ti*, in which the landholdings are considerably greater than the majority, as well as being significantly greater than the average holding if 13 members of the *ki-ri-te-wi-ja* are assumed to hold 1 T 9 in total.⁵⁷ It would therefore be unlikely that these three can be considered as members of the *ki-ri-te-wi-ja*. Having provided these qualifications, it is possible to immediately identify 10 potential candidates with known measures of land (one whose name is unknown), where the cumulative holding equates to 1 T 4 v 3 (table VI-2). From the total given on Ep 704, there remains T 4 v 3 to be accounted for amongst three individuals. As previously alluded to, *u-wa-mi-ja* can be excluded because she does not appear to possess any land *ke-ke-me-na ko-to-na*. Two of the remaining candidates, *pi-ro-na* and *e-ri-qi-ja* only possess land that is *ke-ke-me-na ko-to-na*, but their landholding figures are, unfortunately, tablet *lacunae*. The final candidate is probably the most controversial, having several allocations of privately leased land; exceptionally though, two of these are recorded on the Ep series. Again, the figure for land that is *ke-ke-me-na ko-to-na* is not preserved. However, reconstructing the allocations for these last three individuals as the average of the outstanding T 4 v 3 produces quantities that are entirely consistent with the pattern of allocations in table VI-1. In the case of *po-so-re-ja*, such a reconstruction is also entirely consistent with the known values of privately leased land, which in the two cases (Eo 224.7 and Ep 613.12/Eb 173.2) are also both T 1 v 3. I suggest therefore, that it is possible to tentatively identify 13 members of the *ki-ri-te-wi-ja* at *pa-ki-ja-ne* whose total landholdings match the figure for the group's holdings in Ep 704 (table VI-2).

⁵⁷ The median figure across the 15 known landholdings is T 1 v 3; a uniform distribution across the median (encompassing the lowest value) would therefore be +/- T1. Values above T 2 v 3 could therefore be considered as exceptional. The mean value is not used here because of the biasing nature of the largest entries.

Name	Land holding	Possible reconstructions
<i>mu-ti-ri</i>	T 1	
<i>i-do-me-ne-ja</i>	T 1 V 3	
<i>te-qa-ja</i>	T 2	
<i>ma-ra₃-wa</i>	T 2	
<i>to-ro-ja</i>	T 1	
<i>ta-ra-mi-ka</i>	T 1 v ?	T 1 v 3
<i>ko-pi-na</i>	T 2	
<i>mi-ra</i>	T 1	
<i>qe-ri-ta</i>	T 2	
??	v 3	
<i>pi-ro-na</i>	??	T 1 v 3
<i>e-ri-qi-ja</i>	??	T 1 v 3
<i>po-so-re-ja</i>	??	T 1 v 3
Totals	1 T 4 v ?	1 T 9

Table VI-2: The potential members of the *ki-ri-te-wi-ja* at *pa-ki-ja-ne* and their landholdings.

It must be stressed that this can only be considered a tentative identification, but nevertheless it does appear to confirm that multiple groups of *ki-ri-te-wi-ja* existed in the Pylian region and that in all likelihood their numbers were the same across groups. In addition, the pattern of landholding values between the individuals is suggestive of a similar kinship pattern to that witnessed in An 607 and the frequency of the land size may indeed be age related. Unfortunately, without additional documents it is not possible to proceed further with the analysis of the multiple groups or to confirm or deny the existence of a third group of *ki-ri-te-wi-ja* at Leuktron.

An 607 at the interface between politics and religion

In attempting to resolve the numerous interpretive issues surrounding this document, scholars have generally agreed that two issues are central to any scheme of analysis. The first is that the document surrounds and encompasses slavery concerns through the repetitive use of the terms *dō-e-ra/o* and the second is that, of all the untranslated words, *do-qe-ja* must be the most important and its translation would go a long way to solving the problems. In the interpretation I have suggested, both of these suppositions have been shown to be misplaced. Whilst the relationship of individuals to *do-qe-ja* is certainly of consequence, a precise translation of this word is not the key to unlocking the controversy surrounding this document, for ultimately the subject focus are the 13 individual women and the collective

entity known as the *ki-ri-te-wi-ja*. Moreover, I suggest that despite the interpretive problems surrounding several contentious words, it is in fact the most obvious that create the greatest problems, namely those that have led to the assumption that the document deals explicitly with slaves and slavery, the terms *do-e-ra* and *do-e-ro*.

Without doubt, the issue of slavery causes immense problems, from defining the status of the listed women, justifying the apparent connections between free and servile classes (both between the parents and also between the women and the *heq"etai*), as well as simply understanding the purpose of the gathering and ultimately the rationale behind creating the document. This text, in reality, is a relatively simple statement concerning the prospective initiation or inauguration of 13 women into what could be described as a cultic household known as the *ki-ri-te-wi-ja* (Uchitel 2005). The individuals concerned are described as being under a bond of service (as distinct from a bond of servility) to the deity *do-qe-ja*; a relationship that I suggest is best understood, not in terms of slavery, but as the beginning of religious service analogous to acolytes or neophytes. The text clearly reveals that individually such a relationship already existed, but that through conditions of eligibility, perhaps not all recorded, the 13 women were to become collectively *do-qe-ja ki-ri-te-wi-ja*. A major component of individual eligibility was defined by an inherited status. Unlike the *heq"etai* though, the status of both parents is recorded here suggesting that in this particular instance both familial lines were considered important and both were expected to have some degree of status. Nevertheless, of paramount consideration in defining the conditions whereby these women could enter the collective service of the deity seems to have been that a parental connection with the deity was demonstrable.

Responsibility for the inauguration of the 13 women at *me-ta-pa* is placed upon a group of *heq"etai*, possibly acting as, or in place of, the *telestai* or simply performing the initiation or investiture (depending upon the exact interpretation of *te-re-te-we*). The organization implied within this text suggests that this was a major public event, an event I would argue is either named *ke-ri-mi-ja* or reflected in that term. It is perhaps significant then that one of the defining characteristics of the *ki-ri-te-wi-ja* in Ep 704 is their landholdings and that they are largely *ke-ke-me-na ko-to-na*. Whether or not we can infer therefore that there was a community responsibility in providing the means of existence for this group, it does perhaps provide some explanation for why such a public event was required by An 607. Even if this was not the case, the potential etymological links between the two terms *do-qe-ja* and *ki-ri-te-wi-ja* and agricultural references to barley, fertility, fecundity do suggest that this event would have been viewed as publicly significant such as a spring festival or celebration of divine intervention/protection in the agriculture of the region.

Underlying this, though, is a document very much concerned with identity; in the definition of both individual and corporate identities as well as identifying the relationships connecting these individuals and groups. Within this we can recognise several aspects of personal identity, including kinship, gender and, indirectly, age that are of importance in establishing and validating admission to, and membership of, particular corporate groups. An 607 clearly demonstrates that social identity was of paramount importance in relation to the religious sphere and entering into relationships with a divinity required clear evidence of social eligibility. In this respect, status is evidently of some importance, but this document alone is insufficient to determine how this was defined or the conditions by which such relationships originated. Contrary to the established scholarly view, these relationships concerned, and explicitly emphasized, free citizens. This is particularly evident when we consider the similarities between the *ki-ri-te-wi-ja* and the *heq"etai*; contextually, genealogical or ancestral histories become significant in the advancement of individuals into such groups. It is therefore justifiable to view this document as the preparation of a rite of passage from a low level of service to the deity to one that may be intermediate to the priestly classes. In this respect, Bennett's (1961: 12) suggestion that the document records a change in status of the 13 women, and potentially an initiation, was essentially correct. Significantly though, the document shows that the responsibility for this advancement came within the purview of the palatial authorities. Although this document is unique in expounding so completely the parentage of individuals it does not stand alone as the only document concerning important ceremonies and particularly those of initiation or inauguration. The most obvious parallel is another contentious and long-studied tablet, Tn 316. As I shall argue, not only does this concern important ceremonies, but this document too potentially reveals the initiation or induction of individuals into religious service.

'Cultic households', initiation and Tn 316

Whilst An 607 has long been regarded as having no direct parallel, Tn 316 appears to exhibit many of the same features and intentions behind its composition (Adrados 1957: 53). Unfortunately, the interpretation of this notorious document is no less contentious than that of An 607. Much of the controversy has been fuelled by the physical characteristics of the document as much as by the problems of interpreting the contents. Found in Room 8 of the AC, this large, page-shaped tablet was composed by Hand 44 in a manner and style which, if not unique, is certainly unusual. The text utilizes both sides of the tablet and close study revealed it to be a palimpsest, but the writing style and general aesthetics led to severe assessments of the quality of the scribe's work (e.g. Chadwick 1973: 256, 458-59; 1976: 90).

This, in turn, led to interpretations of haste and carelessness on the part of the scribe brought on by the imminent demise of the palace and that ultimately Tn 316 was written because of a desperate attempt on the part of the palatial authorities to avert an impending disaster (*ibid*; Wood 1985: 216-17).

Subsequently, detailed analysis of the document convincingly refuted the view that the composition was poorly executed, concluding instead that the layout and construction of the text betrayed a significant degree of considered experimentation by the scribe in formatting the entries in the most expedient manner (Palaima 1995a: 627-28; 1999). The use of both sides of the tablet was attributed to dissatisfaction with the construction of the initial paragraph on the *recto* but with a greater sense of scale and proportion obtained through writing this, the scribe was able to utilize the *verso* more efficiently and thus correct for inadequacies in the layout of the *recto* (Palaima 1999: 444-50). Whilst there is undoubtedly some truth in this, I suggest that as a complete explanation of the very precise sectioning of the text this is inadequate. A number of compositional clues point to a very deliberate and precise use of both sides and the four paragraphs for the text. Neither is it possible to entirely dismiss the notion of haste in composition. The organization and presentation of information is not dissimilar to many features of the Jn series that demonstrate this, and the notion of experimentation does not alter the fact that the execution of the ruling and writing on the *verso* is extremely irregular in comparison to the *recto*.

We need to recognize, firstly, that the lexical formulae used, rather than providing the most efficient use of space and simplicity in the composition, actually invite a degree of superfluous repetition (contra Palaima 1999: 447). If the strict separation of paragraphs was unnecessary, much of the information contained within paragraph four could have been subsumed in paragraph three. Indeed, it is perhaps possible to have incorporated both of these into paragraph two; the only potentially problematic distinction being the involvement of 'the town' (*wa-tu*) in the proceedings at the sanctuary of Poseidon. Otherwise, the three paragraphs indicate that rituals will take place at specific sanctuaries or shrines and that particular offerings will be made to named deities using the same formula *i-je-to-qe* *do-ra-qe pe-re po-re-ña-qe a-ke*. The scribe certainly found it possible in the third paragraph to incorporate several events in the introductory statement. This collates three ritual events at 'the sanctuary of *pe-re* *82 and at the sanctuary of *i-pe-me-de-ja* and at the sanctuary of *di-u-ja*' and then subsequently enumerates the specific offerings for each event. This clearly shows that the scribe was prepared to incorporate contemporary events within a single paragraph, if there was a valid reason to do so. Given the potential for collapsing the

specifics into at least two paragraphs, and potentially only one, there must have been a contextual reason for not doing so.

The second clue refers more specifically to the unique event recorded on the *recto*. Here there is a clear distinction between this and the events recorded on the *verso* in that it is to take place at *pa-ki-ja-ne* as distinct from particular shrines dedicated to named deities. That *pa-ki-ja-ne* is thought to be the major cult centre within the Pylian state may also serve to ensure that this paragraph remains distinct from the others. In addition, it is this entry that immediately follows the heading line specifying that the events will take place in the month of *po-ro-wi-to-jo*. Following the primary entry relating to *pa-ki-ja-ne*, the tablet was turned over for the recording of the subsequent entries. So, whilst I agree with the main argument presented by Palaima (1995a; 1999) that this text in no way represents a poorly executed composition or substandard scribal practice producing an incomplete document, the deliberate paragraph formatting cannot be explained away simply by scribal experimentation. This precise formatting to an extent mirrors the particular nature of the textual composition, in which we see:

- ❖ each paragraph beginning with the same majuscule inscription;
- ❖ a consistent syntactical construction of each paragraph, even to the extent that the heading line of the *recto* is kept separate from the primary paragraph to maintain this formula;
- ❖ the repetitive use of common phrases within and between paragraphs;
- ❖ the consistent ordering of the offerings.

I suggest therefore, that to accept the deliberate and careful composition of the text is also to accept the same considerations in the act of composing the text. Yet when we compare the nature of the ruling and writing on the two sides, clear differences exist that, like the Jn series, are suggestive of composition in a dynamic, oral arena. Despite this, the scribe maintained a remarkably ordered approach to the composition, but if this format is so specific and deliberate, what then does it represent? Much of the debate has centred upon relative status and hierarchies, particularly in relation to the deities and the relative quantities and types of offerings to them (Palaima 1999: 452-54). Yet, it seems that each paragraph is contextually specific and, although within each a relative hierarchy may be posited, this can only be taken to apply to that specific event. Likewise, the relative 'richness' of the objects concerned cannot necessarily be compared. To do so would be to assume that each ritual event was identical in nature and there is no evidence to support this.

Indeed, it is not simply the presence of so many recognisable deities that has set this text apart, but also the 13 references to gold vessels, many of which appear to be juxtaposed by offerings of men and women.⁵⁸ With so few references to gold in the Pylian tablets, it was perhaps inevitable that such attention should be levelled on this document. Aside from the oblique references to decorative gold on Ta 714 and 716, the goldsmiths of An 207 and the 'sacred gold' of Ae 303, the most notable parallels are to be found on Jo 438, enumerating some 5 kg of tributary gold and Tn 996 mentioning two gold vessels (Chadwick 1998-1999). Unfortunately, Tn 996 which might have proven to be the most relevant comparative source, listing as it does metal vessels similar to those appearing in Tn 316, appears simply to be an inventory and contextual information as to what this inventory was intended for is missing. Several interpretations as to the significance of the Tn 316 vessels have been proposed, but it remains questionable whether the attention that these objects have received is altogether justified. Given that these appear to be simply accoutrements to a series of ceremonies, albeit lavish in themselves, the various arguments as to their origin and eventual fate are somewhat beside the point (cf. Palaima 1999: 450-54).

More importantly, it is a mistake to assume that the types of vessels being offered can make any statement about the relative importance of the shrines and deities to which they are being presented. There is little or no contextual information as to the nature of the ceremonies or how these objects were used in them; nor are we justified in imposing a modern value judgement as to the relative merits of particular vessel types, such as viewing a bowl as somehow inferior to a goblet (*ibid*: 451-52). As has been observed in relation to the development of Minoan ceramic traditions, the juxtaposition of open to closed vessel forms may be as significant in terms of how the contents of the vessels may be viewed and whether they reflect individual or communal consumption (e.g. Day & Wilson 2002: 149-52). Consequently, it may simply have been appropriate, in these ceremonial contexts, that a goblet is gifted to Potnia, whilst a bowl is provided for Hera. We cannot assume, nor indeed is it likely, that each ceremony was conducted in exactly the same way or that they involved precisely the same actions by the participants. We may conjecture, for example, that the ceremony for Potnia involved the goblet, perhaps filled with wine, being used by only a single person; whilst a similar ceremony for Hera involved the bowl being passed between several participants. I see little merit, or justification, then in attempting to impose a ranked

⁵⁸ Whilst I would not make any serious connection between this figure and the previous analysis of An 607, it is nevertheless intriguing that this number recurs several times within religious or cult contexts, particularly when one considers in addition to these references, the 13 men listed in the 13 so-called *dosmos* tablets providing offerings to Poseidon. Without further investigation any suggestion as to the significance of this number, if indeed it has one, is undoubtedly speculative but given the contexts in which these numbers seem to occur, the most obvious question that arises is whether it relates to the pantheon of Mycenaean deities?

hierarchy upon the ceremonies listed in Tn 316 and, indeed, the format employed by the scribe appears to reflect the highly contextualized nature of these events.

It seems far more likely, therefore, that this careful structuring relates to a sequencing of events. In, and of itself, the formulaic nature of each paragraph appears unnecessary, but if used to emphasize a sequential chain of events within the month designated in the heading line then it does indeed have a definite purpose. I have deliberately avoided claiming this to be a chronology of ritual practices or, as was suggested, that this represents even part of a calendrical text (Palmer 1955: 12). As with the majority of documents, we have no temporal reference points by which to judge this other than the heading line and, unsurprisingly, attempts to locate the calendrical position of this month have had little success. The translation of the name as 'the month of sailing' and comparison with later, documented calendars provide no definite associations to locate it within the year (Chadwick 1976: 90; Palaima 1995a: 629-31; 1999: 443-44). Yet the initial idea that this was part of a ritual calendar was enthusiastically embraced and modified to suggest that the paragraph construction (including those ruled but left blank) represented consecutive days, five on each side, within that month. This idea was subsequently abandoned, perhaps unjustly, following confusion and debate concerning the order of composition of the two sides and the largely aesthetic judgements of carelessness in the composition of the text (Palmer 1955; Chadwick 1973: 286, 459). Detailed analysis has indeed shown that these judgements are largely misplaced and that although the original identification of which side was written first did ultimately need changing, the overall composition is continuous across both sides (Palaima 1995a; 1999). In this respect at least the dismissal of the chronological hypothesis is perhaps surprising and certainly worthy of re-evaluation. There is certainly no reason to exclude the possibility that this offers a basic timetable of events that were expected to occur in the stated month, for it cannot be a coincidence that the document header consisted of the month name alone with no further qualification or amendment, even when the tablet was turned over to complete the text. It is therefore entirely consistent to conclude that the text, at the very least, describes a series of events taking place sequentially and essentially contemporaneously.

The deliberate and distinctive separation of paragraphs employed indicates that these ceremonies were distinct and the syntactic construction serves only to emphasize this. By utilizing a repetitive phraseology, far from abbreviating the information to the most concise form, the scribe has created an extended text but one in which the distinctive nature of individual ceremonies is reinforced. Despite involving common groups of people and similar paraphernalia, it is made clear that each is separate and cannot therefore be happening

simultaneously. Moreover, the events themselves are presented in a clearly structured form; there is an inherent and meaningful order, whether the ordering of the shrines or deities is considered or whether seeking to explain the very specific separation of the events at *pa-ki-ja-ne* from the others. Thus we can conclude that if all of these ceremonies were to happen concurrently there would be no logical need for the scribe to impose such a careful and rigid structure upon the construction of the text. Ultimately, deriving a specific timeframe without further comparative texts would be largely speculative. It is, for example, tempting to equate the four paragraphs employed with events occurring in consecutive weeks of the month of *po-ro-wi-to-jo*, but beyond the division into four complete paragraphs, there is no additional evidence to substantiate this.⁵⁹ We cannot even be sure when these events were supposed to take place in relation to the writing of the tablet and whether this is one of the few prospective texts in the Pylos archives. Unlike Un 718 or Jn 829, there is no overt use of the future tense, but the overall syntactic construction does not exclude the possibility that this does refer to events yet to occur.

So far, I have skirted around the issue of what these events are. Superficially, the document records the dedication or presentation of gifts and individuals to named deities or shrines which, according to the heading of this text, all occur in the month of *po-ro-wi-to-jo*. As with An 607, such a generalized statement of interpretation is largely the point of departure for scholarly agreement. The precise nature and organization of the events being recorded; the identification of the people involved; the relationships of the offerings to the various shrines and deities, and even the timing of the events are all amongst the most controversial issues surrounding this text. Yet these represent only a fraction of the problems generated by Tn 316. Semantic and lexical problems have rendered a full interpretation exceedingly difficult with the result that numerous interpretations have been offered, some more controversial than others. Ultimately, these were based upon the translation of the much debated phrase *do-ra-qe pe-re po-re-na-ge a-ke* and how this relates to the ‘human offerings’ to the various shrines and deities. This has broadly been translated as meaning ‘and brings gifts and leads *po-re-na*’ (e.g. Buck 1989: 132; Palaima 2004b: 120-21), but whilst the gifts are usually taken to refer to the listed vessels, the identity of *po-re-na* is a divisive issue. Gulizio (2000: 107), for example, prefers to break the phrase down into opposing units where *do-ra-qe pe-re* refers to the vessels being presented and *po-re-na-ge a-ke* to the men and women, where *po-re-na* refers to these individuals being led to the shrines. However, is it correct to read *po-re-na* as the people enumerated in Tn 316 as being offered to the shrines or deities? Certainly this has been the most common position adopted, whether or not the men and women were

⁵⁹ This also requires us to make the unsubstantiated assumption that the Mycenaean calendar was based around four week months.

viewed as bearers of the vessels or sacrificial victims (e.g. Chadwick 1973: 284-87, 461-63; 1976: 91-92; Spyropoulos & Chadwick 1975: 94; Buck 1989). However, the dedication of people is not consistent between shrines and deities with, for example, *i-pe-me-de-ja* receiving a gold vessel but no individual. If we equate *po-re-na* with the 'bearers' of these vessels then in this particular instance, there would be no one to bear the vessel, and similar instances are found in r.05 and v.10 (Buck 1989: 133).

Using comparisons with Hittite texts, Uchitel (2005: 58-59) developed an alternative interpretation in which these personnel were viewed as members of a 'cultic household' and that this list records the 'donation' or offering of gifts and members of these cultic households to the gods or shrines. The interpretation of the individuals as cult personnel is significant and raises a number of interesting parallels with An 607. It recalls the suggestion that these are individuals being led to the sanctuaries to begin service to the divinities involved as priests or priestesses (Gulizio 2000: 108). It is possible then to view these events in a similar vein to An 607, in that individuals are being inaugurated into religious or cultic service, but I would argue, given that no names appear, that this represents a lower level of service than Gulizio suggests. If so, these ceremonies could be viewed in a similar way as a rite of passage but one which would effectively be a precursor to that outlined in An 607. Taking this one stage further, given that both men and women are being presented to the shrines and deities, could Tn 316 be representative of the establishment of the individual status or role of *te-o-jo do-e-ra/ro*?

This returns us to the question of identifying *po-re-na* and determining whether *po-re-na* is being led, or is leading. I suggest that this term does not correlate with the personnel being 'gifted' to the shrines, but refers to other members of the religious community that are leading the processions to the shrines. Again, relating this to the analysis of An 607, the question arises as to whether this word refers to an individual or a group and if the latter, whether *po-re-na* can be seen to have similar connotations to the *ki-ri-te-wi-ja* and in effect be a group designation or identity connected to religion and cult? I would argue that there is some reason to suppose that this is the case and that a similar meaning to *ki-ri-te-wi-ja* can be posited for *po-re-na*.

Unfortunately, this term is extremely problematic and essentially remains untranslated despite considerable attention having been paid both to the word itself and to the common phrase in which it occurs (cf. Buck 1989: 133-36; Nagy 1994-1995; Willi 1994-1995; Palaima 1996-1997 [1998]; 1999: 454-55). Attempts to derive a meaning have largely focussed upon later words relating to 'sacrifice' and the assumption that the object of the

word was the 'human offering', with the result that its meaning has been taken to approximate to 'sacrificial victim' (Chadwick 1973: 461; Spyropoulos & Chadwick 1975: 94; Hooker 1977: 176; Buck 1989; Aura Jorro 1993: 143). At the other extreme, Willi (1994-1995) proposed that *po-re-na* should be read as the infinitive *φορηναι*, but this is conditional upon excluding *po-re-si* or other compound forms as derivatives of the former. However, in reality, no definite linguistic associations can be made and the most common interpretation relies more upon earlier convictions that Tn 316 provides direct evidence of the imminent demise of the palace at Pylos. As Palaima (1999: 456) points out, '...evidence for a "state of emergency" can only be found if one assumes that it exists in the first place'. Much the same can be said for the treatment of the word *po-re-na* in that both arguments reflect an unsustainable teleology when further occurrences are taken into account. The most significant evidence, though, comes from the Theban tablet Of 26.3 in which the dative, plural form *po-re-si* appears, which almost certainly removes the possibility of a verbal translation (Uchitel 2005: 52).

The document lists allotments of a specific type of wool, *ku* LANA, to named individuals and groups; the first three entries all contain names receiving a fractional quantity (PA) and the subsequent two entries, again receiving this fractional quantity, are defined by the use of *do-de* as the households of named individuals. Some debate has surrounded the last of these as to whether *di-u-ja-wo* is a divine name (e.g. Palaima 1995a: 628, n. 22) or simply an individual bearing a theonym (e.g. Buck 1989: 134) or, given that the name recurs on Of 33, the priest of the divinity (Gulizio 2000: 113). The name recurs on Of 33 associated, on this occasion, with the toponym *qi-wo*, but again with no indication that this is anything other than a man's name. Contextually then, the latter interpretation of Of 26 would seem more likely within a group of names that do not have divine status and the use of *do-de* appears to confirm this as its use outside of the secular would seem to be at variance with the syntactic conventions describing people's relationships with the deities.

The clearest example of this is provided by Of 36 in which a full unit of this wool type is allocated 'to the shrine of Potnia', for the attention of a group of *a-ke-ti-ra*₂, following two dedications: one to an individual defined by a trade description and one to a group of 'royal' seamstresses. The fragmentary text Of 31, by contrast, lists two allocations to unknown households defined by the epithet *do-de*, and is followed by an entry specifying the name *e-ma-a*₂, but with no associated household epithet. These examples and the preceding discussion suggest that the characteristic formula of the Of series identified by Chadwick (Spyropoulos & Chadwick 1975: 87-88), which was based upon a name or occupation in the dative case and a location or address for that name, may be modified. Whilst there are

certainly exceptions to the pattern, it appears to be a more subtle formula based upon identities, broadly corresponding to individuals, households of individuals, followed by groups and finally religious groups, shrines and deities. The further example of Of 28 again reinforces this pattern: the deity *e-ra* appears in the final entry, preceded by two named individuals.

Returning to Of 26, the final entry defines the allocation of a full unit of the same wool type to the *po-re-si*, but without the use of the household epithet. In terms of translating *po-re-na* / *po-re-si*, in five of the six entries individual or indirect group epithets are being used in a particular order: named individuals followed by groups attached to a named individual. To translate the last entry as a group epithet, but one that is unattached to a named person, is therefore entirely consistent with this pattern. To consider that that epithet refers to ‘victims’, however, is entirely incongruous. As Palaima (1995a: 628 n. 22) has noted, ‘it would be more in keeping with the context of the tablet set for wool to be given out to religious functionaries called *po-re-na* ‘bearers’ than to assume that the wool in this single tablet entry alone is apportioned to victims’. The differential allocation of wool merely reinforces the notion that *po-re-si* here refers to more than one individual. Given these observations, it would seem entirely possible that the positioning of the entry for the *po-re-si* is indicative of a religious group or, to use Uchitel’s (2005) terminology, a cultic household.

However, as with the previous discussion of *ki-ri-te-wi-ja*, the question of whether *po-re-si* is the plural form of a group epithet or the plural of an individual epithet arises. In other words, this does not help in deciding whether *po-re-si* is referring to multiple groups of *po-re-na* or multiple individuals that bear the title *po-re-na*. If the former, is it possible that, as with the *ki-ri-te-wi-ja*, multiple groups existed? In this respect, Of 26 cannot help since it is clear that this enumerates allocations made not only on the basis of individual qualification, but also through membership of a group. The fractional allocations to households rather than individuals, in three cases, perhaps support the hypothesis that several groups are referred to, but without any indication of how allocations are derived or what they represent this evidence is inconclusive. Unfortunately the total number of documents referring to *po-re-na* provides insufficient information to determine whether it is an individual title or a group epithet in itself. Much of the interpretation of the meaning of this word falls back upon the interpretation of Tn 316, in effect creating a circular logic.

The only other references to *po-re-na* are contained in the Pylos tablets Un 443 and Ua 1413, but in both instances the word is one element of a compound, the stem of which is the variant *po-re-no*. In both cases the variant form has been interpreted as a compound incorporating

po-re-no and possibly a festival name, with *po-re-no* as the genitive singular or plural (Palaima 1999: 455). If this form does indeed incorporate a festival or ceremony name then it is perhaps significant that in both instances that festival involving *po-re-no* also involves items of cloth.⁶⁰ Ua 443.3 also places *po-re-no* in association with one of the same individuals who we see indirectly associated with the *ki-ri-te-wi-ja: ka-pa-ti-ja* the keybearer from Ep 704. There can be little doubt that *po-re-na* and the variant forms have strong cultic or religious connections and the combination of common elements with the *ki-ri-te-wi-ja*, their active engagement with religious ceremony, the close association with cloth and the contextual appearance of the same cult personnel lead me to suggest that these two groups are indeed very similar. Unfortunately though, it is I believe not possible to answer with any surety the question of whether *po-re-na* refers to an individual or to a group and thus whether *po-re-si* refers to multiple groups or to a collectivity of individuals. Tn 316 might suggest that it is indeed a group designation in its own right, since within each paragraph, multiple presentations are being made; but if this takes the form of a continuous procession from one place to the next, this need not be the case.

Nevertheless, Tn 316 does imply the existence of the cultic households suggested by Uchitel (2005), whilst the ceremonies of inauguration or investiture indicate a strong hierarchical structure within the organization of cult personnel. Both documents, I suggest, concern publicly visible ceremonies in which selected individuals gain admittance to the lower echelons of the religious organization. Whilst the interpretation of Tn 316 is likely to remain highly contentious, I suggest that it is, in reality, a relatively simple statement. Like An 607, it outlines the timing, ceremonies, personnel and, in this case, the paraphernalia involved in the initiation of new cult members, but in this instance the strict membership criteria appear not to be in place or were previously established. Combined with the lack of personal names this suggests that this could indeed represent the investiture of neophytes into various shrines as *te-o-jo do-e-ra*. Together, these documents demonstrate considerable palatial involvement and interest in the advancement of individuals into socially responsible positions. Although An 607 shows the relatively exacting criteria by which advancement in the religious sphere was achieved, it also reveals the level of political influence, and perhaps interference, being exacted in this sector and across society. As will become apparent in the final chapter, palatial concerns with the establishment and management of power and identity through the careful management of appointments to 'officialdom', and the garnering of support through

⁶⁰ Although perhaps coincidental, it is nevertheless interesting to note how this appears to mimic the situation observed in relation to the previously discussed *ke-ri-mi-ja* on Kn Lc 535+538. If, as I have suggested, this relates to a festival or other event, the provision of cloth by the palaces would appear to be a core element of such proceedings, further strengthening the parallels between An 607 and Tn 316.

mechanisms such as patronage, were to have significant consequences amongst the wider communities by the end of the LBA.

Chapter VII

From patronage to factionalism? Changing identities in the Post-Mycenaean era.

Many men of course became extremely rich, but this was perfectly natural and nothing to be ashamed of because no one was really poor – at least no one worth speaking of.

Douglas Adams, *The Hitch Hiker's Guide to the Galaxy*

And that is the crux of the matter. Who were the people recorded in the tablets? Was a remarkably broad spectrum of Mycenaean society documented, or was it in some way highly selective and biased towards those 'worth speaking of'? Previously, tablet analyses suggested that the palace maintained a curious mix of concern with the highest echelons of society to what may be considered the lowest classes including, strangely enough, slaves. My analyses suggest that this view is incorrect. I propose, instead, that whatever the precise nature of the titles borne by individuals or their overall numbers, the documents concern a select segment of society. It would be unjustifiable to suggest that we should consider the tablets as only documenting elite personnel. Within the context of the palace environment, the tablets reveal a natural hierarchy of rank such that there can be little doubt that this was an important facet of social identity. Yet the theory of patronage warns us against projecting this hierarchy beyond the particular environment in which it operates. The analyses I have presented demonstrate that alongside the political elites are those who, in one form or another, are patronized by that elite. Amongst these are many groups and individuals that beyond the palatial environment may be considered as of considerably lower status. Within this environment though, those individuals and groups are placed in positions elevated from their respective peers in the wider community.

This has not been a study concerned with the political philosophy of the Mycenaean ruling elite *per se* and there is little indication that political rule could be considered tyrannical or oppressive, but it could be characterized as self-serving or self-motivated. The selective privileging of a small proportion of the population strongly suggests that we should be more open to the possibility that by the end of the 13th century BC, Pylian society (if not Mycenaean society as a whole) was deeply divided.⁶¹ Palatial interest was evidently confined to selected individuals and groups with specific roles and positions within society. This selectiveness should not be confused with status or rank, but simply with palatial self-interest. Many of the individuals listed can be considered either as members of the elite or as elements of the political bureaucracy, as in the case of the *lāwāgetās*, *heq^uetai* and *telestai* for example.⁶² Others appear to be employed directly by the palace, appointed to specific posts, fulfilling their obligations to the palace, or receiving gifts in some way. The minutiae of local community issues are, however, entirely missing, subsumed, we can only surmise, in the responsibilities of local officials. Interests in these are seemingly confined to the adherence to community obligations by the representative officials, or the activities of specific individuals in respect of palatial needs. Furthermore, the tablets reveal a significant interference in, and manipulation of, social identities whether through direct appointments to official posts, the imposition of responsibilities upon local officials not necessarily a part of the elite circle, and the patronage distinctions applied to selected individuals. In this respect, we might infer that a high degree of political nepotism is revealed. The palace appears to be carefully selecting individuals, perhaps even protégés, to ensure effective governance and the maintenance or stability of power. It is this selectivity that I propose is fundamental to our understanding of LBA society, for it creates, or reflects, social division – a division that, I suggest, is reinforced by the archaeological record.

⁶¹ Palaima (2007: *passim*) places great emphasis on the figure of approximately 1000 named individuals in the Pylian tablets with respect to the apparent benefits that palatial rule brought. Yet this figure is entirely misleading. As Nakassis (2006: 154, n. 105) notes, around 25% of this figure (calculated as 936 rather than 1000) are names that recur more than once, many of which we can be certain from prosopographical studies refer to the same individual. Furthermore, a large proportion of these names refer specifically to members of the Pylian elite, to senior officials and to religious functionaries, all of whom must be considered as privileged members of society. This figure is therefore not representative of palatial concerns with the entirety of the population. Adjusting for this is problematic but, nevertheless, it seems unlikely that these names amount to little more than 1% of the population, based upon the eminently contestable and *conservative* estimate of a territorial population of 50,000 (cf. Whitelaw 2001: 64; Nakassis 2006: 154).

⁶² The recent analysis of the identity and role of the *lāwāgetās* suggesting this figure was the liaison between the palace and ‘outsiders’ is, in my opinion, highly unlikely (Nikoloudis 2006). One of the fundamental criteria by which this conclusion was reached was the appearance of *ra-wa-ke-ta* in association with ‘individuals and groups of moderate to low status’ (*ibid.*: 233). What, I suggest, we are witnessing here is direct evidence for patronage by the *lāwāgetās*, in a similar, but extended, form to that seen from the *wanax*. It seems to me inconceivable that the office of a person second only in rank to the *wanax* (as far as we can tell) should have been, to all intents and purposes, one of a palatial administrator, but also it would be highly unlikely that a single person could have acted as liaison across the entire Pylian territory.

The archaeology of fracture and separation

When examining the archaeological record for this potential fracturing of society, we need look no further than the palaces themselves. Across the Mycenaean world, palatial building programmes and development were largely turned inwards. From the purely functional constructions through to the more symbolic projections of power and elite identity, these eminently demonstrate the ability of the palaces to mobilize considerable resources, both human and material for their own ends. We should not forget though that identity creation is not an event but a process that cannot be studied in relation to a single generation. Identities are cumulative, drawing on all life experiences, traditions and heritage. At Mycenae, this process is made vividly manifest by the long-term concern shown in the creation of an elite identity and ancestral heritage through the use of funerary architecture, most famously through the incorporation, refurbishment and long-term remodelling of Grave Circle A within the heart of the citadel (Mylonas 1966: 94-96; Gates 1985: 272; Wright 1987: 141, n. 42; Button 2007: 86-89, 93-94, 96-97). Whatever the precise relationship of those interred in these graves to the *wanax* of Mycenae, enclosing this monument within the walls removed it from wider access, emphasizing the distance between the elite and the wider social landscape. Even within the citadel immediate access and view was restricted by the erection of a peribolos wall enclosing the grave circle and reinstated stelai. There could have been little defensive justification for this incorporation; the central authority was undoubtedly engaged in a process of the recreation or alteration of identity and place through the appropriation of earlier symbols of power and authority.

Externally, this process is reflected in the monumental tholoi distributed in the local landscape and in particular, the gradual restriction in their use to the palaces (Voutsaki 1995: 58-59, 62-63; 1999: 112-13). From the early palatial period onwards the mortuary arena became highly politicised. The lavish expenditure in the construction of the Atreus and Clytemnestra tombs, represent the culmination of a process of monumental building actively pursued in the creation of elite identity, the projection of political authority and the legitimation of rulership (cf. Wright 1987; Cavanagh & Mee 1999; Mason 2007). These were manifestly grandiose in design and investment in labour and materials, but close attention was also paid to integrating these structures in the physical landscape, not only for displaying the underlying wealth and power but also to create highly visible, symbolic statements of the authority of the *wanax* beyond the walls of the citadel (Mason 2007: 49-50). Furthermore, despite the strong evidence to show that the dromoi of these tombs would have been immediately backfilled following a burial, they incorporated elaborate facades which, it has been plausibly suggested, incorporate iconographic elements of the Lion Gate

marking these as belonging exclusively to the Mycenaean elite (Wright 1987: 182). So, although these were symbolically charged and dominating features outside of the walls these, like the shaft graves, were normally inaccessible and the iconography invisible. As monumental symbols of the power and heritage of the rulers they were tangible, but as foci of community unification and cohesion they were removed. What was created was a statement of power, wealth and ancestry that could be seen but not accessed, while in the case of the shaft graves not only was access controlled but visibility too.

Probably the most visible manifestation of this process, though, are the 'Cyclopean fortification walls' enclosing and separating many of the political centres from the wider landscape, most notably at Mycenae and Tiryns. While we might plausibly suppose that the original justification (although not necessarily the intent) was to provide a defence for the symbol of a regional identity, the traditional emphasis placed upon fortifications and defence is far from certain (cf. Hope Simpson & Hagel 2006: 23, 141-43). It is equally possible that emergent elites viewed these constructions in terms of aggrandisement – a conspicuous display of wealth, power and authority (e.g. Dickinson 1994: 160; Loader 1998: 41, 160-61). The practical benefit of greatly enhancing the defensible nature of these sites may simply have been an added bonus. At Mycenae, for example, where three phases of construction have been identified, their sheer scale militates against short-term defensive measures and it is questionable whether a defensive justification could have been maintained for later phases of construction. After all, how could extending the walls to incorporate Grave Circle A have enhanced the defences of the citadel?⁶³ Succeeding phases have, unfortunately, robbed us of an overall understanding of the nature and extent of the earliest constructions, but at Tiryns developments are clearer. Here, where an early Mycenaean palace has been identified, enclosing walls first appeared in LH IIIA1 enclosing only the 'royal residence'. Thereafter, in LH IIIB, two phases of building enclosed the acropolis, the last being probably toppled by earthquakes, followed by a final phase of repair and construction in LH IIIC (Kilian 1988a: 134-35, fig. 9; 1996: 63, fig. 3).

⁶³ Curiously, it has also been suggested that the construction of new 'Cyclopean' structures and the expansion of the walls of Tiryns and Mycenae in LH IIIB may actually reflect a defensive need (Dickinson 1994: 162-63; Loader 1998: 161-63). This, in my opinion, represents circular argumentation since the same issue of the time and resources required to build such massive structures used to argue for conspicuous display rather than defence in the early periods applies equally in later times. Indeed, the enormous scope of Cyclopean building and the scarcity of any substantive evidence for sustained warfare suggest that this is the culmination of processes of competitive display and emulation. It is certainly hard to envisage how the palace persuaded the working population to be continuously involved in projects whose only tangible benefit was for the defence of a select few inhabiting the citadels.

To deny the defensive character of these walls would be excessive but, nevertheless, this continual investment in over-engineering undoubtedly demonstrates a conscious investment in conspicuous displays of power and self-aggrandisement, and an overt display of what Kilian (1988b) described as the '*wanax* ideology'. Whatever the precise motivation behind these constructions, the fundamental consequence was the creation of an elite identity and separation of that identity from the wider social landscape. After all, the palace beyond its developing role as a political focus undoubtedly played a central part in defining a region, of encapsulating in a single construction the physical manifestation of a regional identity. Yet the imposition of these walls effectively brought about a change in the world. No longer was the symbol of identity accessible physically, or indeed visually. The effect was most pronounced at Mycenae and demonstrates this fracturing vividly. From its acropolis situation, the palace dominates the landscape and is a clearly visible monument to the elite of society. Yet in every conceivable way, the approach to the palace was restricted, with the circumference walls simply the most tangible barrier. The construction phase that led to the incorporation of the famous Lion Gate was cleverly implemented not simply to restrict physical access, but also visual access. It is not until a visitor has passed through this entrance, loaded with the symbolism of the elite, and made their way up the great ramp that the palace becomes visible. Even then, the succeeding millennia have robbed us of the true effect of the intervening buildings obscuring views of the palace itself.⁶⁴ This elaborately constructed gateway merely emphasised that the power to permit access resided with those within and with the incorporation of powerfully symbolic iconography that power gained a recognizable identity. However we choose to interpret the nature of, and the intent behind, this imagery it imposed an identity upon the citadel and its inhabitants that was entirely different to anywhere outside, where such iconography finds no replication or projection across the wider environment. Whether or not the individual elements of this were familiar to all, the Lion Gate was a vivid statement of a world belonging to the *wanax*. The exclusiveness of this identity was simply reinforced by the regular imposition of physical barriers, whether around the citadel, the grave circle or in the closing off of tombs.

Although the evidence at Pylos with regard to an enclosing wall in the final phase of occupation is, by contrast, ambiguous (Shelmerdine 1997b: 545-47; Zangger *et al.* 1997: 610-13), the building modifications to the palace are not. The latest phases of architectural modification were clearly designed to heighten restrictions to particular areas. Circulatory

⁶⁴ It is also worth considering the effects that the continuous investment in palatial works more generally would have had. The continuous focus and investment on building within the citadel would, at the very least, have demonstrated an introverted concern. We can only speculate as to how this was perceived more widely, but it is entirely plausible that this would have exacerbated any fracturing of society.

routes in and around the complex were dramatically altered which, along with the construction of outlying buildings, enabled the creation of distinct zones, each less accessible and less public than the previous (e.g. Wright 1984; Nelson 2001: 29-44, 207-16; Bendall 2004). Alongside these structural changes a phase of redecoration was also in operation, and noticeably from areas such as the Wine Magazine, only partially completed. There is also considerable evidence, despite McCallum's (1987: 63) assertion that the late structural changes were not accompanied by decorative changes, for the incomplete provision of wall-paintings. The substantial dump of painted plaster on the Northwest Slope has been tentatively dated (stylistically and through associated ceramics) to a period very close to the destruction, and consequently the result of the renovations being undertaken in the palace itself (Lang 1969: 5-6, 217-19, 221; Immerwahr 1990: 106, 111). Furthermore, a great many areas within the palace were found to have only a layer of 'mud-plaster' adhering to the walls, including areas considered to have a storage or industrial function (Lang 1969: 190-216).⁶⁵ This, I suggest, is direct evidence for an incomplete decorative renovation of the entire palace with these surfaces simply the prelude to decorative plaster and paint being applied.

These modifications clearly occurred over a considerable period of time, and in the case of Pylos were never completed. Their overall effect though was to further remove the palace environment, and therefore the political elite that inhabited these places, from outside communities. Much of this has been taken to be indicative of a time of unrest, of external dangers, but direct supporting evidence for this is extremely weak. The long debated threats that the Mycenaean world was supposedly aware of and preparing for are, at best, archaeologically and textually ephemeral. Even the controversial *o-ka* tablets provide no substantive evidence of extraordinary defensive measures. Neither can we realistically attach economic uncertainty to the documents. Arguments suggesting the rationing of limited resources, particularly surrounding acquisitions and transactions of copper/bronze, ignore one of the most fundamental characteristics of the documentary corpus – the timescale

⁶⁵ McCallum (1987: 64, my emphasis) refers to 'certain *formerly* decorated rooms were converted to storerooms (Rooms 31-33)', with the implication that these had been converted but deliberately left undecorated. Yet in all these cases backing plaster was found. Furthermore, layers of this type of backing plaster were also found in the Vestibule and Main Megaron (Rooms 5 and 6) alongside the well-known and extensive wall-paintings, where Lang (1969: 4, n. 1) further notes that none of this had been found to be painted. It would seem that in all these instances we are witnessing the preparatory foundation surface for the decorative plaster itself (Immerwahr 1990: 11-13). It is curious then, that interpretations of the overall state of preservation refer almost entirely to the destructive effects of the fire and to post-depositional processes rather than to the possibility that the absence of painted plaster in many areas may be indicative of an unfinished process of redecoration.

involved.⁶⁶ The snapshot they provide is markedly short, perhaps as little as a few months in a single year, yet there is little evidence at Pylos to relate the production of these tablets to any precursors. The assumption is made, largely on the basis of a bureaucratic model of bookkeeping, that this was an ongoing process month-by-month and year-on-year. The evidence I have presented suggests a situation very much to the contrary. Furthermore, the model of economic troubles is seemingly contradicted by the archaeological evidence from the palace itself. The extensive remodelling that was occurring can hardly be described as defensive in nature, nor can we attribute economic conservatism to it. Even without considering the workforce and man-hours involved, we cannot ignore the lavish expenditure on the decorative programme – a programme that itself argues against this scenario. Why invest time and resources in elaborate displays rather than on defensive measures? The impressive designs of the wall-paintings at Pylos, and indeed the incomplete decoration, appear to speak not of a time of troubles, but of socio-political consolidation and almost of celebration of the achievements of the palace and the state (Davis & Bennet: 1999: 115).

Similarly, the ceramic assemblages found in Rooms 18-22, and Room 60 in particular, whether procured in advance of a precise event, are extensive and do not suggest any foreboding of troubled times. On the contrary, these were seemingly obtained whilst the decorative work was ongoing. A recent study of the ceramic stocks of Rooms 18-22 suggested that they form a unified group, rapidly and somewhat carelessly produced by a single potter, almost certainly for a specific palatial requirement (Hruby 2006: 192-95, 202-03, 225-27). We cannot be certain when these were obtained relative to the destruction date, but it is not inconceivable that a very short interval separated the two. Rather than preparing for threats, either external or from neighbouring territories, the combined archaeological and documentary evidence suggests that it was engaging in considerable expenditure in preparation for an elaborate feast or other gathering. Indeed, it seems at least possible that the last act prior to the fires that consumed the palace was the provision of a large-scale feast, some of the remains of which were still awaiting attention in the AC. Overall, none of the artefacts recovered from the excavations suggest anything other than the interruption of everyday life in a well-stocked palace complex.

⁶⁶ The notion of economic conservatism in troubled times, particularly with respect to the rationing of materials, was forcefully expressed by Chadwick (1976: 141-45) in relation to the apparently small quantities of copper/bronze being distributed in the Jn series. As we have already seen (chapter V) this interpretation is undoubtedly incorrect, but is further refuted by the substantial quantities of bronze artefacts recovered during the excavations at Pylos that were evidently not returned for, or scavenged, following the destruction of the palace (Hofstra 2000: 111). Indeed, it now appears likely that for the period in question, the trade in copper/bronze was not reduced but underwent a transformation in which a previously monopolized elite system of exchange restricting the availability of copper/bronze was overtaken by a more entrepreneurial 'lower level of trade' that potentially increased the amounts circulating and hence its availability (Sherratt 2000: 83, 87, 89; also, Muhly 1992: 17-19).

Intended or not, modifications such as these refashioned the social and political landscape. The gradual and continuous withdrawal of the palatial elite behind the walls of their citadels fashioned a new identity; one in which differences with the external population were increasingly emphasized. At Pylos this process appears, superficially at least, to have been more subtle, with none of the dominating architectural elements visible at Mycenae and Tiryns. Nonetheless, this withdrawal is apparent in the restructuring of the palace complex itself, the decorative schemes employed and indeed the documentary evidence which all point towards a largely self-centred agenda and a deliberate process of elite identity creation (cf. Davis & Bennet 1999; Bennet 2007b; Peters in prep (a)). Such expressions were not restricted to the physical environment but were also manifest in the activities and practices of the palace. Most recently this has been highlighted with respect to the provisioning of feasts and the deliberate segregation of attending groups. Of particular note was Bendall's (2004) study strongly suggesting a threefold differential access to particular feasting venues within the complex according to rank, with each, apparently, served by particular pantries providing different quality vessels.⁶⁷ Within these contexts, it is likely that further differentiation occurred through the particular foodstuffs offered to particular groups (Halstead & Isaakidou 2004: 149; Hruby 2006: 229).

An unsustainable political philosophy?

Contrary to Palaima's (2007) rather utopian view of Mycenaean state and society, in my opinion, both the archaeological and documentary evidence reveal a calculated response to the maintenance of power and authority. It would certainly appear that palatial interest in the entirety of the population is marginal at best. This was not a political elite engaged in amenity provision for the general populace through the implementation of immense infrastructure projects. There is little or no indication that these were of any great benefit to the non-elite communities, whether or not they were justified and implemented as paternalistic acts. Roads, for example, would have provided little benefit beyond the elite.

⁶⁷ Some caution is required here. Bendall's suggestion that 'inferior wares' were being provided for use in the outermost courtyard from Room 60, while fineware vessels for use in the inner court 63 were stored in pantries 21-22, needs further consideration. Hruby's (2006) study of the ceramic assemblages from the latter pantries found an appreciable lack of quality in the finished products. The 'inferior wares' are, however, generally defined by a coarser fabric rather than necessarily the finished quality. Nevertheless, the distinction suggested by Bendall may not be as marked as was first thought. Intriguingly, this may point to an underlying perception of the palace towards those invited in. Clearly a perceptible differentiation of attendees was projected, reinforced by the attention paid to the types of ceramics provided for each, but one wonders from the lack of quality in the finished fineware products whether, from the perspective of the palace, there was little difference perceived in these external groups. In other words, is it possible that whilst superficially acknowledging the different status of groups attending, the palace did not themselves see a marked distinction? After all, although three venues are apparently in operation, two are external to the palace complex itself.

Their design and engineering, with soft surfaces, precluded a general use for heavy goods transportation beyond, perhaps, the occasional movement of building stone – a resource whose focus was on palatial activities. Their primary role undoubtedly lay in providing passage for chariots or other light-wheeled vehicles and hence of providing efficient and accelerated communication (Mylonas 1966: 86-87; Hope Simpson & Hagel 2006: 146-47, 170-73). Conversely, however, if Renfrew's (1972: 355) assertion is correct in that these roads were neither of any economic importance or a significant aid to communication, then their presence presents something of a paradox. It may be then, that we have to consider their role as processional ways (whether secular or religious), a possibility that at Mycenae may explain the apparently close relationship between the roads and cemeteries (French 2002: 120). Yet, this would only fuel the argument that their provisioning was largely aimed at elite society. Likewise, it is doubtful that the 'port of Pylos', if indeed it was a Mycenaean construction, would have significantly benefited more than a few. Even were we to suppose a largely trade orientated design philosophy it is likely, once again, that the chief beneficiary was the palace, particularly if the design was intended to accommodate vessels of the scale of the Ulu Burun wreck (Hope Simpson & Hagel 2006: 210-11). However communities were persuaded to participate in their construction, and labour organized, the primary beneficiaries of these monumental infrastructure projects were the palaces themselves.⁶⁸

Whether by accident or with some measure of design, the activities, policies and processes that the palaces engaged in reflected and contributed to a fractured society. In establishing their power and authority, and managing the internal political situation, a variety of devices were utilized that both drew upon these divisions and exacerbated the situation. We have already witnessed how the Linear B documents betray a socio-political philosophy in which patronage was a powerful mechanism employed by the Palace of Nestor, not only for the conduct of its own business, but also in maintaining its power and authority beyond. The effects of this though are much harder to ascertain. Anthropological and sociological studies have shown that it could have beneficial effects as well as negative consequences for clients, often dictated simply by the underlying motivation of both parties engaging in such

⁶⁸ Even in the case of the drainage of the Kopais, we cannot assume a noble, egalitarian intent to provide sufficient food for the population. Detailed investigations have revealed a long developmental history, with land reclamation beginning in the Middle Bronze Age (cf. Knauss 1991; Hope Simpson & Hagel 2006: 187-209). Yet the philosophy and intent behind the extensive maintenance of this project is largely assumed rather than known. Without a greater understanding of the local political situation or ideology, we cannot exclude the more cynical possibility that the palace of Orchomenos was involved in a long-term political manoeuvre to ensure internal stability and the support of the masses through the provision of work, the appearance of community interest and the promise of greater agricultural resources, of which it must not be forgotten that it too was a formidable consumer. Indeed, as has been noted, it is likely that the very prosperity of Orchomenos depended upon this project (Hope Simpson & Hagel 2006: 204, 207).

relationships. Whether or not the underlying intent is benign, clients are locked into a cycle of obligation and debt, a cycle that even in modern societies has proved difficult to break. Indeed, although it would appear inappropriate to challenge Palaima's (2007: 139) opinion that 'the 'exactions' of the palace are hardly oppressive', we should bear in mind the evidence of the Ma series in which it is clear that communities, for whatever reason, were recorded as still owing the palace from the previous year (Killen 1984a). From our perspective the quantities of material that appear to have been demanded seem manageable, but this series graphically demonstrates that the inability to comply with obligations was not an isolated phenomenon but commonplace. It is quite possible then that the presence of a significant client population in Mycenaean Greece would have had a long-term destabilizing effect on local communities. In a politically stable climate it is unlikely that this would have manifested itself beyond perhaps a degree of envy but nevertheless, those afforded the advantages or protection of elite/palatial interaction were socially distinct from those who were not.

This model does not imply a dominant political ethos of patronage; only that it was one of many powerful mechanisms exploited by the palaces to maintain the prosperity and socio-political stability that they undoubtedly craved. Alone, it does not explain the 'Mycenaean cultural package', or the curious mix of subjects addressed by the tablets. Yet, indirectly, the competition introduced by such practices may hold the potential for explaining much about the uniformity of material culture, particularly with regard to ceramics. Furthermore, the identity gulf that demonstrably existed may also help to explain the general absence of many materials and industries from mention. For although many absences are viewed as highly significant, it is questionable whether we should assume a palatial interest, let alone involvement, in materials or industries that did not form part of the definition of elite identity.⁶⁹

⁶⁹ To list all absences or under-representations of raw materials, foodstuffs, crafts, or the acquisitions of all the mundane objects required by the palace would be an immense task alone. Although there are many commodities, particularly in the Ma series, that are as yet unidentified but may fill some of these apparent gaps, the range of raw materials missing is extensive (cf. Bendall 2007: 274-84). Many of these, for example in relation to the pigments used in the wall-paintings, are exotic in origin and, as far as we can tell were restricted in their use to the palaces. Others, such as obsidian, although possibly more mundane in their application but more widespread in their distribution are equally absent. In the latter instance this absence is usually not regarded very highly and explained away either by reference to decentralized control of acquisition and production (e.g. Kardulias 1999), or to an attention focussed entirely upon the production of elite commodities (e.g. Parkinson 1999: 81). Yet it was still necessary for the palace to procure, in some way, all of these commodities and unless we assume that all such items were obtained as gifts, some form of exchange must have taken place. The absence of this from these 'administrative records' is, I suggest, significant and an under explored issue, but reinforces the basic hypothesis that the tablets should not be regarded solely connected to administering the economic interests of the palaces.

Palatial mechanisms of 'social management' effectively divided communities, with socio-political stability only maintained by the continued presence and power of the palace and the brokering activities and influence of local officials. However, our knowledge of how local communities were organized and operated on a daily basis is minimal. The tablets provide only a tantalizing glimpse of complex local hierarchies, the heads of which were accountable to the palace. Unfortunately, little can be discerned of the activities of such individuals beyond the narrow focus of these documents. Nonetheless, they do reveal how contact between the centre and outlying communities was essentially brokered by a lower tier of officials including, for example, the 'collectors', the '*heq"etai*' and the *telestai*. At the local community level, although the precise nature of many of the titles eludes us, we can see a distinct political structure involving, amongst others, the *da-mo-ko-ro*, *ko-re-te*, *po-ro-ko-re-te* and *qa-si-re-u* each with clear responsibilities to the palace and their communities. Whether in the form of managing 'taxation', tribute, the distribution of materials or compliance with work duties, these were essentially intermediaries between the central authority and the wider, dispersed communities of the polity. It was to these that responsibility fell to ensure the fulfilment of community obligations to the palace or to receive benefits as their representatives. For the majority of the population unable to gain such attention, contact with the palace did not occur and, seemingly, only through the mechanism of patronage would individuals be brought to the attention of the elite. Intentionally or not, between the political centre and peripheral communities clear divisions were operating that, I suggest, demonstrate that the seeds of social unrest and political troubles for the Mycenaean elite were sown long before the destruction of the palaces. It remains to be seen precisely what caused or triggered their eventual destruction but, to a large extent, this should be considered a separate debate; the destructions happened and the Mycenaean political system did collapse. Instead, it seems more pertinent to ask: why was neither the political system nor the palatial infrastructure reinstated and, who did the population turn to in the aftermath?

The *basileus* as a factional focus

One of the few terminological survivals from the Mycenaean political system that has generated considerable debate is the *basileus* (*qa-si-re-u*), particularly with regard to the obvious dichotomy between the position of the *basileus* in the tablets and the later, Homeric references to kings as both *basileus* and *wanax* (e.g. Lindgren 1973b: 126-30; Morpurgo Davies 1979: 95-99; Drews 1983; Carlier 1984: 108-16, 142-50; 1995; Aura Jorro 1993: 189-90; Palaima 1995b; 2006). Translating this term has proved problematic and defining their precise role more so. Although the Classical derivation suggests a translation of 'king'

or ‘chief’ this appears to bear little relation to the Mycenaean *wanax* either in status or authority. The tablets suggest instead that the *qa-si-re-u* were local officials having greater power within outlying districts and communities. Yet, even allowing for the Classical interpretation, it is not possible to suggest that the *qa-si-re-u* held the most prominent district position. Attempts to reconstruct the political structure and hierarchy suggest that they were subject to the authority of other officials such as the *ko-re-te*, but even this interpretation is not unequivocal. Models of the political hierarchy tend to assume a single, pyramidal structure headed by the *wanax* that descends through the echelons of rank within the central authority before spreading out below this to subordinate officials managing the affairs of various peripheral communities (cf. Kilian 1988b: 293, fig. 1; Nakassis 2006: 37, fig. 1). In many ways this represents the imposition of modern political ideologies upon the Mycenaean system, but there is every reason to suppose a far more complex situation at the local level.

Here, analyses have essentially focussed upon three key titles: the *da-mo-ko-ro*, *ko-re-te* and *po-ro-ko-re-te*. The first of these applies to two named individuals, *te-po-se-u* for the FP and *au-ke-wa* for the HP. With the latter, Ta 711.1 indicates that appointment to this office was made directly by the *wanax*, potentially providing another direct example of palatial patronage in action. These individuals would appear to effectively be ‘provincial governors’ responsible directly to the *wanax* and overseeing the *ko-re-te* and *po-ro-ko-re-te* (cf. Carlier 1984: 98-99; Aura Jorro 1985: 154-55; Nakassis 2006: 65-75). Below these were the *ko-re-te* representing the districts of each province and subordinate to them the *po-ro-ko-re-te*. Only three names for the *ko-re-te* are preserved making any analysis of the office problematic, but it would appear that at least one of these, *ku-ru-me-no*, also commands an *o-ka* on An 654. Of these titles, only the *ko-re-te* appears to survive into the later Homeric poems in the form of *κοίρανος*, importantly in the context of an army commander but, significantly, also in combination with a *βασιλεύς* (Lindgren 1973b: 84-86; Carlier 1984: 141 n. 14, 202; Shear 2004: 51). Neither the *da-mo-ko-ro* nor the *po-ro-ko-re-te* appears to survive in the later traditions and the latter although relatively numerous are unfortunately all anonymous.

Unfortunately the tablets provide no clear indication of the degree of autonomy afforded to these as individuals or, consequently, to their respective communities, the specific activities they engaged in or, indeed, to how they achieved their positions. Ta 711.1, for example, demonstrates the direct involvement of the *wanax* in the appointment of *au-ke-wa* as *da-mo-ko-ro*. If we understand this title as effectively a provincial governor then we can clearly observe a palatial interest in establishing some form of control upon the conduct of politics at a local level (Carlier 1984: 94-99; 1995: 356; Nakassis 2006: 71-73). However, direct

evidence for the succession of individuals to the lower tiers of officialdom is scarce, although it is possible that the *ko-re-te* and *po-ro-ko-re-te* are similarly palatial appointments. The possible correlation of the named *ko-re-te*, *te-po-se-u*, (Jo 438.21) with On 300.12 which seemingly describes *te-po-se-u* as *da-mo-ko-ro* indicates that although appointments were made by the *wanax*, individuals could be drawn from amongst the *ko-re-te* (Nakassis 2006: 72-73). However, it is equally possible that this was an exceptional occurrence or was a politically expedient move by the central authority to exert some form of control over otherwise locally influential figures. Without further evidence this situation is unlikely to be resolved. Nonetheless, whilst these titles conform to a straightforward hierarchy, the *qa-si-re-u*, by contrast, cannot be fitted neatly into this sequence (*ibid*: 85).

Indeed, when we consider the *qa-si-re-u* alongside the closely related group of the *ke-ro-si-ja*, an entirely different picture emerges, suggesting their roles fell outside, or paralleled, the palatial hierarchy. Again, how far this can be interpreted in terms of degrees of autonomy or independence from the palace is unclear. Both titles are relatively rare occurrences in the tablets and the latter particularly so. Nevertheless, there is sufficient evidence to suggest that the *ke-ro-si-ja* represent corporate groups, headed by a single individual, whose etymology suggests that a translation of ‘council of elders’ is not unreasonable (e.g. Carlier 1984: 112-13, n. 649; Deger-Jalkotzy 1998-1999: 75-77). Of the four groups mentioned in the tablets, three are headed by individuals identifiable in the Jn series as smiths. One also possesses the qualification of *qa-si-re-u* (*a-pi-go-ta* on Jn 431.6). The obvious connections between these groups, the *basileus* and craft positions have attracted considerable attention and prompted Palmer (1963: 228-29; see also Lindgren 1973b: 78-79; Killen 1987: 66) to reject the notion that *ke-ro-si-ja* referred to groups of ‘local chiefs’ but to suggest instead that it represented a craft establishment. The evidence for this is, however, slim and susceptible of multiple interpretations. Certainly it ignores the possibility that metalworking abilities alone may have been the purview of responsible individuals, or that this ability was considered a distinguishing social characteristic from a palatial perspective or, indeed, that it was potentially organized around kin groups (Deger-Jalkotzy 1998-1999: 76).

Superficially, therefore, the claims that the *qa-si-re-u* is an official concerned with craft production are justified but, as Killen (1987: 62) rightly points out, in relation to the connections established by the Jn series documents, ‘...it is not possible to be certain from them of the precise function of the /g^wasileus/ in connection with bronzeworkers’. Nor, as my analysis of the Jn series indicated, is there any substantive evidence to support the notion that they supervise the work of others. In this context I suggested, instead, that they are simply responsible representatives of the smiths, able to take delivery of copper allocations

on their behalf. There is no evidence to suggest that either the *basileis* or, consequently, the *ke-ro-si-ja* were solely involved in craft production (Palaima 1995b: 124; Deger-Jalkotzy 1998-1999: 75-76). Yet the relationship between the *qa-si-re-u* and the *ke-ro-si-ja* is potentially revealing in another respect, for there is evidence, from the mention on Jn 431.6 of the son of a *basileus* possibly working in the same capacity and that the title of *qa-si-re-u* was not bequeathed by the palace but was hereditary in nature (Carlier 1984: 110; 1995: 358; Deger-Jalkotzy 1998-1999: 76, n. 60; Nakassis 2006: 82, 85). As Deger-Jalkotzy (1998-1999: 75-77, 80) has compellingly argued, it is reasonable to suppose the *qa-si-re-u* represent the heads of kin-groups, with Jn 431.6 potentially direct evidence for patrilineage. As such, the *ke-ro-si-ja* conceivably represents the senior ‘elders’ of these groups – groups that may themselves represent an institution older than the palaces (Nakassis 2006: 84).

Despite the somewhat elusive nature of the roles of all of these officials there are, therefore, strong reasons for establishing the *basileus* as the most prominent of these from the perspective of palatial outsiders. The key, I suggest, is the hereditary nature of the title. Furthermore, although etymological studies of the respective titles *wanax* and *basileus* suggest that both represent imported terms, it is possibly significant that the figure of the *basileus* may be derived from a ‘Helladic mainland culture’ (Palaima 1995b: 128; 2006). And, although a hereditary element has been postulated for the *wanax*, the archaeological evidence suggests that the palaces were engaged in a process of ‘enforced’ or ‘constructed hereditariness’, with rulers apparently attempting to define their own identities in terms of an ancestry. From the incorporation of the shaft graves into the citadel at Mycenae, the design and construction of the Lion Gate, through to the imagery of the wall-paintings that appear to contain political or pseudo-historical legitimizing narratives defining Mycenaean identity (Davis & Bennet 1999; Peters in prep (a)). By contrast, the *basileus* (and consequently the *ke-ro-si-ja*) of all the officials possess titles that appear to embody values that were traditional and founded upon an ideology far removed from the palace. Heritage and tradition, and perhaps also military service, rather than the ‘royal seal of approval’ appears to have provided their mandate of acceptability. As such it is entirely conceivable that the *qa-si-re-u* and the *ke-ro-si-ja* represent the interests of local communities and are the interface with the palatial elite. In other words they represent a foil or buffer against the central authority that suggests further distance and disconnection. Indeed, the hereditary nature of the *qa-si-re-u* and *ke-ro-si-ja* appears to be in marked contrast to many of the elite positions, where palatial appointments demonstrate a formalized political structure designed to represent the interests of the palace. If so, individuals and groups such as these may have represented a potential political threat to the establishment – a threat that would have required careful management. It would seem, in one respect at least, that the political

influence of the Mycenaean palaces was limited. However powerful, in local political matters, the palace had to respect the power of tradition.

The social divisions that I have been arguing existed are, therefore, apparently supported by the political division observable in the documentary evidence. However, we must not forget that the Pylian territory also incorporated a natural geo-political divide. Even by the time of the destruction of the palace, the tablets demonstrate quite clearly that there was a provincial distinction – one that would have formed a significant element of individual and community identity. The background to this unification under the authority of the palace of Pylos unfortunately remains something of an enigma (cf. Bennet 1995; 1999; 2007b; Davis & Bennet 1999). Yet the study of the narrative scenes in Room 64 suggests that warfare played a central role in this process. Indeed, there is strong evidence to suggest that the sequence of wall-paintings across the palace portrays a wider narrative encapsulating all of the events surrounding this amalgamation. If this is correct, then it is possible to see in the imagery not only the rendering of distinct provincial identities prior to the unification, but also the creation of a new, combined identity brought about through the resolution of conflict (Davis & Bennet 1999; Bennet 2007b; Peters in prep (a)).⁷⁰ Yet we should not be misled. This was a *political* unification. How much these activities affected communities in both provinces and their sense of identity beyond the political centre is unknown.

There does, however, remain the problem of how precisely the *basileus* could become an influential individual in the post-palatial social environment. The previous discussion simply demonstrates the viability of the various models that place these figures at the head of emergent communities (e.g. Drews 1983: 112-14; Fagerström 1988: 144, 166; Whitley 1991; Shear 2004: 101; Mazarakis Ainian 2006). Alone, it does not explain how society was transformed under their guidance to produce the features that we see in the archaeological

⁷⁰ This issue represents only one element of a much larger problem yet to be addressed in any detail. Our knowledge of the respective social and political characters of the provinces prior to amalgamation, as well as the nature of the relationship between them, is extremely limited. This is especially true of the FP where the majority of information derives from the excavations at Nichoria – essentially an unknown quantity in the political structure of the territory. Indeed, as has been suggested, the supremacy of the FP capital of Leuktron may have been relatively late in coming and at the expense of other centres such as Nichoria (Bennet 1995: 598-99; 1998-1999: 29-30; 1999; 2007b: 12; Davis & Bennet 1999: 105-07). If so, numerous questions remain as to how this province was governed prior to the ascendancy of Pylos. Where was the original focus of political power in this region? Was it indeed at Leuktron, or was this too ‘a new capital for a newly expanded kingdom’ (Davis & Bennet 1999: 106)? Are the two modes of identifying Leuktron in the tablets representative of a newly established FP capital? Was there a *wanax* of the FP with an associated retinue? And, if there was, what happened to these former political leaders upon integration? In my opinion, research into the FP should be considered an urgent priority, not only to fully appreciate the historical development of the Pylian polity socially, politically and economically, but also to provide an accurate political framework in which to situate analyses of the Linear B tablets.

record. We must first recognize that the dominant feature of the post-palatial period and EIA is variability. Conventional treatments of this period tend to emphasize general trends, whether this relates to changes in material typologies, settlement forms or burial practices. With the latter, for example, discussions tend to emphasize a trend away from the collective burial of the palatial period towards individual interments, from the use of chamber tombs to cist graves, or the gradual shift from inhumation to cremation with sites such as Perati, Lefkandi and the Kerameikos being central to these. Yet in reducing the immense variability of these practices to a 'trend' or 'a shift towards', there is a distinct danger of falsely homogenizing the archaeological record, of suppressing concepts of regionalism or even individuality. Instead we need to embrace this variability as a function of social choice, community re-organization and political change, which is ultimately empirically meaningful. Rather than attempting to establish trends then (of which the only one worth mentioning is the general shift from uniformity to variability), analyses should acknowledge and respect that diversity is the hallmark of this period and proceed from this perspective. Indeed, it is this diversity that suggests that a central feature of the post-palatial period was the establishment of new identities through the mechanism of factionalism.

Very much a related concept to patronage, factionalism has recently become of interest to archaeologists generally (e.g. Brumfiel 1989; 1994; Neupert 2000; Díaz-del-Río 2004), and Aegean prehistorians specifically, in assessing the apparently unrelated themes of social fragmentation, the emergence of state societies and power relations (Hamilakis 2002; Adams 2004; Wright 2004). Defined most usefully by Brumfiel (1989: 127): '...factions within a society are structurally and functionally identical groups which, by virtue of their similarity, compete for resources or positions of power or prestige, or both', later modified to 'structurally and functionally *similar* groups' (Brumfiel 1994: 4, my emphasis).⁷¹ It is a profound irony then that factions and factionalism lead to the fragmentation and segmentation of communities and societies but at the same time their creation contains the seeds of community coalescence and the future unification of communities into formative state structures. However, factionalism is a resultant phenomenon, not a socio-political

⁷¹ This is an important revision and one that has been under-emphasized. Hamilakis (2002: 186) in a self-contradictory overview describes factions, in the first instance, as having 'similar cosmological and ideological principles' and subsequently that within a wider social group, factions are 'united by cosmological and ideological principles'. Since, by definition, factions are competing socio-political units, the notion of ideological unity negates the formation of factions. If groups were fundamentally united in their ideological outlook, competition and the need to distinguish oneself would not arise. In the case of post-palatial society, it is precisely the ideological differences that, I suggest, are the means by which factions could, and did, emerge. Furthermore, it is these differences and the need to formulate identities encapsulating these ideologies that were fundamental to the creation of an archaeological record that appears to demonstrate 'continuity' with the former system, as well as 'discontinuity'.

mechanism in and of itself, as many studies seem to imply. For factionalism to emerge, a socio-political climate is required in which degrees of competition along with social and political disengagement have occurred. Without social cohesion, fragmentation will inevitably create a climate in which socio-political divisions are naturally manifested and differential interests able to develop. Wolf (1966: 17) suggested that the seeds of factionalism could be found in patron-client relations where clients as part of the ties of obligation become embroiled in overt political support for their patrons. In doing so, clients become members of factions focussed upon particular patrons and as a result are active figures in the competition between opposing factions.

Continuities, discontinuities or confusion in the Early Iron Age?

This relationship, between patronage and factionalism, presents the possibility of understanding the dynamics of social change between the LBA palatial societies and the considerably smaller, and seemingly more isolated and individualistic communities of the EIA. In particular, it provides a mechanism by which the 'continuity' and 'discontinuity' evidence may be reconciled. When we look to the archaeological record of these periods, we apparently have conflicting patterns in the data in which some continuity with the palaces exists, whilst much else seems to be in complete variance. Yet if we suppose that this is the result of competing factions attempting to define their own identities, then it is entirely reasonable to expect a wide diversity of material expression. Indeed, it is this characteristic of diversity that represents one of the fundamental criticisms of recent applications of this concept to the emergence of the Mycenaean state ideology. As was rightly pointed out, whilst factions develop from a common basis of competition, the expression of competition should not 'lead to the same end result' (Day & Relaki 2002: 225). As such, we might expect the degree of adherence to the preceding palatial system to be exceedingly varied amongst emerging factions and for this to be a significant factor in the 'recruitment' of followers.

This model allows us to move beyond the simplistic search for continuity or discontinuity that has been a consistent concern for this transitional period. The selective highlighting of aspects of the archaeological record suggesting some form of ideological inheritance or, conversely, a break with the Mycenaean way of life, continues an unsustainable polarized debate. Indeed, the situation at Tiryns warns us against taking a simplistic view of the continuity/discontinuity argument. For although becoming the focus for a substantial settlement in the post-palatial period and where rebuilding of the 'Cyclopean' walls occurred, neither constitute direct continuity. Even the enigmatic Building T, built upon the

remains of the Mycenaean megaron, does not replicate the form of the preceding structure.⁷² There is no recreation or reinstatement of Mycenaean ideology, but an attempt to utilize a tradition and memories of power in the formulation and legitimization of a new identity (Maran 2001: 119-21; 2006). Nonetheless, there is clearly a degree of adherence to, and respect for, the Mycenaean world, unlike the virtual desertion of Pylos. Unfortunately, attempts to understand and interpret the historical record tend to assume a single continuum of progression. The Mycenaean palatial age evolves out of the Shaft Grave period, in which the formative stages of Mycenaean social and political culture can be recognized, which in turn emerges out of a, thus far, poorly attended Middle Bronze Age society. Following the palatial era, the historical process has been viewed as declining into the EIA before gradually recovering in the 9th - 8th centuries BC.

Although exceedingly simplified, this sequence does emphasize the basic problem that archaeological research has developed a 'roller coaster' ride of emergence and decline of cultures. From the perspective of investigating the EIA, it can only ever be viewed as a low point in Greek history; culture has simply devolved from a preceding high point. Presented, therefore, with the hypothesis that the character of EIA communities was analogous to that of earlier, Middle Helladic communities and that there was 'the fundamental continuity of Greek life from the Middle Bronze Age to the Iron Age' (Snodgrass 1971: 385), few scholars have responded (Snodgrass 2000: xxvi). However, Dickinson (2006b: 119; also, 2006a: 183, 244) was adamant that such a continuity was not demonstrable since, 'a picture of instability and change in patterns of behaviour must militate against Snodgrass's idea...'. Yet the very concept of discontinuity in the historical process is anachronistic. Without the complete replacement of population and the removal of all traces of previous social systems, discontinuity cannot occur. Even if it is retained simply as memory, succeeding generations are motivated, at least in part, by that which has gone before. The most vivid manifestation of this lies in the debate concerning the extent to which Homeric poetry reflects one world or another. Was Homer a poet of the Bronze Age or the Iron Age (cf. Snodgrass 1974; Finley 1983; Dickinson 1986; Morris 1986; Sherratt 1990; Bennet 2004)? I would argue that the answer is neither and both. Or, more accurately Homer was a poet of many ages. There can be little doubt that the *Iliad* contains elements that are wholly Mycenaean and that the situational context reflects the EIA, but there is considerable evidence that elements are drawn from pre-Mycenaean remembrances (Sherratt 1990: 817-18, fig. 4).

⁷² A parallel for this changed architectural form may be found in the LH IIIC reconstruction of the megaron at Midea (Maran 2001: 117; Walberg 2007: 198).

However, as Bennet (2004: 101) rightly points out, historical contingency must be borne in mind when considering such issues and although 'orality and performance' may provide the *medium* for the 'remembrance of times past', it does not define the multiplicity of contexts in which it is situated and can operate. Even allowing for an oral tradition extending back to the Mycenaean palaces and perhaps earlier, the mechanism of remembrance is still poorly understood. Orality, performance, memory and tradition were not solely confined to the palaces, and were arguably of greater importance amongst communities disassociated or disenfranchised from the political heart (and hence its ideology). The transmission of values and tradition, whether founded upon community or kinship, would occur alongside the prevailing ideology of a distant political authority and would potentially achieve greater permanence within these groups. Indeed, within the Pylian territory, it seems unlikely that traditions established in the respective provincial communities were fully suppressed or integrated by political unification. With the removal of the unifying authority it is possible to envisage how these different traditions and interests would be projected to the fore. In other words, it is entirely plausible that these would have provided sufficient differences for the creation of factions around authority figures whose power and acceptability lay in re-establishing subsumed identities, particularly when we consider that hereditary figures such as the *basileis* were not confined to a single province.⁷³ In this respect, we should perhaps dispense with the notions of continuity, discontinuity and a single historical trajectory completely, and acknowledge the overlapping of multiple possibilities even within the period of palatial domination. The transmission of cultural heritage from much earlier times is eminently plausible, as Homer demonstrates, even without necessarily acquiring material expression in the archaeological record. Indeed, the structure of local community authority, as far as can be determined, suggests an embedded ideological divide with the palaces; one that suggests the seeds of social change are to be found in the Mycenaean period, not as a result of the destruction of the palaces.

There is, as I have argued throughout, considerable evidence that patronage relationships were actively exploited by the palatial elite in a developing system of debt and obligation binding individuals and communities to their authority. It would be excessive to imply that this was either endemic or an established system of government, but even the selective

⁷³ Indeed, the significance of this may be largely underestimated. As yet, this territorial division remains unique within Mycenaean Greece and it may, therefore, be no coincidence that Messenia as a whole is seemingly afflicted by severe depopulation in the post-palatial period. If the creation of the Pylian polity was ultimately the result of warfare and conflict, the removal of the unifying influence of the palace at Pylos may have resulted in the resurgence or renewal of this conflict. Indeed, we might speculate that the effective 'decapitation' of the controlling power may have prompted an attempt to reassert a discarded identity, with the resultant upheavals driving many away from the region. It may be then, that compared to other regions Messenia was afflicted by a unique historical problem.

employment of patronage for the purpose of manipulating the relationship of individuals and groups with the palace holds the seeds of social fragmentation. And indeed, both the documentary and archaeological evidence for this late phase of the palatial Bronze Age suggests that a fracturing between the palatial elite and the wider communities was occurring. The resultant conflict of identity and the division of society, I suggest, laid the foundations for the emergence of factional competition following the collapse of the palaces. Indeed, it is worth considering one of the conclusions of a study of the emergence of factionalism in the Indian state of Haryana: 'Factionalism has been stimulated by rapid social and cultural change. The role or status ambiguity in potentially competitive situations allows competition to become so intense that conflict results. The failure, for one reason or another, of institutionalized mechanisms to resolve the conflict (or the absence of such a mechanism) allows the growth of factionalism' (Yadava 1968: 909). Here we can see many of the elements that have emerged from this analysis – a situation that also recalls the proposed power relationship and differential between group identities holding the potential for stability or change (Doosje *et al.* 2002: 60).⁷⁴

The effective decapitation of the Mycenaean states, providentially or deliberately, provided an opportunity for such rapid socio-political change by altering the socio-political equilibrium. The cohesive and stabilizing influence of the political centre was removed allowing competition, rivalry, enmity and conflict to emerge and flourish. Factions, though, require individuals around which groups may coalesce. Under normal circumstances these would be hard to identify within the archaeological record. However, for the LBA we are privileged to possess a considerable body of direct information as to the identities of the Mycenaean elite, to the extent that plausible attempts can be made to assign a name to the *wanax* at Pylos (cf. Palaima 1995b: 129-30, 134-35; 1998-1999; 2006: 62-63; Nakassis 2006: 98-124). With the disappearance of the palaces any direct, contemporary evidence is lost, resulting in reconstructions that rely almost exclusively upon applying the structure and titles suggested by later poetic tradition. It is largely for this reason that the *basileus* has played such a prominent role in debates concerning EIA society. Yet we also know that whatever connotations applied in later times, the Mycenaean *basileus* was a relatively minor official. Nonetheless, when scholars have attempted to define the leadership or the emergent elites, the *basileus* has inevitably become the focus of attention and however critically we view poetic traditions as a source of evidence, the above discussion indicates that this is entirely reasonable. What is questionable though is the privileging of the *basileus* above all others. Mycenaean communities were evidently subject to a complex political hierarchy

⁷⁴ See chapter I.

extending from the palaces, one that was not straightforwardly linear in nature. Furthermore, much of the Linear B vocabulary concerning individual and group titles remains at best contentious and at worst simply unknown. Our knowledge of local politics and the organization of power at this level therefore remain vague, and it is entirely feasible that other influential figures existed, otherwise unrecognized, around which individuals, groups or communities may have rallied.

Indeed, there has been a strong tendency to argue for the *basileus* as the post-Mycenaean chief - the 'big man' of Whitley's (1991) model, but it is just as likely that others that bridged the palatial/extra-palatial divide were ideally positioned to assume leadership roles or become the focus of factional attention. Not all officials linked to extra-palatial society should necessarily be considered in this vein. The obvious proximity of the *da-mo-ko-ro* to the ruling elite would seemingly preclude them from consideration. Whilst there are no absolute grounds for excluding individuals such as these from becoming factional foci, with no evidence for the reinstatement of the former institutions, or of later traditions recalling these titles, it seems illogical to suppose that post-palatial society looked to the previous elite or those appointed by them. But other potential candidates may plausibly include the *ko-re-te* and *po-ro-ko-re-te*. It is possible that the somewhat ambiguous nature of their position within the palatial elite lies at the heart of the survival of a title such as *κοίρανος* (e.g. Nakassis 2006: 74). Nonetheless, when we look to the period of the dissolution of the palaces we cannot deny the position that the *basileus* occupied, and it would seem entirely natural that it would have been individuals such as these that would become the focus of authority. The *basileis* appear to represent one of the few direct links between local communities and the centralized authority at Pylos that retained some independence from the central political institution. They clearly held a significant rank within society and with direct access to the palatial authority their role provided a bridge between the dispersed communities and the centre. From the perspective of the palace, these individuals provided the mediation necessary to implement and maintain policy. By the same token they could mediate between a local community and that authority. In effect the *basileis* were brokers between the political institution and the wider population. For communities perhaps disenchanted, disillusioned or simply disaffected by an authority far removed from the concerns of peripheral communities, the *basileis*, whose hereditary title indicates that tradition rather than palatial patronage defined their social position, would represent acceptable figures. As such they were uniquely positioned to establish leadership roles should the political system fail and, indeed, it was officials such as these that, I suggest, were the ultimate beneficiaries of the collapse of the palatial system.

It is my contention then that conflicting concepts of identity, in part, laid the foundations for the emergence of factions within the region. As these factions emerged, coalescing around figures such as the *basileis*, the archaeological correlates of social expression greatly diversified. Whether we choose to view this as diversity or ‘instability’, adherence to a faction constitutes the observance, or adoption, of a particular social identity. It is entirely feasible that some would have demonstrated closer ties to the previous authority than others who, perhaps renewing kinship or community traditions, would conceivably manifest themselves in markedly different ways. Snodgrass’s suggestion of some form of a renewal of earlier practices, therefore, merits far greater consideration. There is nothing inherently implausible in earlier traditions and identities being maintained amongst distant communities whose adherence to these was only suppressed by the power and political authority of the palaces.

Some confirmation for this scenario may be provided by the apparently increasing prominence of Nichoria in the EIA, contrasting the proposed demotion of status and significance in the palatial period (Bennet 1995: 598-99). A desperate lack of excavated sites in this region allows for little more than speculation as to its pre-eminence at this time, but successive phases of occupation during the Dark Age are highly suggestive of a factional basis for the community. Unit IV-1 and its successor Unit IV-5, have plausibly been interpreted as representing the residences of chiefs, even of *basileis* (e.g. McDonald *et al.* 1983: 40; Dickinson 2006a: 110-11; Mazarakis Ainan 2006: 185-88) and, although the evidence for the earliest phase is scanty, growth in activity during the first two phases of occupation is clear. Yet by the final phase it appears that the community was appreciably smaller and more isolated (McDonald *et al.* 1983: 326). Here, the excavators observed some adherence to ‘Mycenaean conventions’ such as in the continuing use of the Mycenaean cemetery, but in many other ways community life was considerably changed. It may well be that at Nichoria we are witnessing the dynamics of an emerging faction retaining vestiges of Mycenaean society but in large part reviving older traditions. Whilst in its later demise, we see the results of factional competition eroding the community.⁷⁵ The changes in fortune at Nichoria, rather than suggesting some form of continuity with the palatial system (e.g. Eder 2006), may reflect the establishment of a new community identity and, in large part, a return to an older tradition, the origins of which predate the dominance of Pylos.

⁷⁵ It is worth noting here the contradiction presented by interpretive categories in Aegean archaeology. Confronted with similar situations in mortuary practice – the imposition of a new monument or rite over an earlier site prompts the suggestion of the supplanting of earlier ideologies or the creation of a new identity (e.g. Georganas 2002). Faced with this situation in a settlement context such notions are ignored in favour of discussions over occupational or ideological continuities. If mortuary practice provides an arena for the promotion or negotiation of social identities and realities, then surely the everyday arena of the living can too?

The disappearance of writing (and the ideological disintegration of a world of 'princes and peasants'?)

With the dissolution of the Mycenaean palaces, the first Greek foray into the use of writing ends. Whether or not the recent discoveries from the Thessalian region can be shown to relate to the LH IIIC period and are thus the latest attestations of the use of the Linear B script, by the EIA finds of this syllabic script disappear. Archaeologically, the visibility of writing is undoubtedly affected to a degree by the extent of literacy, the contexts in which it was used and the use to which it was put, but I would argue that for this disappearance to apparently have been so abrupt and final there must have been a deliberate rejection of its use. The traditional view that Linear B provided the palaces with an administrative tool, essential for the operation of their economic activities and interests has contributed heavily to the problem of explaining why post-palatial society should not utilize this technology. As a purely practical, utilitarian tool, even with the concomitant social changes, there is no obvious reason for writing not to have been employed even at the most basic level of individual transactions. Furthermore, although international trade and exchange may have been significantly affected, sites such as Perati and Lefkandi demonstrate that exchange contacts survived, with imported commodities remaining an important facet of life. As a purely functional tool, writing would surely still retain a place and with the restructuring of the political landscape, it would have represented one means by which emergent leaders could distinguish themselves. Despite this, there is no evidence that Linear B, or any other writing system, was in use in the EIA and curiously, the question of why it should have disappeared has received far less attention than the associated issue of the development and introduction of the Greek alphabetic script (e.g. Jeffery 1990: 1-21; Thomas 1992: 52-61; Hall 1997: 143-44; Powell 2002; Sherratt 2003).

Although always mentioned as a defining element of post-palatial society, this disappearance is widely dismissed as a direct function of the disappearance of the palaces. With their downfall, the need for any complex administration was gone and writing, therefore, had no place (e.g. Dickinson 2006a: 72-73; Palaima 2007: 132). Not only would I conclude that this is an inappropriate and inaccurate analysis of the use of writing in the palatial period, but it is a somewhat arrogant assumption that social complexity following palatial collapse was so reduced that communities were not engaging in activities that could have made use of writing. This was not simply a technology that had outlived its usefulness in a changed political and economic climate, but something that was deliberately rejected alongside the paraphernalia of the political institutions. If interpretations of the nature of post-palatial society are even remotely correct, then the destruction of the palaces could not have resulted

directly in the loss of knowledge of writing. Whether or not officials such as the *basileus* adopted leadership roles amongst post-palatial communities, individuals previously connected, or in contact, with the palaces would possess some knowledge of this particular tool and the uses to which it was put. So we cannot simply reduce this problem to a simple correlation of the destruction of the palaces destroying the knowledge of a restricted practice, or that it simply became a redundant technology (Jeffery 1990: 13; more generally see, Houston *et al.* 2003: 467-70).

Largely confined to the palaces themselves, writing represented a defining element of elite identity, one that had no place in the identity of those living outside of the palace institution. Like wall-paintings and palatial architecture itself, writing was a visible manifestation of the socio-political ideology of the palaces – an ideology that, I suggest, led to the disenfranchisement of communities beyond the palace and was ultimately rejected. In the post-palatial period it was these accoutrements of palatial identity that disappeared from use. My analyses of the uses to which writing was put and the contexts in which it appeared do, however, suggest a plausible alternative. These suggest that far from being a benign, passive tool of administration, the Linear B tablets were intimately involved in the monitoring, emphasis and broadcasting of debts and obligations. Writing, within an oral arena, was being used to display the power of the palatial authority. Perhaps integrated with wider notions of the magical properties of writing (as is suggested by the famous Bellerophon tablet reference in the *Iliad* vi.168-69), the tablets were a physical manifestation of debts incurred. In such a situation it is entirely conceivable that writing was viewed with suspicion, mistrust and the memories of persistent obligations; a memory that endured through the post-palatial and EIA ultimately finding expression in Homeric poetry. When writing re-appeared after a substantial hiatus, the form that it took was radically different. An alphabetic script replaced the syllabic and however restricted literacy was, the contexts in which it appeared were never again so constrained to being a ‘tool of government’. Linear B, by its very nature, had defined one element of a divided society. It helped to define an ideology that, whether by design or intent, could be described crudely as dividing a world ‘of princes and peasants’.

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APPENDICES

Notes to the appendices.

Appendix A: Data Tables

Tablet find-spots

As was noted in chapter III (see note 15), tablet find-spots in this thesis are not presented in the format adopted during the excavations and that was used in all subsequent publications. Instead, they have been presented in a simpler X-Y co-ordinate system. As Pluta (1996-1997 [1998]: 233) noted, a simple transposition of the 1st and 3rd digits provide the X o-ordinate, while the 2nd and 4th digits provide the Y co-ordinate. The principle reason for adopting this form here is to simplify the plotting of relative find-spots. In practice, however, these distributions cannot be considered wholly accurate. The method adopted in recording tablet positions was originally to reference them to a hypothetical 1 metre grid superimposed on the plan of the 'Archives Complex'. Each grid square was subdivided into a smaller 10cm grid allowing for the location of each tablet to be recorded within a 10 cm grid square referenced to a particular metre grid square. The find-spot resolution is therefore not as accurate as is implied, particularly given the variability in tablet sizes. The dimensions of Jn 829, for example, are 22.0 x 11.2 x 2.5 cm. Wa 948, by contrast, measures only 2.7 x 4.5 x 0.9 cm. In the first example, the tablet could clearly encompass an area equivalent to two grid squares; the second, only a fraction of a single square. Furthermore, as Pluta (*ibid*) discussed, the original grid overlay was incorrectly positioned on the archaeological plan. The re-aligned grid that he provides, largely addresses this issue, but it is also clear from the distribution diagrams that I have presented here that problems exist with the original archaeological plan. Many of the plotted find-spots place tablets within the chasm, clearly contradicting the tablet find-spot records.

Similarly, the find-spot data does not include any relative height information, as might be expected in modern 3-axis recording systems. As such, duplicate find-spots for individual tablets have not been included here. The following tables do not, as a result, represent or constitute a record of the component elements of reconstructed tablets or a direct representation of the density of tablet fragments in any one location.

Where tablet find-spots have been provided, three sources have been examined (Bennett 1973; Palaima 1988; Bennet *et al.* n.d.). Primacy has been given to the find-spots recorded in the forthcoming, definitive publication of the 'Palace of Nestor' excavation reports (Bennett *et al.* nd). However, discrepancies are apparent between all of these sources and it remains to be seen whether further emendations are necessary. All find-spots not included in the following tables but use in this thesis are drawn directly from Bennet *et al.* (n.d.).

Appendix B: Tablet Transcriptions

This catalogue presents transcriptions for the Linear B documents central to this thesis. Documents are arranged according to the thesis chapters with the principal series from Pylos listed first. Particularly with regard to the 'checked documents' listed for chapter IV, the transcriptions cannot be understood to be an accurate portrayal of character positioning. For example, in the case of Cn 131, the position of the checkmarks is highly variable, frequently appearing below, or as close as space will permit, to the numbers being checked. With the exception of the Pylos Jn series, tablet illustrations are not included for all, but are provided where tablet or inscription characteristics are either referred to directly in analyses or exhibit general features relevant to the discussion. These illustrations, inevitably, do not always correspond to the latest transliterations. In many instances these were drawn before all the

latest joins were found. In addition, many of the fainter features were not observed in the original readings – the clearest example being the ‘checked documents’. Here many of the checkmarks are not illustrated; almost certainly the result of the difficulty of identifying marks made on dry clay. These illustrations should, therefore, be considered as a guide only.

Unless otherwise stated, all tablet transcriptions are derived from:

- Pylos: Bennett, E., L. Jr, Melena, J. L., Olivier, J.-P., Palaima, T. G. and Shelmerdine, C. W. n.d. *The Palace of Nestor at Pylos in Western Messenia. Volume IV. The inscribed documents*. University of Texas Press.
- Knossos: Killen, J. T. and Olivier, J.-P. 1989. *The Knossos tablets. Fifth Edition*. Minos Supplementary Volume 11. Salamanca: Ediciones Universidad de Salamanca.
- Thebes: Aravantinos, V. L., Freo, M. D., Godart, L. and Sacconi, A. 2005. *Thèbes Fouilles de la Cadmée IV. Les textes de Thèbes (1-433). Translittération et tableaux des scribes*. Pisa: Institut Editoriali e Poligrafici Internazionali.

Sources for tablet illustrations are provided with each image but are principally derived from:

- Pylos: Bennett, E., L. Jr 1955. *The Pylos tablets. Texts of the inscriptions found 1939-1954*. Princeton: Princeton University Press.
- Smith, J. S. 1992-1993. The Pylos Jn series. *Minos*, 27-28: 167-259.
- Knossos: Chadwick, J., Godart, L., Killen, J. T., Olivier, J.-P., Sacconi, A. and Sakellarakis, I. A. 1986. *Corpus of Mycenaean inscriptions from Knossos. Volume 1 (1-1063)*. Incunabula Graeca 88. Cambridge: Cambridge University Press.

Appendix A

Additional Data Tables

Table A-1: Data summary of the labels from Pylos (Wa series).

Table A-2: Data summary of the Sa series tablets from Pylos.

Table A-3: Individuals in the Jn series designated as *a-ta-ra-si-jo*.

Label	Scribe (Hand – Stylus)	Find-spot	Found with (tablets)	Suggested tablet associations	Associated series scribe	Subject
Wa 114	H1 – S240	13, 35 (Room 8)	Aa 60-98	Aa series (60-98?)	S60 – H4	People
Wa 362	?	24, 35; 34, 41 (Room 8)	?	An 607; Ep 704	H1 / S74 H1	?
Wa 401	H1	28, 32; 46,54 (Room 8)	?	?	?	People?
Wa 569	?	38, 40 (Room 8)	?	?	?	Objects?
Wa 730	H2 – S90?	51, 24 (Room 7)	Es/Sh series	Ma ?/ Es series	S644 – H1	Places
Wa 731	H24	51, 24 (Room 7)	Es/Sh series	Un 718?	S312 – H 24	People
Wa 732	Cii – S733	51, 24 (Room 7)	Es/Sh series	Sh 733 - 744	S733 – Cii	Objects
Wa 748	Ci	51, 24 (Room 7)	Es/Sh series	?	?	?
Wa 784	H41	63, 40 (Room 7)	Ea 756 – 936	Ea series	H43	People
Wa 917	H1 – S106?	75, 18 (Room 7)	?	Na series?	H1	People
Wa 930	H1 – S131	51, 24 (Room 7)	Es/Sh series	Cn 131, 202, 328, 421	S131 – H1	Place?
Wa 931	?	51, 24 (Room 7)	Es/Sh series	?	?	?
Wa 947	?	51, 24 (Room 7)	Es/Sh series	?	?	Place?
Wa 948	H1 – S106	51, 24 (Room 7)	Es/Sh series	Na series?	H1	Place
Wa 1008	H1 – S240	12, 31 (Room 8)	An 292, Aa 240	Aa (240 - 1182?)	S240 – H1	People
Wa 1093	Cii? – S337	36, 42 (Room 8)	Na 1089, 1091, 1092, 1094, 1097	Na series?	S106 – H1	Place?
Wa 1148	H26	Room 8	?	Sa series	S287 – H26	Objects/People?
Wa 1248	?	Room 8	?	?	?	?
Wa 1271	H26?	60, 55 (Chasm)	Sa 1264, 1265, 1266, 1267	Sa series	S287 – H26	Objects?

Table A-1: Data summary of the labels from Pylos (Wa series).

Tablet	Find-spots	Area	Scribe	Current Subset	Revised Subset
Sa 22	34,15; 35,14	Room 8	Ciii	Omitted	Omitted
Sa 287	15,29	Room 8	H 26, S 287	B	A
Sa 403	29,33	Room 8	H 26, S 287	Omitted	A
Sa 483	26,23; 28,22	Room 8	H 26, S 287	C	A
Sa 487	25,21; 26,20	Room 8	H 26, S 287	A	A
Sa 488	25,21; 26,22	Room 8	H 26, S 287	B	A
Sa 682	56,39; 53,44; 58,43; 59,40	Room 7	H 26, S 287	B	B
Sa 751	65,40; 65,41; 83,43	Room 7 / Chasm	H 26, S 287	B	B
Sa 753	72,41; 85,42	Room 7 / Chasm	H 26, S 287	A	B
Sa 755	72,38; 72,42; 81,47	Room 7 / Chasm	H 26, S 287	A	C
Sa 758	70,42; 70,43	Room 7	H 26, S 287	A	C
Sa 760	71,41; 72,41; 72,43	Room 7	H 26, S 287	A	C
Sa 761	60,55; 71,43	Room 7 / Chasm	H 26, S 287	B?	B
Sa 763	62,46; 71,42; 72,41; 72,44	Room 7	H 26, S 287	A	C
Sa 766	73,43	Room 7	H 26, S 287	A	C
Sa 767	35,55; 70,41; 73,43	Room 7 / Chasm	H 26, S 287	A	C
Sa 768	72,41; 73,41	Room 7 / Chasm	H 26, S 287	A	C
Sa 769	73,42	Room 7	H 26, S 287	A	C
Sa 774	46,54; 62,44; 73,54	Room 7 / Chasm	H 26, S 287	A	C
Sa 787	63,43; 64,40; 64,45	Room 7	H 26, S 287	C	B
Sa 790	64,40; 64,45	Room 7	H 26, S 287	B	B

Tablet	Find-spots	Area	Scribe	Current Subset	Revised Subset
Sa 791	46,54; 63,41; 63,42; 64,44	Room 7 / Chasm	H 26, S 287	B	C
Sa 793	55,24; 64,44; 66,43	Room 7	H 26, S 287	B	B
Sa 794	64,46; 72,56	Room 7 / Chasm	H 26, S 287	B	B
Sa 796	64,42; 71,42	Room 7	H 26, S 287	A	C
Sa 797	64,43; 65,41; 66,45	Room 7	H 26, S 287	A	C
Sa 834	64,55	Chasm	H 26, S 287	A	C
Sa 840	72,54; 46,54	Chasm	H 26, S 287	B?	B
Sa 843	35,55; 26,54; 60,55; 73,55	Chasm	H 26, S 287	C	C
Sa 1264	60,55	Chasm	H 26, S 287	A	C
Sa 1265	60,55	Chasm	H 26, S 287	A	C
Sa 1266	60,55	Chasm	H 26, S 287 ?	A	B
Sa 1267	35,55; 60,55	Chasm	H 26, S 287	A	C
Sa 1313		Room 98	H 26, S 287	Omitted	Omitted

Table A-2: Data summary of the Sa series tablets from Pylos.

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
Jn 310.8	<i>pa-qo-si-jo</i>	<i>a-ke-re-wa</i> (HP)	smith	No	Yes	see below, Jn 310.12	Jn 601.8; Jn 832.10	on Jn 601, a <i>basileus</i> at <i>po-wi-te-ja</i> (HP); on Jn 832, a 'finishing' smith at <i>a-to-mo</i> (HP)
Jn 310.8	<i>ke-we-to</i>	<i>a-ke-re-wa</i> (HP)	smith	No	Yes	see below, Jn 310.11	No	N/A
Jn 310.8	<i>wa-[]re-u</i>	<i>a-ke-re-wa</i> (HP)	smith	No	No	N/A	No	N/A
Jn 310.9	<i>pe-ta-ro</i>	<i>a-ke-re-wa</i> (HP)	smith	No	No	N/A	No	N/A
Jn 310.11	1 man	<i>a-ke-re-wa</i> (HP)	<i>do-e-ro</i>	<i>of ke-we-to</i>	No	N/A	No	N/A
Jn 310.11	1 man	<i>a-ke-re-wa</i> (HP)	<i>do-e-ro</i>	<i>of i-wa-ka-o</i>	No	N/A	No	N/A
Jn 310.12	1 man	<i>a-ke-re-wa</i> (HP)	<i>do-e-ro</i>	<i>of pa-qo-si-jo</i>	No	N/A	No	N/A
Jn 310.12	1 man	<i>a-ke-re-wa</i> (HP)	<i>do-e-ro</i>	<i>of po-ro-u-te-wo</i>	No	N/A	No	N/A
Jn 310.17	<i>pu₇si-ja-ko</i>	<i>a-ke-re-wa</i> (HP)	Potnian smith	No	No	N/A	An 340.7?	assigned to <i>a-ta-o</i> (see Jn 431.23 below)
Jn 320.14	<i>]ta</i>	<i>o-re-mo-a-ke-re-u</i> (HP)	smith	No	No	N/A	No	N/A
Jn 320.14	<i>do-ro-jo</i>	<i>o-re-mo-a-ke-re-u</i> (HP)	smith	No	No	N/A	Cn 45.6	herds 35 female goats at <i>u-po-ra-ki-ri-ja</i> (HP)
Jn 320.15	<i>]wo</i>	<i>o-re-mo-a-ke-re-u</i> (HP)	smith	No	No	N/A	No	N/A
Jn 320.15	<i>ta-[]-no[</i>	<i>o-re-mo-a-ke-re-u</i> (HP)	smith	No	No	N/A	No	N/A
Jn 320.15	<i>]re-u</i>	<i>o-re-mo-a-ke-re-u</i> (HP)	smith	No	No	N/A	No	N/A

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
Jn 389.12	<i>te-te-re-u</i>	<i>a-ka-si-jo-ne</i> (HP)	smith	No	No	N/A	Eb 1176.A; Ep 539.8	a <i>do-e-ro</i> of the priestess with a landholding, at <i>pa-ki-ja-ne</i> (HP)
Jn 389.12	<i>pa-pa-jo</i>	<i>a-ka-si-jo-ne</i> (HP)	smith	No	No	N/A	No	N/A
Jn 389.12	<i>pi-ro-we-ko</i>	<i>a-ka-si-jo-ne</i> (HP)	smith	No	No	N/A	No	N/A
Jn 389.12	<i>a₂-nu-me-no</i>	<i>a-ka-si-jo-ne</i> (HP)	smith	No	No	N/A	No	N/A
Jn 389.13	<i>ko-so-u-to</i>	<i>a-ka-si-jo-ne</i> (HP)	smith	No	No	N/A	No	N/A
Jn 410.10	<i>ke-ro</i>	?	smith	No	No	N/A	No	N/A
Jn 410.10	1 man	?	smith	<i>do-e-ro</i> of <i>de-ko-to-jo</i>	No	N/A	No	N/A
Jn 415.10	<i>]me-no</i>	<i>ru-ko-a₂-ke-re-u-te</i> (HP)	smith	No	No	N/A	No	N/A
Jn 415.10	<i>na-wa-to</i>	<i>ru-ko-a₂-ke-re-u-te</i> (HP)	smith	No	No	N/A	No	N/A
Jn 415.10	<i>[.]te-re-ro</i>	<i>ru-ko-a₂-ke-re-u-te</i> (HP)	smith	?	?	?	?	?
Jn 415.11	<i>ru-ki-jo</i>	<i>ru-ko-a₂-ke-re-u-te</i> (HP)	smith	No	No	N/A	No	N/A
Jn 415.11	<i>a-na-te-u</i>	<i>ru-ko-a₂-ke-re-u-te</i> (HP)	smith	No	No	N/A	No	N/A
Jn 431.10	<i>a-ta-tu-ro</i>	<i>a-pe-ke-e</i> (HP)	smith	No	No	N/A	Cn 436.2	a herder of 38 goats
Jn 431.10	<i>i-ta-ra-jo</i>	<i>a-pe-ke-e</i> (HP)	smith	No	No	N/A	No	N/A
Jn 431.10	<i>sa[</i>	<i>a-pe-ke-e</i> (HP)	smith	?	?	?	?	?
Jn 431.11	<i>wi-ja-te-wo</i>	<i>a-pe-ke-e</i> (HP)	smith	No	No	N/A	Cn 45.11; Cn 600.10	on Cn 45, a herder of 16 female goats at <i>u-po-ra-ki-ri-ja</i> (FP) with <i>a-ke-o</i> as the collector; on Cn 600 owns a flock of 80 ewes at <i>re-qa-se-wo</i>

Tablet	Name	Place	Designation	Other Designation	do-e-ro attached	do-e-ro details	Other Tablets	Other details
								wo-wo (FP)
Jn 431.11	no-e-u	a-pe-ke-e (HP)	smith	No	No	N/A	No	N/A
Jn 431.11	tu-ri-ja-jo	a-pe-ke-e (HP)	smith	No	No	N/A	No	N/A
Jn 431.11	1 man	a-pe-ke-e (HP)	smith	do-e-ro of qe-ta-ko	No	N/A	No	N/A
Jn 431.12	a-ka-ma-wo	a-pe-ke-e (HP)	smith	No	No	N/A	Cn 131.3; Cn 719.11; Jn 706.18	on Cn 131, a shepherd of 120 rams at <i>pi</i> -*82 (HP); on Cn 719 a shepherd of 96 rams at <i>wi-ja-we-ra</i> ₂ (HP) with <i>a-ko-so-ta</i> as collector; on Jn 706, at <i>pa-to-wo-te</i> a smith without an allocation (see Jn 706.18 below).
Jn 431.12	e-pe-ke-u	a-pe-ke-e (HP)	smith	No	No	N/A	No	N/A
Jn 431.12	*82-de	a-pe-ke-e (HP)	smith	No	No	N/A	Cn 600.15	a swineherd with 12 sows at <i>ti-mi-to a-ko</i> (FP)
Jn 431.12	pu-te-u	a-pe-ke-e (HP)	smith	No	No	N/A	No	N/A
Jn 431.13	ko-ne-wa-ta	a-pe-ke-e (HP)	smith	No	No	N/A	No	N/A
Jn 431.13	qe-to-ro-no	a-pe-ke-e (HP)	smith	No	No	N/A	Jn 431.5	smith with an allocation of AES M 7
Jn 431.13	mo-re-u	a-pe-ke-e (HP)	smith	No	No	N/A	Jn 389.6; Jn 750.10	on Jn 389, a smith an allocation of AES M1 N2 at <i>a-ka-si-jo-ne</i> (HP); on Jn 750, a

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
								smith with an unknown allocation at <i>a-si-ja-ti-ja</i> (FP)
Jn 431.13	<i>a-e-ri-qi</i>	<i>a-pe-ke-e</i> (HP)	smith	No	No	N/A	No	N/A
Jn 431.22	<i>ka-ri-se-u</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	No	No	N/A	No	N/A
Jn 431.22	<i>du-ko-sof</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	No	No	N/A	No	N/A
Jn 431.23	<i>kof</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	?	?	?	?	?
Jn 431.23	<i>e-u-wa-ko-ro</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	No	No	N/A	No	N/A
Jn 431.23	<i>ke-we-no</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	No	No	N/A	Cn 600.1	a shepherd of 50 rams at <i>o-re-e-wo wo-wo</i> (FP)
Jn 431.23	<i>a-ta-o</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	No	No	N/A	An 340.1-14; Vn 34.5	on An 340, lists named individuals (craftsmen?) responsible to him; on Vn 34, the woman <i>pi-ro-pa-ta-ra</i> is designated 'of <i>a-ta-o</i> '
Jn 431.24	<i>fw-ti-ro</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	?	?	?	?	?
Jn 431.24	<i>me-ri-wa-[.]</i>	<i>a-pe-ke-e</i> (HP)	Potnian smith	?	?	?	?	?
Jn 431.25	10 men	<i>a-pe-ke-e</i> (HP)	Potnian smith	of <i>i-je-ref</i>	?	?	?	?
Jn 431.25	5 men	<i>a-pe-ke-e</i> (HP)	Potnian smith	of <i>to-sa-no-jo</i>	?	?	?	?
Jn 431.26	1 man	<i>a-pe-ke-e</i> (HP)	Potnian smith	of?	?	?	?	?
Jn 431.26	31 men	<i>a-pe-ke-e</i> (HP)	Potnian smith	of <i>a-mu-ta-wo-no</i>	?	?	?	?
Jn 478.10	<i>e-u-ro-to-qi</i>	<i>wi-ja-we-ra</i> ₂ (HP)	smith	No	No	N/A	No	N/A

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
Jn 601.11	<i>po]ti-na-jo</i>	<i>po-wi-te-ja</i> (HP)	smith	No	No	N/A	Jn 692.7; Jn 725 ??	if correctly reconstructed as <i>po-ti-na-jo</i> then this may be the smith on Jn 692 without an allocation at <i>na-i-se-wi-jo</i> (HP) and an unknown allocation on Jn 725 at the same place
Jn 601.12	<i>po-so-ri-jo[</i>	<i>po-wi-te-ja</i> (HP)	smith	No	No	N/A	Aq 64.7; Jo 438	??
Jn 601.12	<i>]ne-wo</i>	<i>po-wi-te-ja</i> (HP)	smith	No	No	N/A	No	N/A
Jn 601.12	<i>i-pe-ra-ta</i>	<i>po-wi-te-ja</i> (HP)	smith	No	No	N/A	No	N/A
Jn 601.13	<i>sa-nu-[</i>	<i>po-wi-te-ja</i> (HP)	smith	No	No	N/A	No	N/A
Jn 605.7	<i>wi-ti-mi-jo</i>	<i>a-pi-no-e-wi-jo</i> (HP)	smith	No	No	N/A	No	N/A
Jn 605.7	<i>ma-no-u-ro</i>	<i>a-pi-no-e-wi-jo</i> (HP)	smith	No	No	N/A	Jn 692.5; Jn 725 r orig. 19	on Jn 692, at <i>na-i-se-wi-jo</i> (HP) a smith without an allocation (see Jn 692.5 below); on Jn 725, a smith with a joint allocation of AES M 12 with 7 additional smiths at <i>na-i-se-wi-jo</i> (HP)
Jn 605.7	<i>a-we-ke-se-u</i>	<i>a-pi-no-e-wi-jo</i> (HP)	smith	No	No	N/A	Cn 131.9; Cn 285.5-6; Cn 595.2	on Cn 131, a shepherd of 170 penned rams at <i>ma-ro</i> (HP); on Cn 285, herds 50 rams and 30 male goats at <i>ro-u-so</i> (HP); on Cn 595, a deficit of 5

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
								OVIS+TA is recorded at <i>me-ta-pa</i> (HP)
Jn 605.10	2 men	<i>a-pi-no-e-wi-jo</i> (HP)	<i>do-e-ro</i>	of <i>pe-re-qo-no</i>	No	N/A	No	N/A
Jn 605.10	2 men	<i>a-pi-no-e-wi-jo</i> (HP)	<i>do-e-ro</i>	of <i>a3-ki-e-wo</i>	No	N/A	No	N/A
Jn 605.10	1 man	<i>a-pi-no-e-wi-jo</i> (HP)	<i>do-e-ro</i>	of <i>mi-ka-ri-jo</i>	No	N/A	No	N/A
Jn 605.11	1 man	<i>a-pi-no-e-wi-jo</i> (HP)	<i>do-e-ro</i>	of <i>pu-ra-ta</i>	No	N/A	No	N/A
Jn 692.5	<i>ra-ma-jo</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	Cn 285.9; Jn 725 r. orig. 19; Ub 1316?	on Cn 285, a herder of 20 female goats at <i>ro-u-so</i> (HP); on Jn 725, a smith with an unknown allocation at <i>na-i-se-wi-jo</i> (HP); on Ub 1316, recorded as having a deficit of 8 deerskins
Jn 692.5	<i>ma-no-u-ro</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	Jn 605.7; Jn 725 r. orig. 19	on Jn 605, at <i>a-pi-no-e-wi-jo</i> (HP) a smith without an allocation (see Jn 605.7 above); on Jn 725, a smith with a joint allocation of AES M 12 with 7 additional smiths at <i>na-i-se-wi-jo</i> (HP)
Jn 692.6	<i>re-u-si-wo</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	An 1281.3; Jn 725 r. orig. 19	on An 1281, assigned to the shrine of Potnia Hippeia; on Jn 725, a smith with an unknown allocation at <i>na-i-se-wi-jo</i> (HP)

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
Jn 692.6	<i>ke-ki</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	Jn 725 r. orig. 20	a smith with an unknown allocation at <i>na-i-se-wi-jo</i> (HP)
Jn 692.6	<i>ri-ku-we</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	Jn 725 r. orig. 20	a smith with an unknown allocation at <i>na-i-se-wi-jo</i> (HP)
Jn 692.7	<i>po-ti-na-jo</i>	<i>na-i-se-wi-jo</i> (HP)	smith	No	No	N/A	Jn 725 r. orig. 20	a smith with an unknown allocation at <i>na-i-se-wi-jo</i> (HP)
Jn 693.10	<i>ta-na-wo</i>	<i>a-pu₂</i> (HP)	smith	No	No	N/A	No	N/A
Jn 693.10	<i>pe-qe-u</i>	<i>a-pu₂</i> (HP)	smith	No	No	N/A	Cn 45.5	a shepherd with a flock of 65 ewes at <i>u-po-ra-ki-ri-ja</i> (FP) with <i>a-ke-o</i> as the collector
Jn 706.16	<i>ka-ri-si-jo</i>	<i>pa-to-wo-te</i>	smith	No	No	N/A	No	N/A
Jn 706.17	<i>ko-a₂-ta</i>	<i>pa-to-wo-te</i>	smith	No	No	N/A	No	N/A
Jn 706.17	<i>ko-do-ro</i>	<i>pa-to-wo-te</i>	smith	No	No	N/A	No	N/A
Jn 706.18	<i>a-ka-ma-wo</i>	<i>pa-to-wo-te</i>	smith	No	No	N/A	Cn 131.3; Cn 719.11; Jn 431.12	on Cn 131, a shepherd with 120 rams at <i>pi-*82</i> (HP); on Cn 719, a shepherd with 96 rams at <i>wi-ja-we-ra₂</i> (HP) with <i>a-ko-so-ta</i> as collector; on Jn 431, at <i>ape-ke-e</i> (HP) a smith without an allocation (see Jn 431.12 above)

Tablet	Name	Place	Designation	Other Designation	<i>do-e-ro</i> attached	<i>do-e-ro</i> details	Other Tablets	Other details
Jn 750.12	<i>du-wo-jo</i>	<i>a-si-ja-ti-ja</i> (FP)	smith	No	No	N/A	An 656.11; Vn 34.9	on An 656, an <i>o-ka</i> commander at <i>a-ke-re-wa</i> (HP); on Vn 34, amongst a list of men with individual women attached to them
Jn 750.12	<i>di-ra-wo-no</i> [<i>a-si-ja-ti-ja</i> (FP)	smith	No	No	N/A	No	N/A
Jn 750.13	<i>l man</i>	<i>a-si-ja-ti-ja</i> (FP)	smith	<i>do-e-ro</i> of <i>e-u-we-te-ro</i> , who has an allocation on the same text	No	N/A	No	N/A
Jn 750.13	<i>e</i> [<i>a-si-ja-ti-ja</i> (FP)	smith?	?	?	?	?	?
Jn 832.5	<i>j-ka-no</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	No	N/A
Jn 832.5	<i>ma-ri-ta</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	No	N/A
Jn 832.5	<i>ka-ra-u-ko</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	Cn 285.4; Jn 706 r.8	on Cn 285, a shepherd of 30 male goats at <i>ro-u-so</i> (HP); on Jn 706, a smith with an unknown allocation at <i>pa-to-wo-te</i>
Jn 832.5	<i>wo-ti-jo</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	An 340.8	a 'finishing' smith?; assigned to <i>a-ta-o</i> (see Jn 431.23 above)
Jn 832.7	<i>wa-na-ta-jo</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	Eo 211.1-5; Eo 224.5; Ep 301.3; Eb 369.A; En 609.3-4	

Tablet	Name	Place	Designation	Other Designation	do-e-ro attached	do-e-ro details	Other Tablets	Other details
Jn 832.7	<i>ka-u-sof</i>	<i>ro-u-so</i> (HP)	smith	No	No	N/A	No	N/A
Jn 832.8	<i>ka-[-</i>	<i>ro-u-so</i> (HP)	smith	?	?	?	?	?
Jn 832.14	<i>a-ri-qa</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	No	N/A
Jn 832.14	<i>o-na-jo</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	No	N/A
Jn 832.14	<i>si-pa-ta-no</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	No	N/A
Jn 832.15	<i>a-da-ma-o</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	Eb 747. A; Ep 301.4; Eo 351.1-2, En 659.8- 9	
Jn 832.15	<i>a-no-ra-ta</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	An 340.12	assigned to <i>a-ta-o</i> (see Jn 431.23 above)
Jn 832.15	<i>pu-wa-ne</i>	<i>a-to-mo</i> * (HP)	smith	No	No	N/A	No	N/A
Jn 845.11	<i>po-so-ro</i>	?	smith	No	No	N/A	An 1281.6; Jn 601.5; Jn 750.3	on An 1281, assigned to the woman <i>me-ta- ka-wa</i> at the shrine of Potnia Hippeia; on Jn 601, a smith with an allocation of AES M 8 at <i>po-wi-te-ja</i> (HP); on Jn 750, a smith with an allocation of AES M 1 N 2 at <i>a-si-ja-ti-ja</i> (FP)
Jn 845.11	<i>na-pu-ti-jo</i>	?	smith	No	No	N/A	No	N/A
Jn 845.11	<i>ma-ta-ko</i>	?	smith	No	No	N/A	No	N/A

Tablet	Name	Place	Designation	Other Designation	do-e-ro attached	do-e-ro details	Other Tablets	Other details
Jn 845.12	<i>ku-ke-re-u</i>	?	smith	No	No	N/A	No	N/A
Jn 845.12	<i>a-ti-ja-wo</i>	?	smith	No	No	N/A	An 654.2	an officer in the <i>o-ka</i> of <i>ku-ru-me-no</i> , possibly at <i>me-ta-pa</i> (HP)
Jn 845.12	<i>wa-ra-ko-no</i>	?	smith	No	No	N/A	No	N/A
Jn 845.13	<i>qe-re-me-ne-u</i>	?	smith	No	No	N/A	No	N/A
Jn 927.3	<i>a-tu-ko</i>	<i>]me-no</i>	smith	No	No	N/A	En 609.5; Eo 211.2; Ep 301.5; Jn 725.7; Jn 658.10; Eb 903	Armorer of the Wanax; <i>ki-ti-me-na</i> landholder from the <i>te-re-ta wa-na-ta-jo</i> ; smith with two allocations
Jn 927.3	<i>o-pi-si-jo</i>	<i>]me-no</i>	smith	No	No	N/A	No	N/A
Jn 927.4	<i>e-pi-ta-jo</i>	<i>]me-no</i>	smith	No	No	N/A	No	N/A
Jn 927.4	<i>i-mo-ro-ne[-u</i>	<i>]me-no</i>	smith	No	No	N/A	No	N/A

Table A-3: Individuals in the Jn series designated as *a-ta-ra-si-jo*.

Appendix B

Linear B Tablet Transcriptions

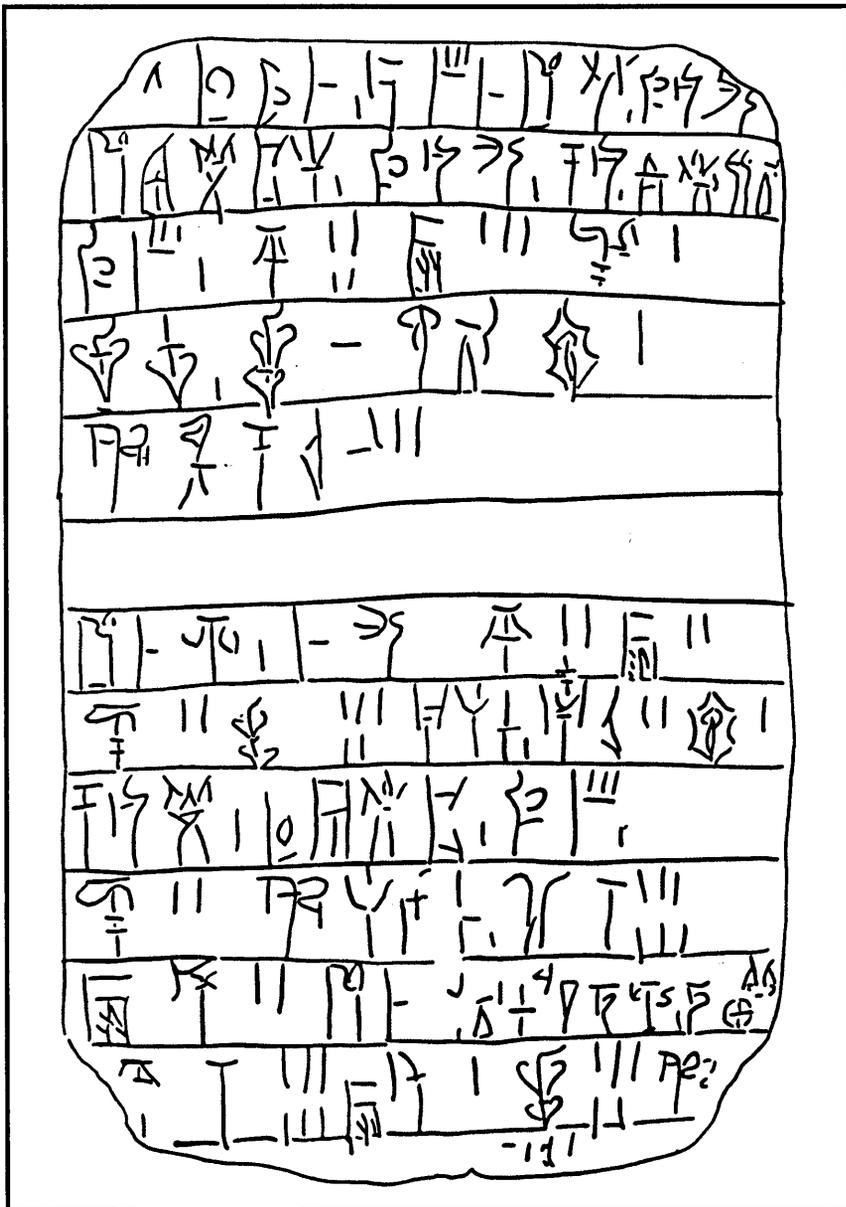
The principal documents discussed in,

Chapter II

Nestor's 'Archives Complex': Fact or Fiction?

Pylos

Un 718



Un 718 (Bennett 1955: 83)

Un 718

(S312 H24)

- .1 sa-ra-pe-da , po-se-da-o-ni , do-so-mo
 .2 o-wi-de-ta-i , do-so-mo , to-so , e-ke-ra₂-wo
 .3 do-se , GRA 4 VIN 3 BOS^m 1
 .4 tu-ro₂ , TURO₂ 10 ko-wo , *153 1
 .5 me-ri-to , V 3.
 .6 vacat
 .7 o-da-a₂ , da-mo , GRA 2 VIN 2
 .8 OVIS^m 2 TURO₂ 5 a-re-ro , AREPA V 2 *153 1
 .9 to-so-de , ra-wa-ke-ta , do-se ,
 .10 OVIS^m 2 me-re-u-ro , FAR T 6
 .a -ma
 .11 VIN S 2 o-da-a₂ , wo-ro-ki-jo-ne-jo , ka-
 .12 GRA T 6 VIN S 1 TURO₂ 5 me-ri[
 .13 vacat [me-]ri-to V 1

The principal documents discussed in,

Chapter III

Setting the public agenda? Labelling gifts and obligations at Pylos.

Pylos

Wa series

(Wa 114, 362, 401, 569, 730, 731, 732, 748, 784, 917, 930, 931,
947, 948, 1008, 1093, 1148, 1248, 1271)

Sa Series

(Sa 22, 287, 403, 483, 487, 488, 682, 751, 753, 755, 758, 760, 761,
763, 766, 767, 768, 769, 774, 787, 790, 791, 793, 794, 796, 797,
834, 840, 843, 1264, 1265, 1266, 1267, 1313)

Thebes

Of 36

The Wa Series 'labels'

The Wa series 'labels' are presented in a tabular arrangement for convenience only. Transliterations here maintain the conventional format despite this arrangement. As was discussed in chapter III, palaeographic identifications of scribal hands should be used cautiously since these have often been justified through assumed associations with other tablet series.

Tablet	Scribal Hand	Transliteration	Possible reconstruction	Suggested Translation
Wa 114	H1(S240)	.1 me-ni-jo ,MUL .2 pe-ra ₃ -ko-ra-i-ja , ko[.1 me-ni-jo , MUL .2 pe-ra ₃ -ko-ra-i-ja , ko-wa/wo	monthly ration, women of the Further Province, boys/girls
Wa 362	?	.1 <i>vacat</i> []-to-ṣe[.2 <i>vest.</i> []te-wi-ja[.1]-to-se[.2 ki-ri-te-wi-ja	? (to the?) <i>ki-ri-te-wi-ja</i>
Wa 401	H1	.1] <i>vacat</i> [.2]ta , a-ko-[?	?
Wa 569	?	.1]ḏe to-ra pe[.2] <i>vacat</i> [?	?
Wa 730	H2(S90)?	.1]-ra-o , ḏa-so-mo 10 .2 sa-ma-ra , do-si-mi-jo-qe 1	.1]-ra-o , da-so-mo 10 .2 sa-ma-ra , do-si-mi-jo-qe 1	
Wa 731	H24	.A do-ṣo-mo .B]o-wi-de-ta[.A do-so-mo .B o-wi-de-ta-i	contribution to the sheep-flayers
Wa 732	Class ii (S337?)	.1]ṭo-ra-ke	.1 to-ra-ke	corslets
Wa 748	Class i]wi-to-te ra[?	?
Wa 784	H41	.1]o-na-te-re[.2]ko-to-na-o	.1 o-na-te-re .2 ko-to-na-o	holders of <i>ktoina</i> land
Wa 917	H1(S106?)	.1]o-da-sa-ṭo , a-ko-so-ṭa[.2]e-qe-ta , e-re-u-te-re[.1 o-da-sa-to , a-ko-so-ta .2 e-qe-ta , e-re-u-te-re	<i>a-ko-so-ta</i> distributed to the <i>heqⁿetai e-re-u-te-re</i>

Tablet	Scribal Hand	Transliteration	Possible reconstruction	Suggested Translation
Wa 930	H1 (S131)]re-ke, [we-re-ke	enclosures
Wa 931	?	a-ri[]kø[a-ri-qo-ko	<i>a-ri-qo</i> (Place name) + ?
Wa 947	?]do-ro	ka-ra-do-ro	<i>ka-ra-do-ro</i> (Place name)
Wa 948	H1 (S106?)]dę-we-ro-a ₃ -kø-ra[de-we-ro-a ₃ -ko-ra	(Name of the) Hither Province
Wa 1008	H1 (S240)	.1 MUL [.2 o-si-to-[.1 MUL .2 o-si-to-ko-wo	Women, thus the ‘
Wa 1093	Class ii	me-te-[me-te-to	<i>me-te-to</i> (Place name)
Wa 1148	H26	.1 a-]mø-ta , e-qe-si-ja[.2 we-]ję-ke-a	.1 a-mo-ta , e-qe-si-ja .2 we-je-ke-a	wheels belonging to the <i>heq"etai</i> (serviceable)
Wa 1248	?]jo ko[?	?
Wa 1271	H26?]rą-ke [/je-ke	wo-ka we-je-ke	serviceable wheels

The Sa Series

Tablets are presented here in the groups suggested in Chapter III. Despite being presented in a tabular arrangement, the standard form of transliteration has been maintained within the tables. With the exception of Sa 22 (Class iii) and Sa 137 (Class ii), all tablets of this series are attributed to H26 (S287). Sa 137 has not been included in this analysis, because although this has been reclassified as an Sa series tablet, this must be considered as extremely dubious. Very little of the inscription has been preserved and none that identifies the subject matter (Bennett *et al.* nd).

Tablet	Transliteration	Suggested Translation
Sa 287	a-ku-ro , dē-de-me-no , ROTA ZE 1	bound with silver, 1 pair of wheels
Sa 403] ROTA ZE 2	2 pairs of wheels
Sa 483	to-ṣa ROTA+TE ZE 10[MO] 1 [so many wheels with rims ¹ , 10 pairs, 1 single
Sa 487	ke-ro-ke-re-we-o , wo-ka we-je-ke-e , ROTA+TE ZE 2[serviceable chariot wheels with rims of <i>ke-ro-ke-re-we-o</i> , 2+ pairs
Sa 488	ku-pa-ri-se-ja , ROTA+TE ZE 1 MO 1	of Cypress wood, wheels with rims, 1 pair, 1 single

Translations of the Sa series tablets in the modified group A subset.

¹ The TE ligature to the ROTA ideogram, thought to be the abbreviated form of *te-mi-dwe-ta*, is here translated in an extremely 'loose' fashion as a wheel rim. The precise meaning remains unclear (e.g. Palmer 1963: 321, 456; Chadwick 1973: 584), but is understood more generally as 'provided with a *termis*' (e.g. Palaima 1996b: 40). My choice to use 'rim' reflects a desire to simplify the discussion of this series, which is unaffected by the precise meaning of this term.

Tablet	Transliteration	Suggested Translation
Sa 682	te-tu-ko-wo-a ₂ , no-pe-re-a ₂ , ROTA ZE 6	fully worked, unserviceable wheels, 6 pairs
Sa 751	za-ku-si-ja, no-pe-re-a ₂ , ROTA+TE ZE 32 [unserviceable Zakynthian wheels with rims, 32 pairs
Sa 753	se-we-ri-ko-jo, wo-ka, e-qe-si-jo, ROTA+TE ZE 2 [for the <i>heq"etai</i> , chariot wheels with rims, of <i>se-we-ri-ko-jo</i> , 2 pairs
Sa 761	a-sa-má-to, ZE 2	of <i>a-sa-ma-to</i> , 2 pairs
Sa 787 ²	.A pa-ra-ja, we-je-ke-a ₂ , ROTA ZE 30 MO 1 .B to-sa, e-qe-si-ja pa-ra-ja ROTA ZE 12 za-ku-si-ja ROTA ZE 32	so many, old wheels belonging to the <i>heq"etai</i> , 12 pairs; Zakynthian wheels, 32 pairs; old, serviceable wheels, 30 pairs, 1 single
Sa 790	a-mo-ta, e-qe-si-ja, no-pe-re-a ₂ , ROTA+TE ZE 6	unserviceable chariot wheels with rims belonging to the <i>heq"etai</i> , 6 pairs
Sa 793	e-re-pa-to, te-mi-dwe-ta, pa-ra-ja, ta-na-wa, ROTA+TE ZE 11	old wheels with ivory rims worn thin, 11 pairs
Sa 794	ka-ko, de-de-me-no, no-pe-re-e, ROTA ZE 1 [unserviceable wheels, bound with bronze, 1 pair
Sa 840	ke-ra-e, te[]ROTA+TE ZE 1 [horn?, wheels with rims, 1 pair
Sa 1266 ³	.a te-mi-de-we-te e-pi-wo-qa-ta-o, wo-ka, we-je-ke-e [ROTA ZE] 1	serviceable chariot wheels with rims of <i>e-pi-wo-qa-ta-o</i> , 1 (single?)

Translations of the Sa series tablets in the modified group B subset.

Tablet	Transliteration	Suggested Translation
Sa 22]te-ra-u-re-o EQU ZE 1	(of) te-ra-u-re-o, 1 pair of horses
Sa 1313]wę-je-ke-e[ROTA	serviceable [wheels?

Sa series tablets omitted from the modified groups.

² This is a revised transliteration, based upon the suggestion that the 1 of 31 may have been an accidental stroke (Bennett & Olivier 1973: 226). In chapter III, I maintain that the original reading of 31 was in fact correct. However for the purposes of convention, I have maintained the currently accepted transliteration.

³ Sa 1266 is a palimpsest text, vestiges of which can be read on both the *verso* and *recto*.

Tablet	Transliteration	Suggested Translation
Sa 755	qa-sa-re-o , wo-ka we-je-ke-e ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>qa-se-re-o</i> , 1 pair
Sa 758	tu-ri-si-jo-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>tu-ri-si-jo-jo</i> , 1 pair
Sa 760	e-ke-i-jo-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>e-ke-i-jo-jo</i> , 1 pair
Sa 763	wo-ro-ko-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 1 [serviceable chariot wheels with rims of <i>wo-ro-ko-jo</i> , 1 pair
Sa 766	wa-de-o , wo-ka , we-je-ke-e , ROTA+TE ZE 2 [serviceable chariot wheels with rims of <i>wa-de-o</i> , 2 pairs
Sa 767	a-pa-si-jo-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 1 [serviceable chariot wheels with rims of <i>a-pa-si-jo-jo</i> , 1 pair
Sa 768	pe-qe-ro-jo , wo-ka we-je-ke-e , ROTA+TE ZE 1 [serviceable chariot wheels with rims of <i>pe-qe-re-jo</i> , 1 pair
Sa 769	e-te-wa-jo-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 2	serviceable chariot wheels with rims of <i>e-te-wa-jo</i> , 2 pairs
Sa 774	mo-qo-so-jo , wo-ka , we-je-ke-e ROTA+TE ZE 1[serviceable chariot wheels with rims of <i>mo-qo-so</i> , 1 pair
Sa 791	te-mi-dwe-ta , we-je-ke-a ₂ ROTA+TE ZE 2 [serviceable wheels with rims, 2pairs
Sa 796	po-ru-we-wo wo-ka we-je-ke-e ROTA+TE ZE 2	serviceable chariot wheels with rims of <i>po-ru-we-wo</i> , 2 pairs
Sa 797	a-te-wo-jo , wo-ka , we-je-ke-e ROTA+TE ZE 1 [serviceable chariot wheels with rims <i>a-te-wo-jo</i> , 1 pair
Sa 834	a-me-ja-to , wo-ka , we-je-ke-e ROTA+TE ZE 1	serviceable chariot wheels with rims <i>a-me-ja</i> , 1 pair
Sa 843	to-sa , we-je-ke-a ₂ , ne-wa , ROTA+TE ZE 20	so many new, serviceable wheels with rims, 20 pairs
Sa 1264	e-ti-ra-wo-jo , wo-ka , we-je-ke-e , ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>e-ti-ra-wo</i> , 1 pair
Sa 1265	a-re-to-to , wo-ka , we-je-ke-e , ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>a-re-to-to</i> , 1 pair
Sa 1267	e-te-wa-jo , wo-ka , we-je-ke-e ROTA+TE ZE 1	serviceable chariot wheels with rims of <i>e-te-wa-jo</i> , 1 pair

Translations of the Sa series tablets in the modified group C subset.

The principal documents discussed in,

Chapter IV

**Patronage in an oral context: Rethinking the role
of the tablets.**

Pylos

'Checked documents'

(Ab 585, An 39, 594, Cn 131, 155, 200, 328, 436, 437, 485, 491, 1059, Eb 339, Fr
1255, Jo 438, Vn 851)

Others

Er 312

Thebes

Of 36

Checked Documents

Ab 585

(S186 H21)

] 6 × [

An 39

(Ci, Ciii)

Recto

- | | | | | | | |
|-----|----------------------|------------------|-------|--|--|---|
| | | | | | | |
| .1 | pu-ka-wo × | VIR 16 | | | | x |
| .2 | me-ri-du-ma-te | VIR 10 × | | | | |
| .3 | mi-ka-ta × | VIR 3 | | | | |
| .4 | o-pi-te-u-ke-e-we | VIR 4 × | | | | |
| .5 | e-to-wo-ko × | VIR 5 | | | | |
| .6 | ka-sa-to × | VIR | | | | |
| .7 | pu-ka-wo × | VIR 23 | | | | |
| .8 | me-ri-da-ma-te , | VIR 6 | | | | |
| .9 | o-pi-]te-u-ke-e-we , | VIR 5 × | | | | |
| .10 | mi-ka-]ta , | VIR 6 × | | | | |
| .11 | e-]t̥o-wo-ko , | VIR 4 a-to-po-qo | VIR 3 | | | |

w

Verso

- | | | | | | | |
|-----|--------------------------|-------|------------|-----|-----|--|
| | | | | | | |
| x | | | | | | |
| .1 | po-ru-da-ma-te | VIR 4 | | | | |
| .2 | <i>vacat</i> | | | | | |
| .3 | qa-ra ₂ -te , | VIR | | | | |
| .4 | pu-ko-ro , | VIR | | | | |
| .5 | a-ko-so-ta , | VIR | | | | |
| .6 | pi-ri-ja-me-ja | VIR | | | | |
| .7 | e-ni-ja-u-si-jo | VIR | | | | |
| .8 | p̥t̥e-jo-ko | VIR | qo-ta-wo | VIR | [| |
| .9 | a-ta | VIR | te-o-po-qo | [| VIR | |
| .10 | <i>vestigia</i> | [| | | | |

w

An 594

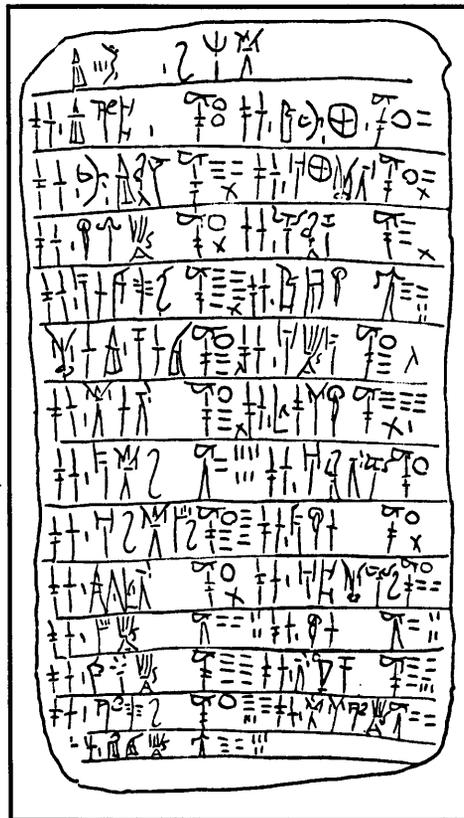
(S39v Ciii, Ciii)

Recto

- | | | | | | | |
|----|------------------|-------|------------|---------|--|--|
| | | | | | | |
| .1 | ma-ri-ti-wi-jo × | VIR 1 | pu-ka-wo × | VIR 10 | | |
| .2 | me-ri-du-ma-te | VIR 4 | mi-ka-ta | VIR 1 × | | |

Verso originalis

- | | | | | | | |
|----|-----------------|--|--|--|--|--|
| | | | | | | |
| .1 | <i>vestigia</i> | | | | | |
| .2 | [pu-]ka-wo | | | | | |



Cn 131 (Bennett 1955: 17)

Cn 131

(S131 H1)

- | | | |
|-----|-----------------------|---|
| .1 | pi-*82 , we-re-ke | |
| .2 | pa-ro , pi-me-ta , × | OVIS ^m 200 pa-ro , o-ku-ka , OVIS ^m × 130[|
| .3 | pa-ro , ku-pi-ri-jo , | OVIS ^m 50 × pa-ro , a-ka-ma-wo OVIS ^m 120 × |
| .4 | pa-ro , ko-ru-no | OVIS ^m 100 × pa-ro , ne-ri-to OVIS ^m 30 × |
| .5 | pa-ro , po-ro-u-te-we | OVIS ^m 90 × pa-ro , o-wa-ko CAP ^f 54 × |
| .6 | ma-ro-pi , to-ro-wi | OVIS ^m 130 × pa-ro , a-no-po OVIS ^m 130 × |
| .7 | pa-ro , ke-ro-wo | OVIS ^m 130 × pa-ro , ra-pa-sa-ko OVIS ^m 91 × |
| .8 | pa-ro , po-ke-we | OVIS ^f 27 × pa-ro , a-ri-wo-ne × OVIS ^m 100 |
| .9 | pa-ro , a-we-ke-se-we | OVIS ^m 170 × pa-ro , po-ko-ro OVIS ^m 100 × |
| .10 | pa-ro , e-ti-ra-wo | OVIS ^m 100 × pa-ro , a-ta-ma-ne-we OVIS ^m 140 × |
| .11 | pa-ro , se-no | OVIS ^f 44 × pa-ro , ko-ro OVIS ^f 24 × |
| .12 | pa-ro , do-qo-no × | OVIS ^m 80 pa-ro , wo-ki-to × OVIS ^m 73 |
| .13 | pa-ro , me-te-we | OVIS ^m 163 × pa-ro , ke-sa-me-no OVIS ^f 40 × |
| .14 |]pa-ro , pu-wi-no | CAP ^f 55 |

Cn 155

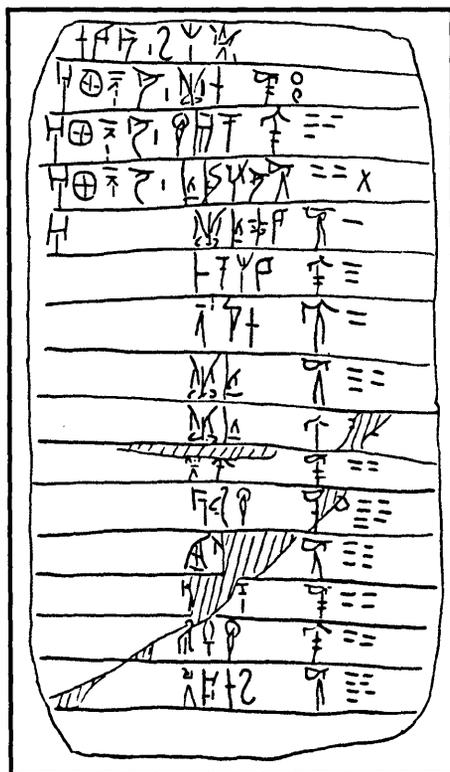
(S155 Ci)

- | | |
|----|---|
| .1 | OVIS ^m 204 OVIS ^x [|
| .2 | × CAP ^m [|
| .3 | vestigia |

Cn 200

(S155 Ci)

- | | | |
|----|-----------------------|-------------------------|
| .1 | wa-wo-u-[] , pa-ra-ku | OVIS ^x 160 × |
| .2 | i-sa-ma[-]tā | CAP ^m 63 |



Cn 328 (Bennett 1955: 39)

Cn 328

(S131 H1)

.1	ro-u-so , we-re-ke ,	
.2	a-ka-na-jo , ma-ro	OVIS ^m 200 ×
.3	a-ka-na-jo , ko-wa-to	CAP ^m 50 ×
.4	a-ka-na-jo , ra-mi-ni-jo	CAP ^f 40 ×
.5	a ma-ra-te-u	OVIS ^f 10 ×
.6	da-to-re-u	CAP ^m 30
.7	wo-ki-ro	CAP ^f 20 ×
.8	ma-ra	OVIS ^f 40 ×
.9	ma-ra	CAP 40
.10	ke-zo	OVIS ^m 40 ×
.11	po-ri-ko	OVIS ^m 170 ×
.12	wi-sa-t̄o	OVIS ^f 60 ×
.13	a[]to	OVIS ^m 40 ×
.14	pu-za-ko	CAP ^m × 40
.15	a ₃ -ta-ro-we	OVIS ^f 70 ×

Cn 436

(S131 H1, Cii?)

.1	ke-to	OVIS 100 ×
.2	a-ta-tu-ro	CAP ^m 38
.3	a-mi-nu-wa-ta	OVIS ^f 50 × [
.4	o-ko	OVIS ^f 100 × [
.5	i-ma-di-jo	OVIS ^f [<i>qs</i>
.6	ko-tu-ro ₂ []	CAP ^f [<i>qs</i>
.7	wa-ra-pi-si-ro[]	CAP ^x [<i>qs</i>
.8	e-qa[
.9		<i>deest</i>
.10	vestigia[

Cn 437**(S719 H1)**

- .1] vacat
 .2]no-[-]-ta[] OVIS^m[qs] ×
 .3]wo-wo , po-ru-qo-ta[]54[] vac.
 .4]wo-wo , [] vac.[]60[
 .5]wq-wo , [] e-wi-te-u OVIS^m 50[

Cn 485**(S719 H1)**

- | | | | |
|-----|------------------------|---|-------------------------------|
| .1 | da-we-u[-pi] , kq[| | |
| .2 | da-we-u-pi , a[| α | .1] OVIS ^f × 15 [|
| .3 | da-we-u-pi , ka[| | |
| .4 | da-we-u-pi , e-[| β | .1] , ka[|
| .5 | a ₃ -zo-wq[| | |
| .6 | da-we[-u-]pi wq[| γ | .1]no-wo[|
| .7 | da-we-u[-pi] , e-ke[| | |
| .8 | da-we-u[-pi | δ | .1]ma ₃ -jo-wo-[|
| .9 | a-re[| | |
| .10 | a-zq | | |
| | [| | |

Cn 491**(S131 H1)**

- .1] OVIS^m 60 × []
 .2]e-u OVIS^f 40 []
 .3]pi-ta OVIS^f 10 []
 .4]re-ro CAP [qs]
 .5]pe-se-to CAP^f 46
 .6] , e-ta-wo-jo 12

Cn 1059**(S719 H1)**

- .1] , []ke-u [] 20 × []
 .2]ko [] vac.[]
 .3]wq-wo , ne[]
 .4]wo , []
 .5]ka-ni[]

Eb 339**(H41)**

- .A i-je-re-ja , pa-ki-ja-na , e-ke-qe[o-na-to ke-ke-me-na] ×
 .B ko-to-na , pa-ro da₃-mo [] to-so-de]pe-mo GRA T 4

Fr 1255**(Cii)**

- .a]zq-tq
]nu-wi-jo^aV 1 × []

*Recto**Latus dextrum*

f			
.1]ko-re-te	
.2]kq[] AUR M 1	
.3	e-re-[] AUR N 1	
.4	po-ro-ko-re[-te] AUR N 2	
.5	do-ri-ka-o , mo-ro-[qa	AUR]N 1	
.6	ru-ro , mo-ro-qa	AUR [] 1	
.7	ne-da-wa-ta	AUR [qs]	
.8	e-ke-me-de	AUR N[qs]	
.9	ro-]u-so , ko-re-te	AUR P 4[]	
.10	pa-ki-ja-ni[-ja ko-]re-te[] AUR P 4[]	×
.11	a-pu ₂ -ja , ko-re-te[] AUR P 5	
.12	ka-ra-do-ro , ko-re-te[] AUR P 5	×
.13]kq-re-te[]	AUR P 5	×
.14]]	AUR N 1	
.15]ma	AUR P 6	×
.16	wo-no[]ma	AUR N 1	
.17	qo-wo []mo-ro[-qa]AUR N 1	
.18	a-ka-wo	AUR P 3	×
.19	e-re-e , po-ro-ko-re-te	AUR P 3	×
.20	a-ke-ro , qa-si-re-u	AUR P 3	×
.21	te-po-se-u , ti-nwa-si-jo ko-re-te	AUR N 1	
.22	po-ki-ro-qo	AUR N 1	
.23	au-ke-wa	AUR N 1	
.24	ti-mi-ti-ja , ko-re-te	AUR P 6	
.25	i-te-re-wa	AUR P 6	×
.26	pi-*82	AUR P 6	×
.27	e-ra-te-re-wa-o , ko-re-te	AUR P 6	
.28	a-ke-re-wa , ko-re-te	AUR P 5[]	×

y

Latus sinistrum

po-so-ri-jo []ma AUR P ×[]-jo a-to-mo AUR P 3 ×

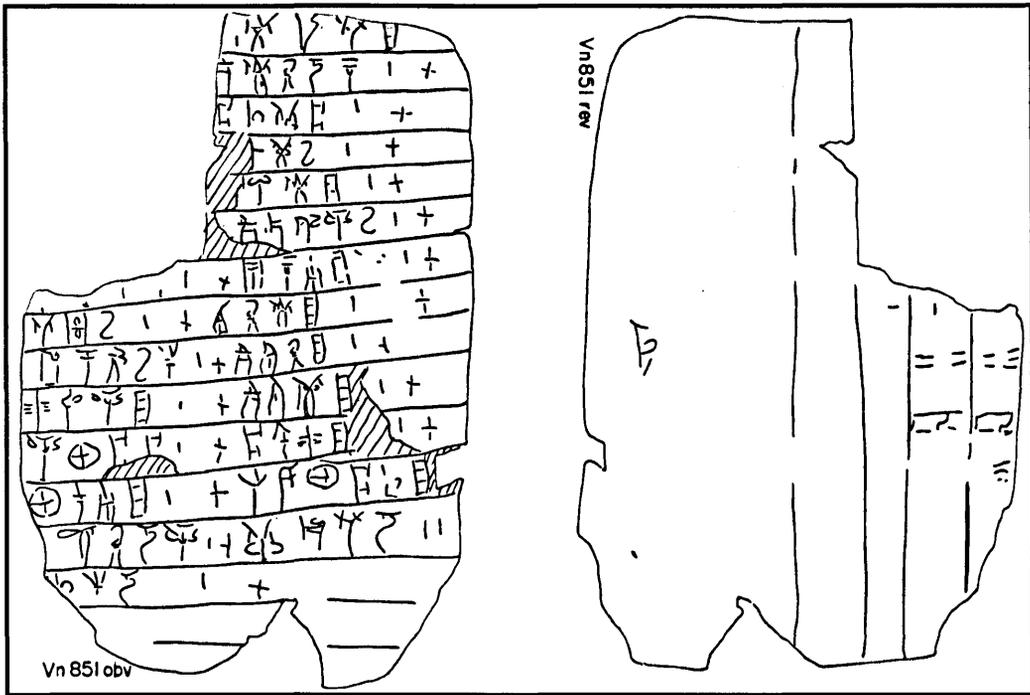
f

y

Fragmentum separatum

.1~ .14]vest.[

.2~ .15]ki[



Vn 851 (Bennett 1955: 101)

Vn 851

(H12)

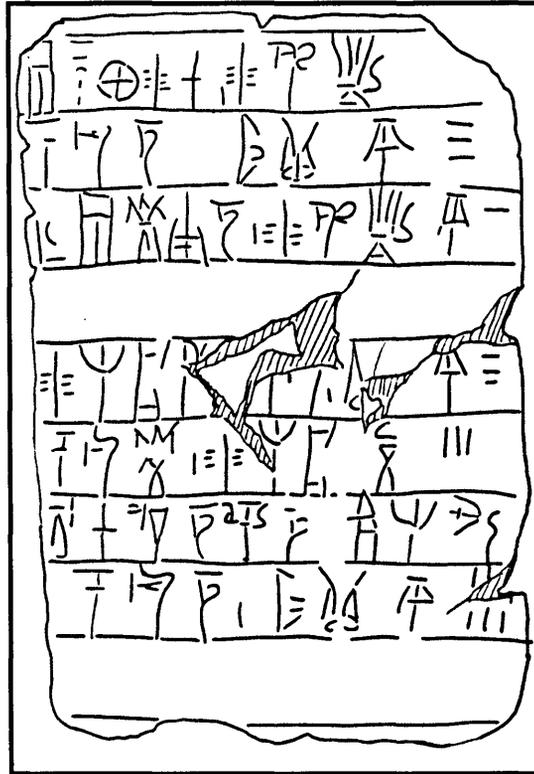
Recto

f			x
.1] ,de-mi-ni-ja	
.2]e-ke-ri-jo-na 1 x	
.3	[1 x]ta-ra-ma-ta 1 x	
.4	ka[1 x]da-je-we 1 x	
.5	e-[1 x]ru-ke-ja 1 x	
.6	tu[1 x]e-po-me-ne-we 1 x	
.7]na 1 x wa-na-si-ja-ke 1 x	
.8	sa-nu-we 1 x	wi-ri-ke-ja 1 x	
.9	o-to-wo-we-i 1 x	e-ti-ri-ja 1 x	
.10	te-do-ne-ja 1 x	e-ti-je-ja 1 x	
.11	ne-ka-ta-ta 1 x	ta-zo-te-ja[]1 x	
.12	ka-pa-si-ja 1 x	re-u-ka-ta-ra-ja[qs	
.13	te-]qi-ri-jo-ne 1 x	ma-so-ni-jo 2	
.14]ra-i-jo 1 x	
.15] vacat [] [] [] []	
y			w

Verso

f			y
.1]ko-jo[]5 o 3	
.2]4 o [qs] vacat	
.3]2	
.4	vacat		
x			w

Others



Er 312 (Bennett 1955: 34)

Er 312

(H24)

- .1 wa-na-ka-te-ro ,te-me-no [
- .2 to-so-jo [[]]pe-ma GRA 30
- .3 ra-wa-ke-si-jo ,te-me-no GRA 10
- .4 *vacat*
- .5 te-re-ta-o ṯṱ-ṣṱ pṣ-ma GRA 30
- .6 to-so-de , te-re-ta VIR 3
- .7 wo-ro-ki-jo-ne-jo , e-re-mo
- .8 to-so-jo , pe-ma GRA 7[
- .9 *vacat*

Thebes

Of 36

(303)

- .1 no-ri-wo-ki-de ku LANA 1 a-ke-ti-ra₂
- .2 po-ti-ni-ja , wo-ko-de , a-ke-ti-ra₂ ku LANA 1
- wa-na-ka[

[

The principal documents discussed in,

Chapter V

**‘Forging identities’: a case study in the
oral/written interface.**

Pylos

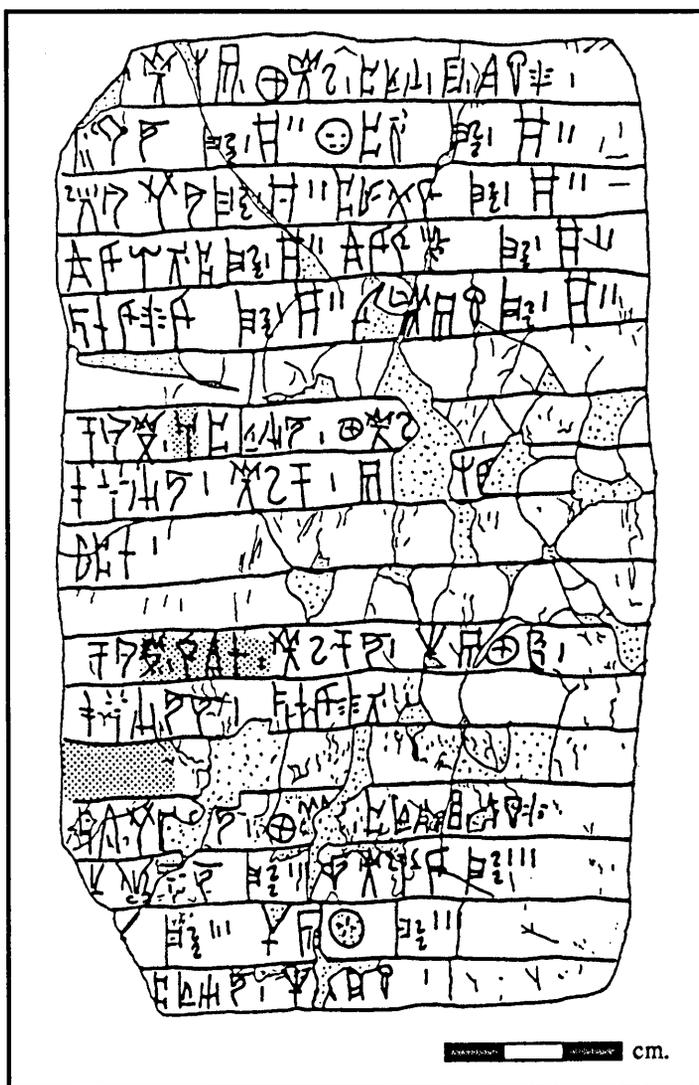
Jn Series

(Jn 310, 320, 389, 410, 415, 431, 478, 601, 605, 658, 692,
693, 706, 725, 750, 829, 832, 845, 881, 927)

Knossos

Oa 730

The Jn Series

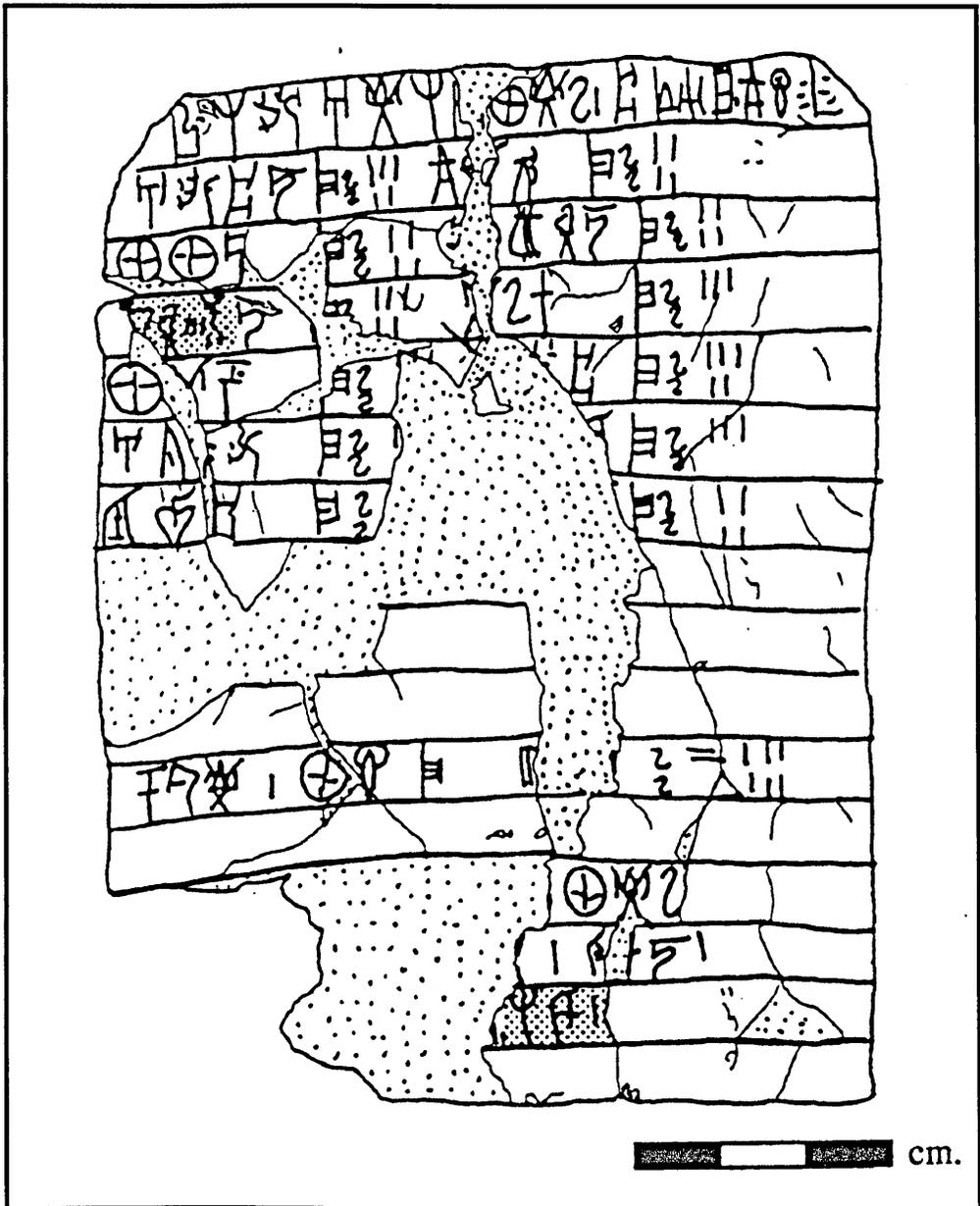


Jn 310 (Smith 1992-1993: 219, fig.15)

Jn 310

(S310 H2)

- .1 ja-ke-re-wa, ka-ke-we, ta-ra-si-ja, e-ko-te,
 .2 ti-qa-jo AESM 1 N2 qe-ta-wo AESM 1 N2
 .3 a₃-so-ni-jo AESM 1 N2 ta-mi-je-u AESM 1 N2
 .4 e-u-ru-wo-ta AESM 1 N2 e-u-do-no AESM 1 N2
 .5 po-ro-u-te-u AESM 1 N2 wi-du-wa-ko AESM 1 N2
 .6 *vacat*
 .7 to-so-de, a-ta-ra-si-jo, ka-ke-we[] *vacat*
 .8 pa-qo-si-jo 1 ke-we-to 1 wa-[]re-u[1]*vacat*
 .9 pe-ta-ro 1
 .10 *vacat*
 .11 to-so-ḍe, do-e-ro, ke-we-to-jo 1 i-wa-ka-o 1
 .12 pa-qo-si-jo-jo 1 po-ro-u-te-wo 1
 .13 [[]] *vacat*
 .14 po-ti-ni-ja-we-jo, ka-ke-we, ta-ra-si-ja, e-ko-te,
 .15 i-ma-di-jo AESM 3 tu-ke-ne-u AESM 3
 .16]AESM 3 i-wa-ka AESM 3
 .17 a-]ta-ra-si-jo, pu₂-si-ja-ko 1

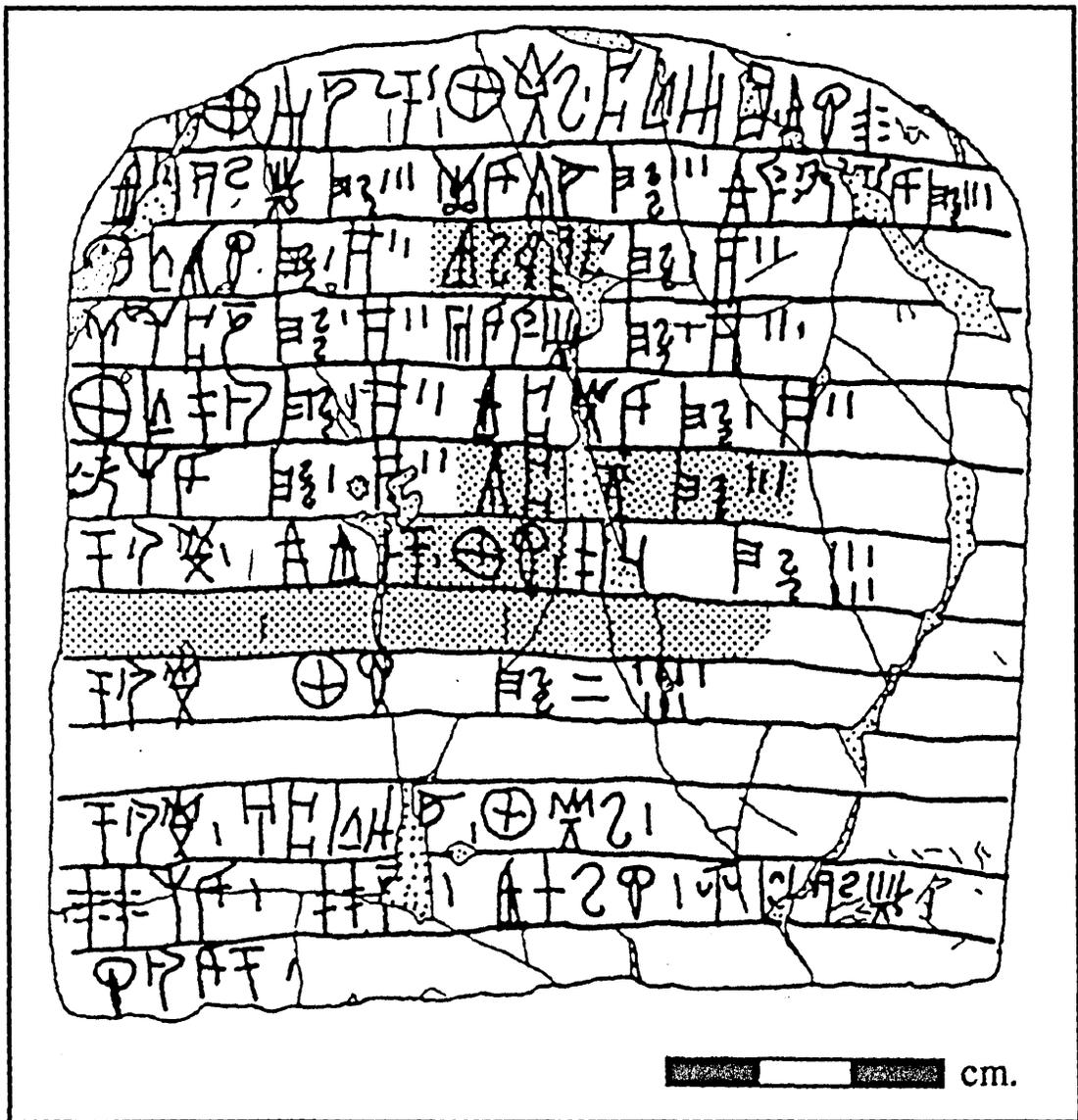


Jn 320 (Smith 1992-1993: 221, fig.16)

Jn 320

(S310 H2)

- .1 o-re-mo-a-ke-re-ų , ka-ke-we , ta-ra-si-ja , e-ko-te
- .2 a-mo-ta-jo AES M 5 e-ki-wo AES M 4
- .3 ka-ka-po AES M 4 kų-pi-ri-jo AES M 4
- .4 ki-ri-*82-jo AES M 5 de-we-ro AES M 3
- .5 ka-sa-to AES M 3 ke-[]qo-ta AES M 5
- .6 a-ti-ųa-mo AES M 1[]u AES M 3
- .7 wi-tu-ta AES M [qs] AES M 4
- .8] vacat
- .9] vacat [] vacat
- .10 vacat [] vacat
- .11 to-so-de , ka-ko AES L[]1 M 26
- .12 vacat
- .13], ka-ke-we
- .14]ta 1 do-ųo-jo 1
- .15]wo 1 ta-[]-no[1]-re-u 1

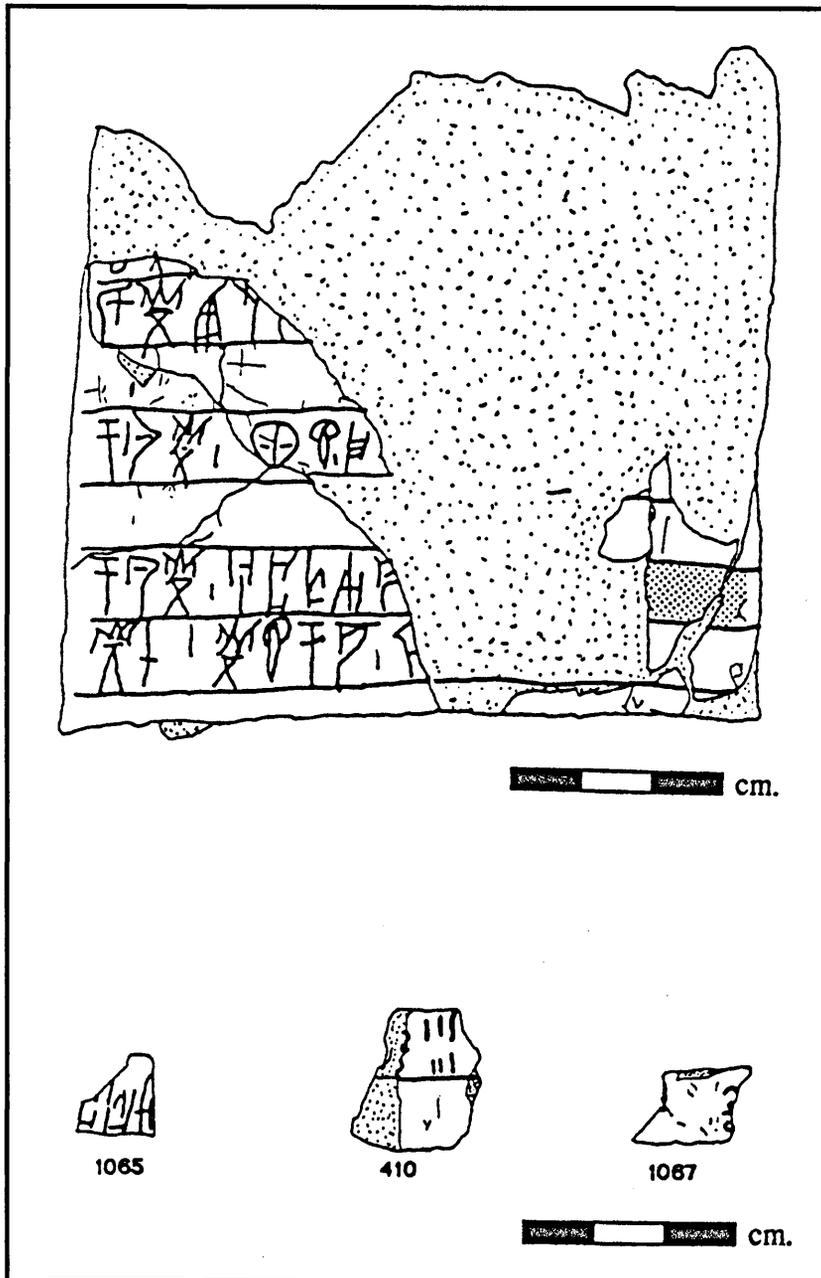


Jn 389 (Smith 1992-1993: 223, fig.17)

Jn 389

(S310 H2)

- .1 a-ka-si-jo-ne, ka-ke-we, ta-ra-si-ja, e-ko-te
- .2 pi-ra-me-no AES M 3 ma-u-ti-jo AES M 3 e-do-mo-ne-u AES M 3
- .3 ka-ra-wi-ko AES M 1 N 2 pi-we-ri-ja-ta AES M 1 N 2
- .4 sa-mu-ta-jo AES M 1 N 2 wa-u-do-no AES M 1 N 2 [[]]
- .5 ka-ra-pa-so AES M 1 N 2 pi-ta-ke-u AES M 1 N 2
- .6 mo-re-u AES M 1 N 2 ti-ta-[]wo AES M 3
- .7 to-so-de, e-pi-da-to, ka-ko, pa-ši AES M 6
- .8 vacat
- .9 to-so-de ka-ko AES M 27
- .10 vacat
- .11 to-so-de, a-ta-ra-si-jo, ka-ke-we,
- .12 te-te-re-u 1 pa-pa-jo 1 pi-ro-we-ko 1 a₂-nu-me-no 1
- .13 ko-so-u-to 1

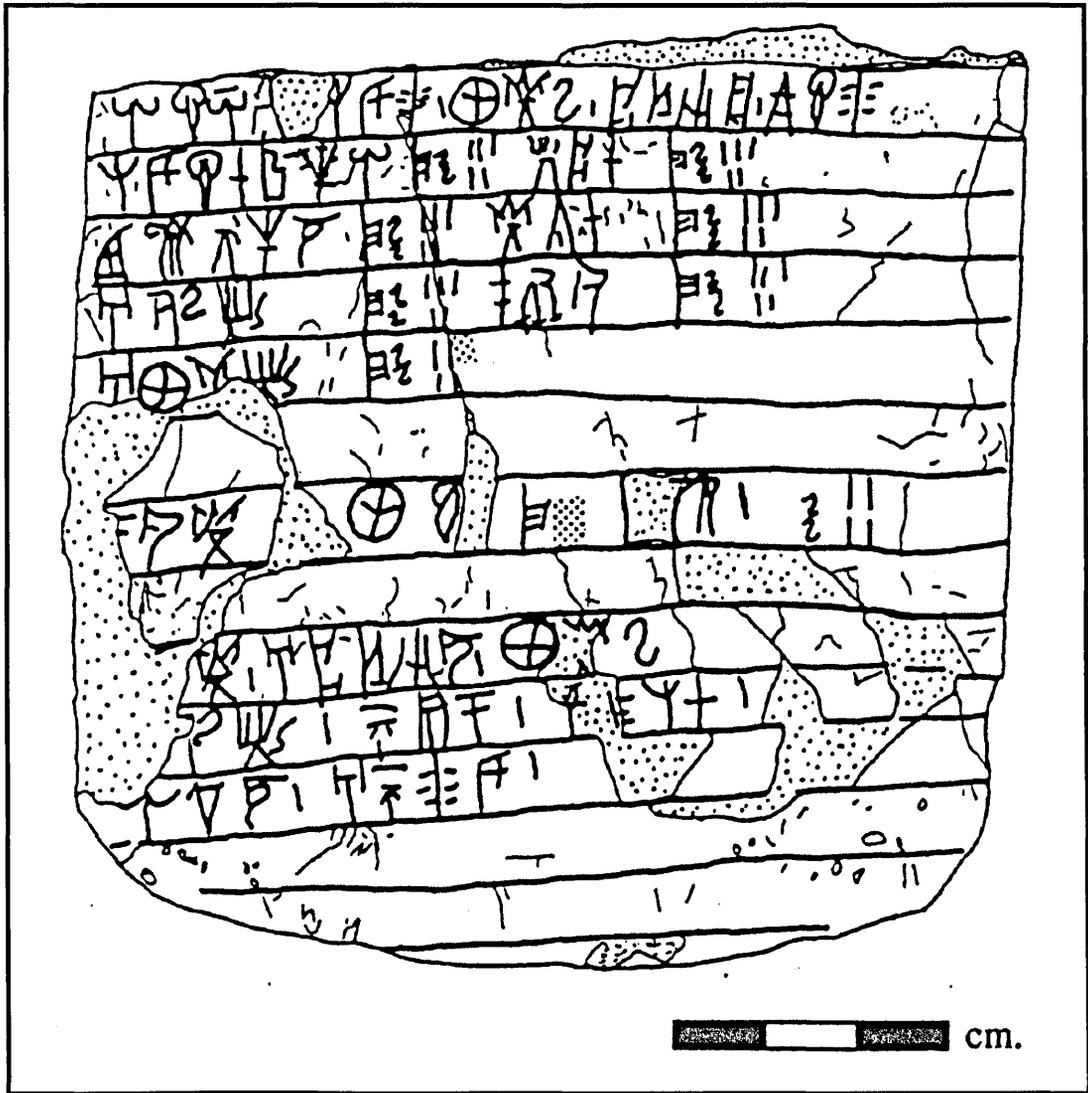


Jn 410 (Smith 1992-1993: 225, fig.18, 19)

Jn 410

(S310 H2)

- .1]ta-ra-si[-ja e-ko-te
.2 *deest*
.3 *deest*
.4 wē-
.5 u-de-wi-ni-jo[AES]M 6[
.6 *vacat* [] *vacat* [
.7 to-so-de , ka-ko , AES [L]1 M[*qs*
.8 *vacat* [] *vacat* [] *vacat*
.9 to-so-de , a-ta-ra-si-jo[ka-ke-we] [["]]
.10 ke-ro 1 de-ko-to-jo , do[-e-ro] *vacat*

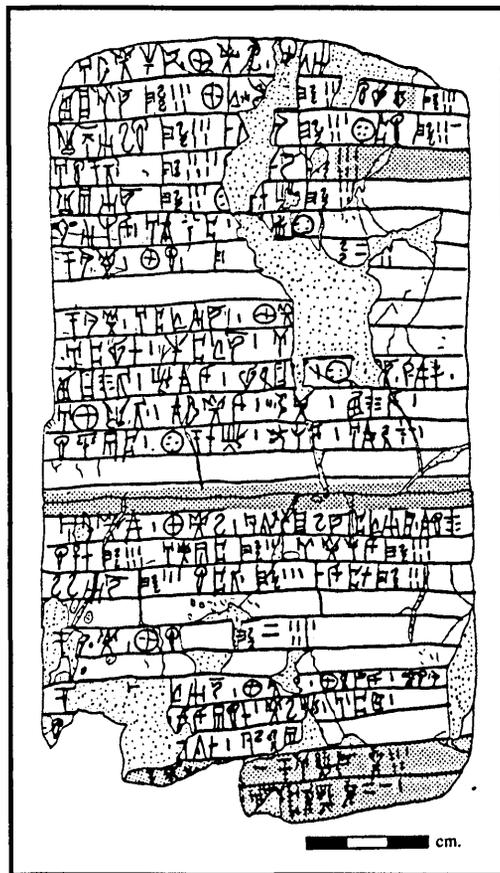


Jn 415 (Smith 1992-1993: 227, fig.20)

Jn 415

(S310 H2)

- .1 ru-ko-a₂-ḳe-re-u-te, ka-ke-we, ta-ra-si-ja, e-ko-te
- .2 re-u-ko-ro-o-pu₂-ru AES M 5 a₃-ta-ro AES M 5
- .3 wi-du-wo-i-jo AES M 5 ke-ti-ro AES M 5
- .4 a-me-no AES M 5 pa-pu-so AES M 5
- .5 a-ka-ṣa-no AES M 4
- .6 *vacat*
- .7]to-so-de [] ka-ko [] AES [[]] L 1 M 4
- .8 " *vacat* "
- .9 to-so-]de, a-ta-ra-si-jo, ka-ḳe-we [] v.
- .10]me-no 1 na-wa-to 1 [•]-te-re-ro 1 [] v.
- .11 ru-ki-jo.1 a-na-te-u 1 [] vac. [] v.
- .12 *vacat*
- .13 *vacat*

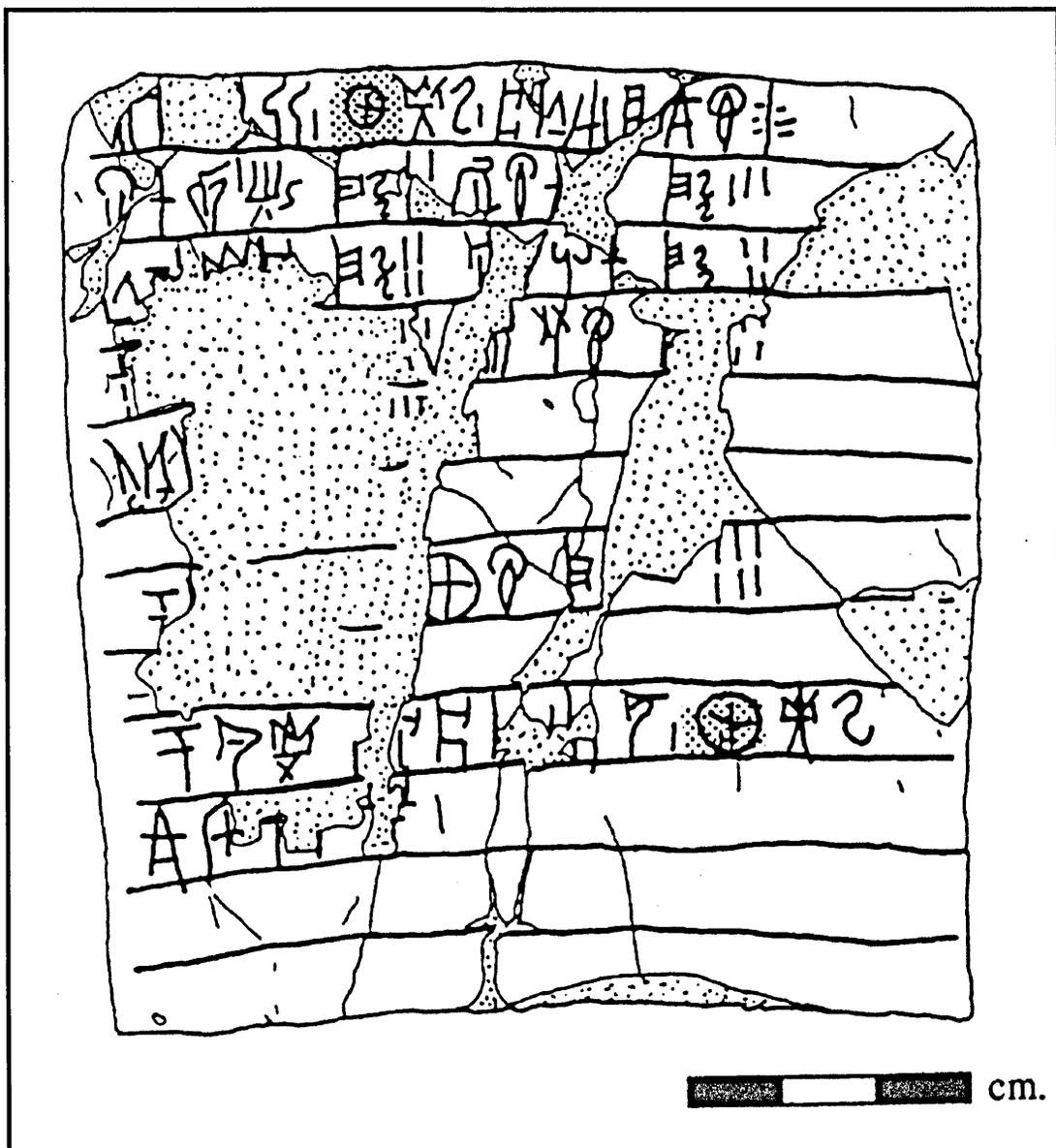


Jn 431 (Smith 1992-1993: 229, fig.21)

Jn 431

(S310 H2)

- .1 a-pe-ke-i-jo , ka-ke-we , ta-ra-si[-ja e-ko-te
.2 wi-ja-ni-jo AES M 5 ka-ra-*82[] AES M 5[] ko-tu-ro₂ AES M 5
.3 ma-na-si-we-ko AES M 5 da-ma-so AES M 5 qe-ta-ko AES M 4
.4 a-ko-to-wo AES M 7 u[] i-jo AES M 6 [[]]
.5 ma-wa-si-jo AES M 5 qe-to-ro-no AES M 7
.6 qa-si-re-u , a-pi-qo-ta 1 [] i-*65-qe 1
.7 to-so-de , ka-ko , AES [L 1] M 24
.8 vacat [] vacat
.9 to-so-de , a-ta-ra-si-jo , ka-ke[-we] vacat
.10 a-ta-tu-ro 1 i-ta-ra-jo 1 sa[] 1] vacat
.11 wi-ja-te-wo 1 no-e-u 1 tu-ri-ja-jo 1 qe-ta-ko-jo , do-e-ro 1
.12 a-ka-ma-wo 1 e-pe-ke-u 1 *82-de 1 pu-te-u 1
.13 ko-ne-wa-ta 1 qe-to-ro-no 1 mo-re-u 1 a-e-ri-qo 1
.14 vacat
.15 vacat
.16 a-pe-ke-e , ka-ke-we , po-ti-ni-ja-we-jo , ta-ra-si-ja , e-ko-te
.17 ko-za-ro AES M 6 a-ke-wa-ta AES M 3 sa-ke-re-u AES M 6
.18 we-we-si-jo AES M 3 ko-ta-wo AES M 3 da-u-ta-ro AES M 6
.19 vacat
.20 to-so-de , ka-ko AES M 27 []
.21 vacat []
.22 to[-so-de ja[-ta-]ra-si-jo , ka-ke-we , ka-ri-se-u 1 du-ko-so[] 1
.23 ko[] 1 e-u-wa-ko-ro 1 ke-we-no 1 a-ta-o 1 []
.24 [] wa-ti-ro 1 me-ri-wa-[] 1 []
.25 [] i-je-re[-wo] VIR 10 to-sa-no-jo VIR 5
.26 [] VIR] 1 a-mu-ta-wo-no VIR 31

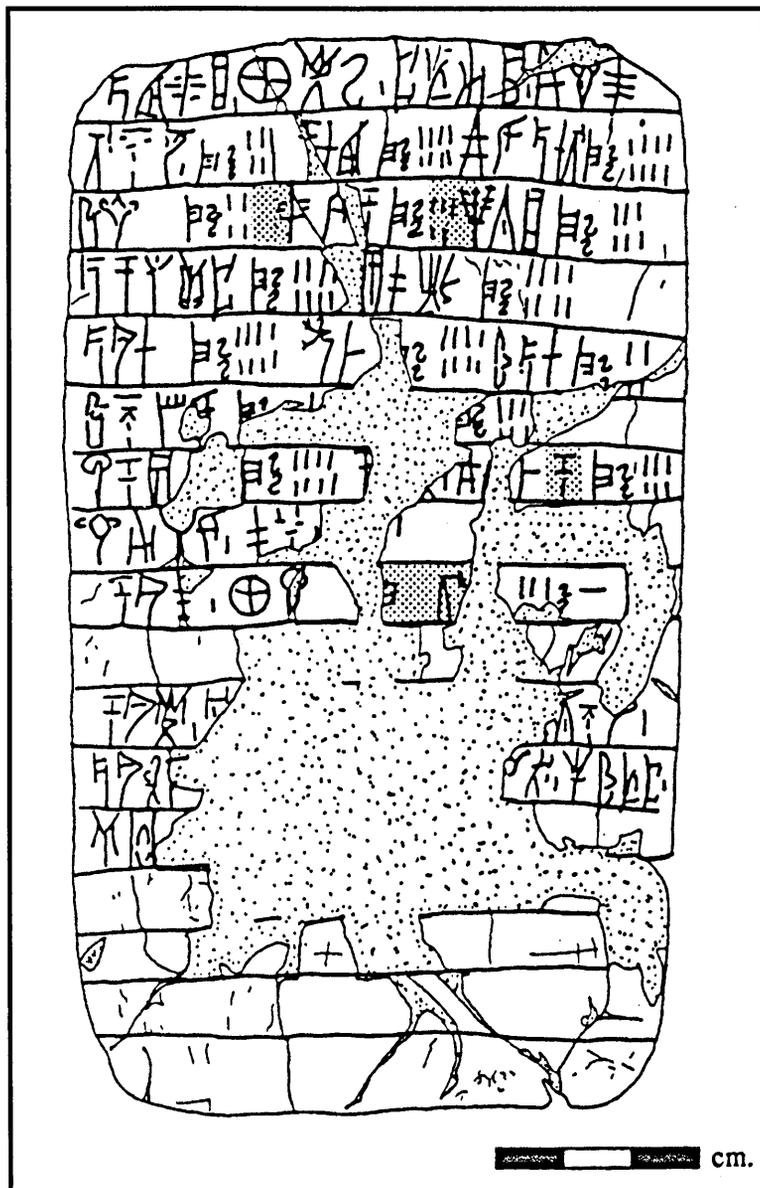


Jn 478 (Smith 1992-1993: 231, fig.22)

Jn 478

(S310 H2)

- .1 wi-ja-we-ra₂ , ka-ke-we , ta-ra-si-ja , e-ko-te
 .2 ko-ro-tu-no AES M 4 pu-ko-ro[] AES M 3 [
 .3 ra-wo-ke-ta AES M 4 a-[]-a₂-ro AES M 4 [
 .4 di[AES M]4[]wa-ni-ko AES[M] 4
 .5 ma-[AES M]2[] vacat [] vac.
 .6 v.[] vacat [] [] vac.
 .7 to-şo[-de]ka-ko AES M 26
 .8 v.[] vacat [
 .9 to-so-de , a-ta-ra-si-jo , ka-ke-we
 .10 e-u-ro-fo-fo l
 .11 vacat
 .12 vacat

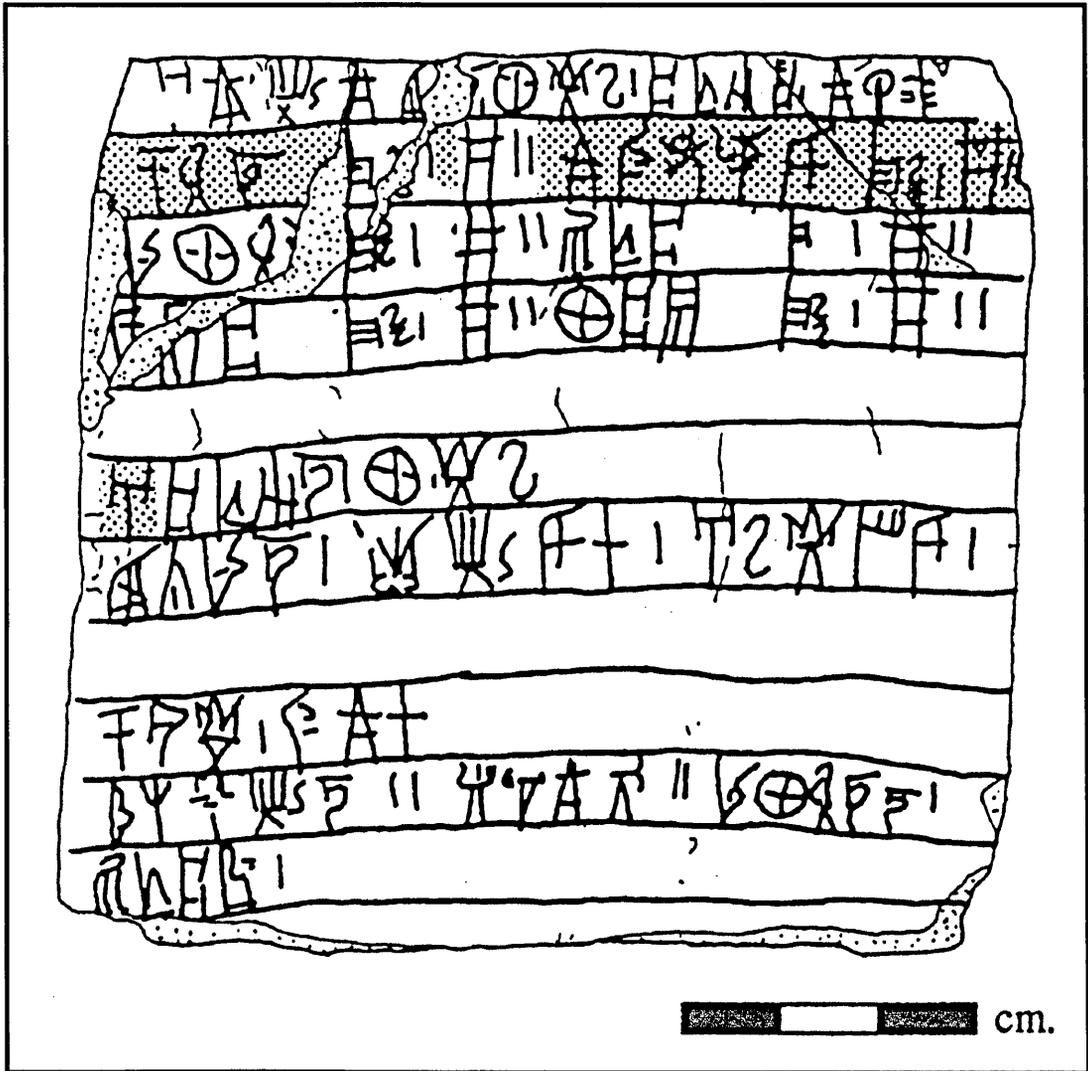


Jn 601 (Smith 1992-1993: 233, fig.23)

Jn 601

(S310 H2)

- .1 po-wi-te-ja , ka-ke-we , ta-ra-si-ja , e-ko-te
 .2 wo-di-jo , AES M 6 to-ro-wi AES M 8 e-u-po-ro-wo AES M 8
 .3 o-qa AES M 4 te-u-to AES M 5 pu₂-ti-ja AES M 6
 .4 po-to-re-ma-ta AES M 8 []wa-pa-no AES M 8
 .5 po-so-ro AES M 8 mo-da AES M 8 pe-po-ro AES M 4
 .6 o-na-se-u AES M 12 vest.[]AES M 8[]vacat
 .7 ko-to-wa-[]AES M 8 to[-so-]de , e-pi-da-to AES M 7
 .8 qa-si-re-u , pa-qo-si[-jo]1
 .9 to-so-pa , ka-ko []AES L 3 M 1.4[
 .10 vacat []vac. []vac.[]vacat
 .11 to-so-de ; a[-ta-ra-]si-jo[ka-ke-we]ti-na-jo 1
 .12 po-so-ri-jo[1]ne-wo 1 i-pe-ra-ta 1
 .13 sa-nu-[]1[]vacat []vacat
 .14 vacat
 .15 vacat
 .16 vacat

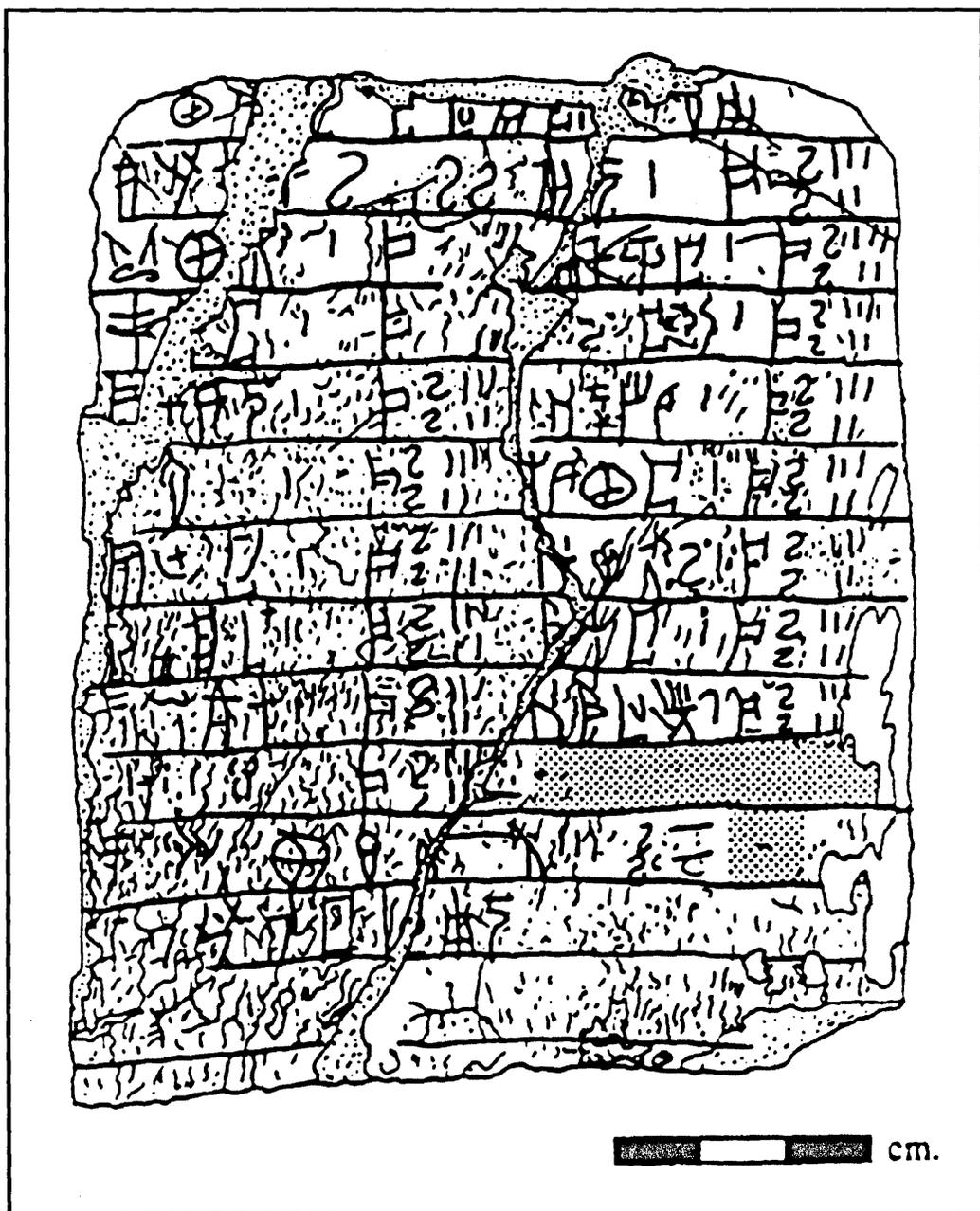


Jn 605 (Smith 1992-1993: 235, fig.24)

Jn 605

(S310 H2)

- .1 a-pi-no-e-wi[-jo], ka-ke-we, ta-ra-si-ja, e-ko-te
- .2 to-ri-jo AES M 1 N 2 e-do-mo-ne-u AES M 1 N 2
- .3 mi-ka-ri-jo[] AES M 1 N 2 pu-ra-ta AES M 1 N 2
- .4 u-wa-ta AES M 1 N 2 ka-ta-wa AES M 1 N 2
- .5 *vacat*
- .6 a-ta-ra-si-jo, ka-ke-we
- .7 wi-ti-mi-jo 1 ma-no-u-ro 1 a-we-ke-se-u 1
- .8 *vacat*
- .9 to-so-de, do-e-ro
- .10 pe-re-qo-ho-jo 2 a₃-ki-e-wo 2 mi-ka-ri-jo-jo 1
- .11 pu-ra-ta-o 1

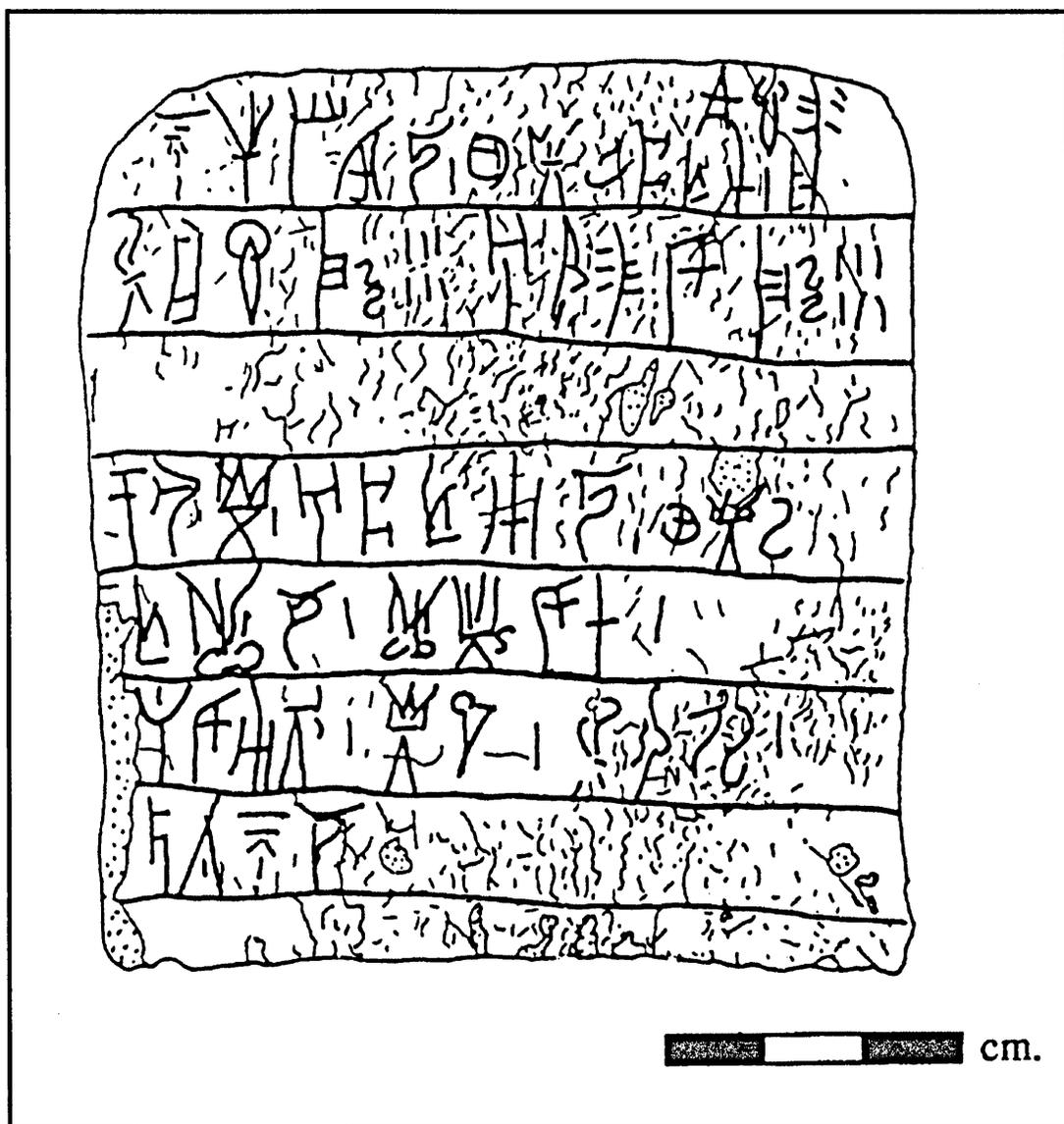


Jn 658 (Smith 1992-1993: 236, fig.25)

Jn 658

(S658 H21)

- .1 ka-ke-we , ta-ra-si-ja , ę-ko-si
 .2 e-ni-pa-te-we , we-we-si-jo 1 AES M 5
 .3 ma-ka-wo 1 AES M 5 pi-ro-ne-ta 1 AES M 5
 .4 pa-qa-ta 1 AES M 5 au-ta-mo 1 AES M 5
 .5 po-ro-u-jo 1 AES M 5 o-na-se-u 1 AES M 5
 .6 po-]ro-ko 1 AES M 5 re-u-ka-ta 1 AES M 5
 .7 wa-ka-ta 1 AES M 5 o-tu-wo-we 1 AES M 5
 .8 wo-wi-ja-ta 1 AES M 5 pe-re-ta 1 AES M 5
 .9 po-ru-e-ro 1 AES M 5 o-pe-ra-no 1 AES M 5
 .10 a-tu-ko 1 AES M 5 [[]]
 .11 to-so-de , ka-ko L 2 M 20 [[]]
 .12 to-so-de , a-ta-ra-si-jo
 .13 vacat

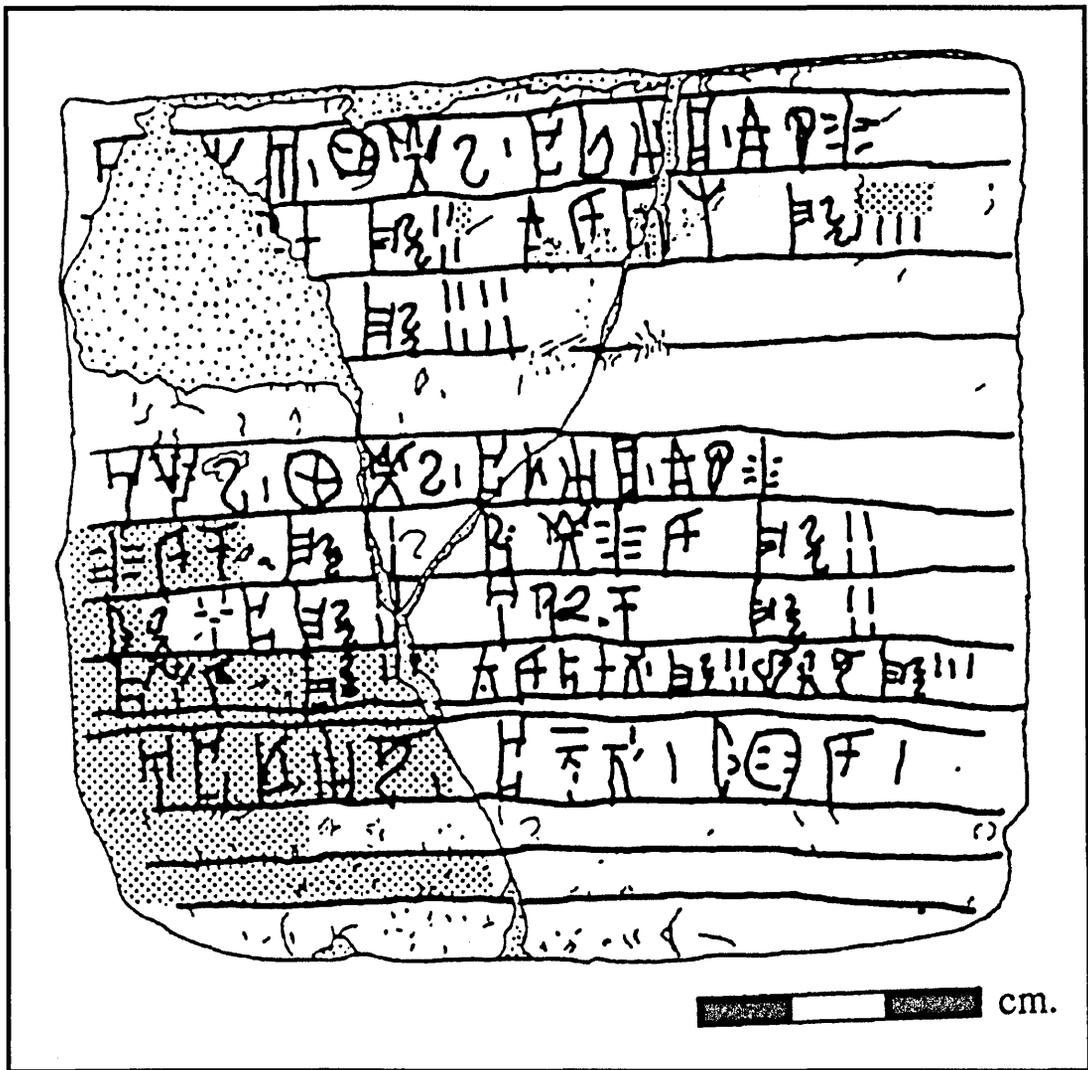


Jn 692 (Smith 1992-1993: 239, fig.26)

Jn 692

(S310 H2)

- .a e-ko-te
 .1 na-i-se-wi-jo , ka-ke-we , ta-ra-si-ja
 .2 ri-ja-ko AES M 6 a-pe-te-u AES M 6
 .3 *vacat*
 .4 to-so-de , a-ta-ra-si-jo , ka-ke-we
 .5 ra-ma-jo 1 ma-no-u-ro 1
 .6 re-u-si-wo 1 ke-ki 1 ri-ku-we 1
 .7 po-ti-na-jo 1

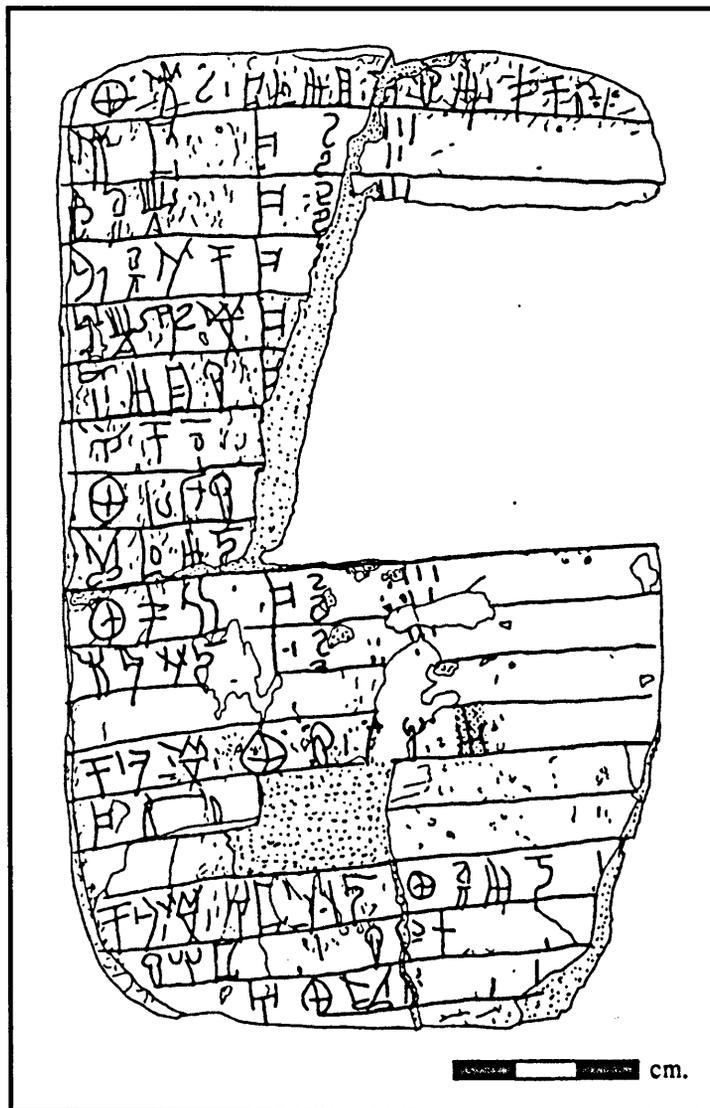


Jn 693 (Smith 1992-1993: 241, fig.27)

Jn 693

(S310 H2)

- .1 a[-ke-]re-wa , ka-ke-we , ta-ra-si-ja , e-ko-te
 .2]-ro AES M 4 e-u-wa-re AES M 4
 .3] AES M 8
 .4 *vacat*
 .5 a-pu₂-we , ka-ke-we , ta-ra-si-ja , e-ko-te
 .6 te-ū-to AES M 4 o-ke-te-u AES M 4
 .7 pe-ri-qo-ta AES M 4 a-me-to AES M 4
 .8 ta-re-u AES M 3 e-u-po-ro-wo AES M 4 tu-ri-jo AES M 3
 .9 *angustum*
 .10 a-ta-ra-si-jo , ta-na-wo 1 pe-qe-u 1
 .11 *vacat*
 .12 *vacat*



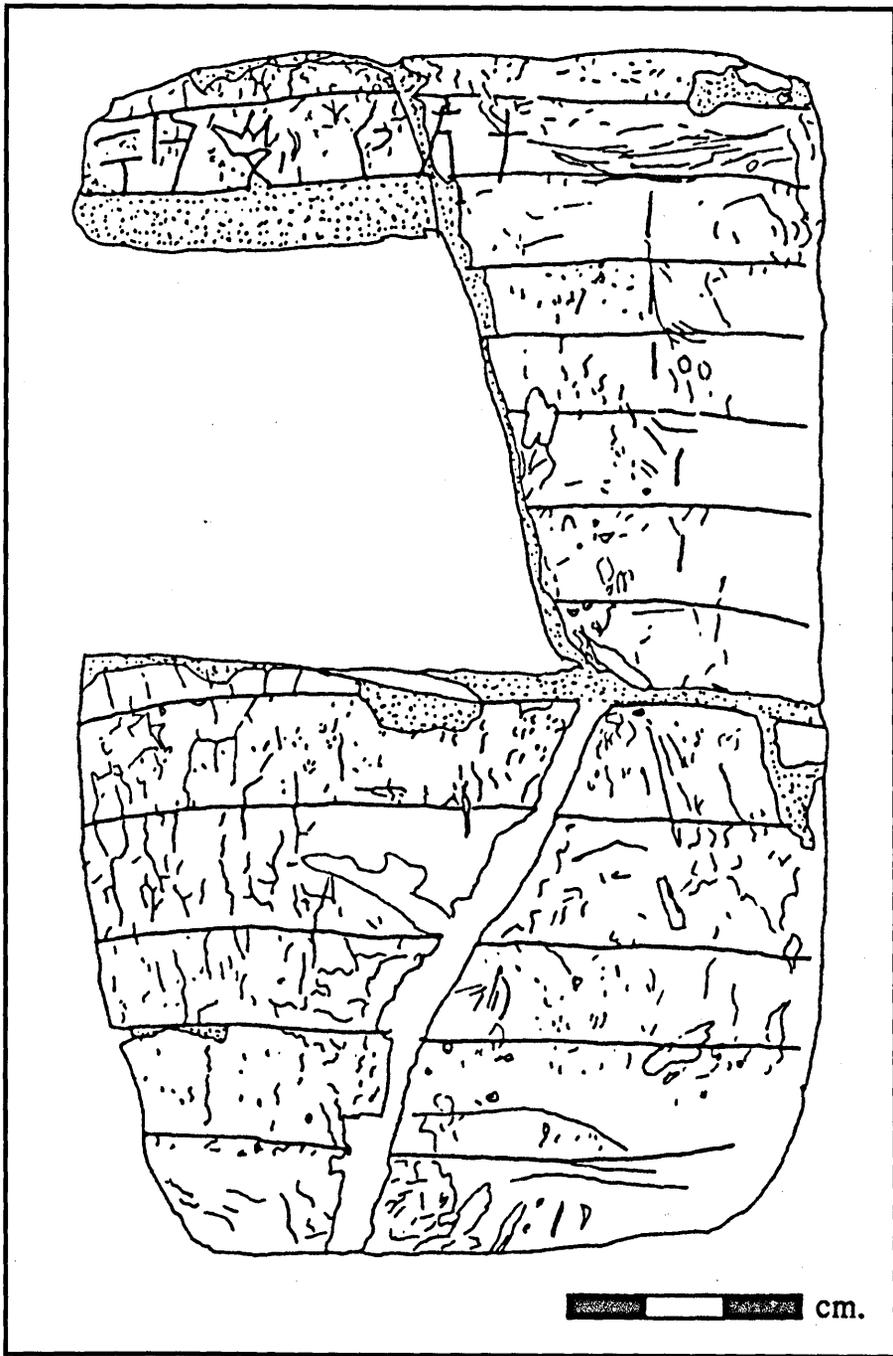
Jn 706 recto (Smith 1992-1993: 243, fig.28)

Jn 706

(S658 H21)

Recto

- | | |
|-----|---|
| .1 | ka-ke-we , ta-ra-si-ja , e-ko-si , pa-to-wo-te |
| .2 | ke-ta AES M 5 |
| .3 | pe-ri-no AES M 3 |
| .4 | ku-ri-sa-to AES M[<i>qs</i> |
| .5 | a-no-me-de AES[<i>qs</i> |
| .6 | qi-si-ja-ko AES[<i>qs</i> |
| .7 | mu-to-na AES[<i>qs</i> |
| .8 | ka-ra-u-ko AES[<i>qs</i> |
| .9 | ma-ra-si-jo [AES <i>qs</i> |
| .10 | ka-pa-ra ₂ AES M 5 |
| .11 | a ₃ -so-ni-jo AES M 5 |
| .12 | <i>vacat</i> |
| .13 | to-so-de , ka-ko , e-ko-si |
| .14 | AES L 1 [] <i>vacat</i> |
| .15 | <i>vacat</i> [] <i>vacat</i> |
| .16 | to-so-de , a-ta-ra-si-jo , ka-ri-si-jo 1[|
| .17 | ko-a ₂ -ta 1 ko-do-ro 1[|
| .18 | a-ka-ma-wo 1 [|



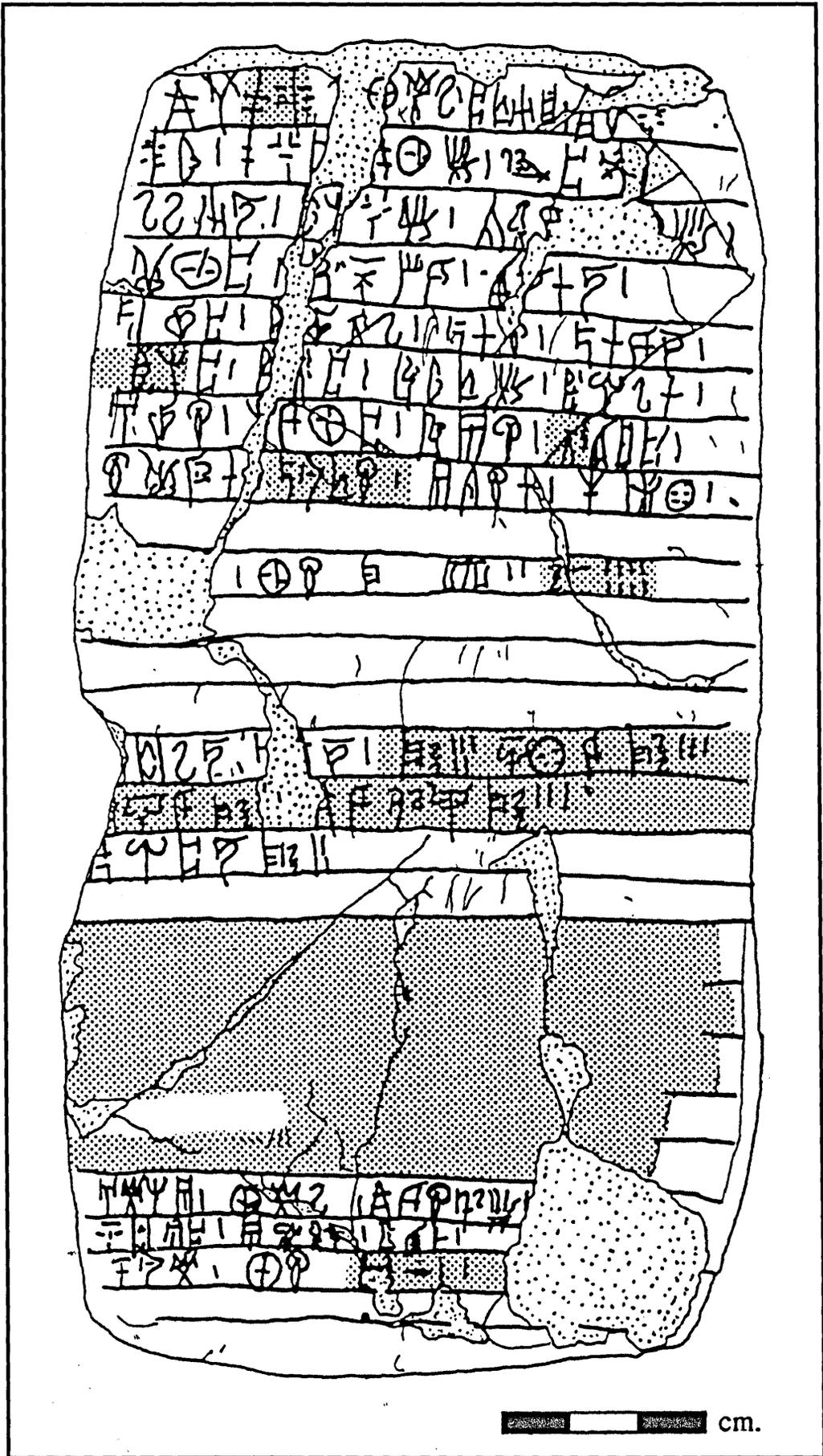
Jn 706 verso (Smith 1992-1993: 244, fig.29)

Jn 706

(S658 H21)

Verso

.1	to-so-de; do-e-ro	
.2	"]	1
.3]]	1
.4]]	1
.5]]	1
.6]]	1
.7	<i>vacat</i>	
.8	<i>vacat</i>	
.9	<i>vacat</i>	
.10	<i>vacat</i>	
.11	<i>vacat</i>	

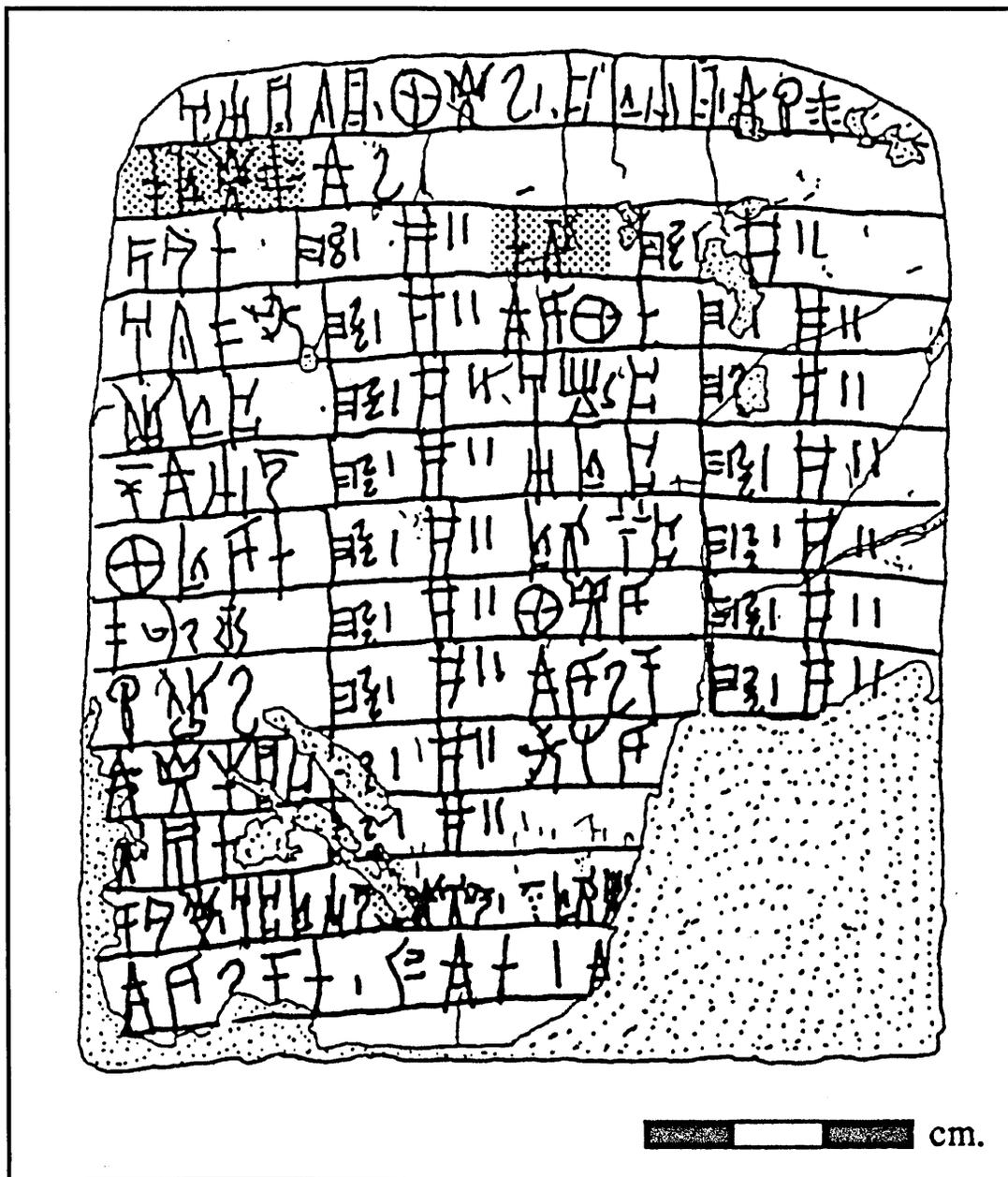


Jn 725 (Smith 1992-1993: 247, fig.30)

- .1 e-ni-pa-te-wę ka-ke-we , ta-ra-si-ja , e-ko-te
 .2 te-pe 1 pa-qo-ta[1]e-ka-no 1 au-ta-mo 1
 .3 we-we-si-jo 1 pe-re-qo-no 1 ti-ri-[•] 1[]kq-no 1
 .4 ma-ka-ta 1 o-na-se-u 1 wi-jo-ro-jo 1
 .5 wa-tu-ta 1 o-tu-wo-we 1 po-ro-ko 1 po-ro-u-jo 1
 .6 pe-re-ta 1 o-wo-ta 1 o-pe-ra-no 1 o-ru-we-ro 1
 .7 a-tu-ko 1 re-u-ka-ta 1 o-wa-ko 1 wo-wi-ja-ta 1
 .8 ko-ma-do-ro 1 po-so-ra-ko 1 wa-ti-ko-ro 1 i-*65-qe 1
 .9 vacat
 .10 to-so-de], ka-ko AES L 2 M 18
 .11 vacat
 .12 vacat
 .13 vacat
 .14]-nu-we-jo , a-pi-jo 1 AES M 5 ne-qe-u AES M 3
 .15]ne-u AES M 1[]e-u-me-ne AES M 3
 .16]e-ru-ta-jo AES M 4
 .17 vacat
 .18 vacat
 .19 vacat
 .20 vacat
 .21 vacat
 .22 vacat
 .23 a-ke-re-wa , ka-ke-we , e-u-ko-me-no 1[] v.
 .24 di-nu-wa-ta 1 wa-du-ri-jo 1 o-wi-da 1 []
 .25 to-so-de , ka-ko AES M 12 []
 .26 vacat []

Recto originalis

- .18 na-i-se-wi-jo , ka-ke-u , ta-ra-si-ja , e-ko-te
 .19 ra-ma-jo 1 ri-ja-ko 1 ma-no-u-ro 1 re-u-si-wo 1
 .20 ke-ki 1 a-pe-te-u 1 ri-ku-we 1 po-ti-ṇa-jo 1
 .21 to-so-de , ka-ko AES M 12
 .22 to-so-de , ka-ko AES M 30 [] vacat

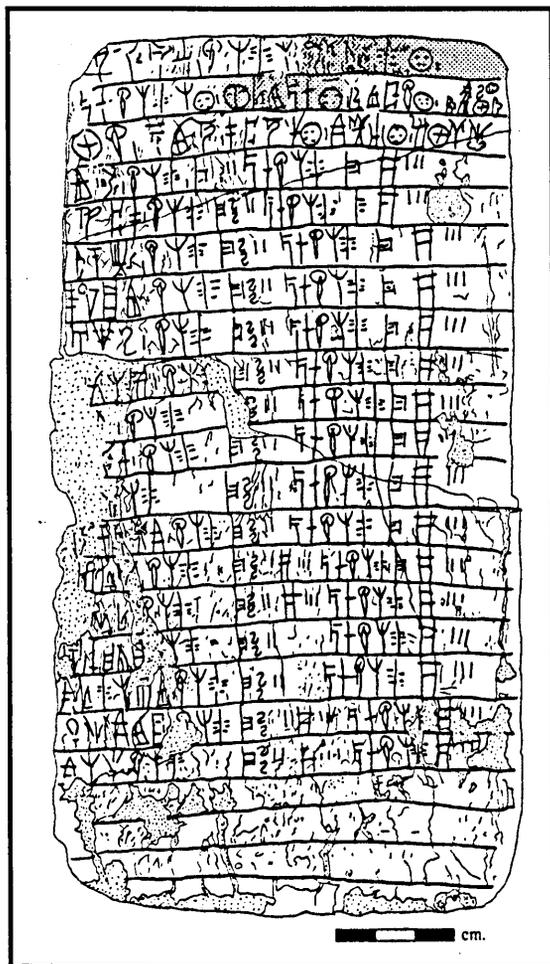


Jn 750 (Smith 1992-1993: 249, fig.31)

Jn 750

(S310 H2)

- .1 a-si-ja-ti-ja , ka-ke-we , ta-ra-si-ja , e-ko-te
 .2 pa-ra-ke-te-e-we ,
 .3 po-so-ro AES M 1 N 2 ro-wo AES M 1 N 2
 .4 a-ti-pa-mo AES M 1 N 2 e-u-ka-ro AES M 1 N 2
 .5 ma-ra-ta AES M 1 N 2 a-no-ta AES M 1 N 2
 .6 na-e-si-jo AES M 1 N 2 si-ra-ta AES M 1 N 2
 .7 ka-ra-u-ro AES M 1 N 2 ra-wo-qo-ta AES M 1 N 2
 .8 pa-ku-ro₂ AES M 1 N 2 ka-ke-u AES M 1 N 2
 .9 ko-ma-we AES M 1 N 2 e-u-we-to AES M 1 N 2[
 .10 e-ke-i-ja-ta AES M 1 N 2 mo-re-u [AES *qs*
 .11 wo-wa-ro AES M 1 N 2 []
 .12 to-so-de , a-ta-ra-si-jo , du-wo-jo 1 di-ra-wo-no [1
 .13 e-u-we-to-ro , do-e-ro 1 e []

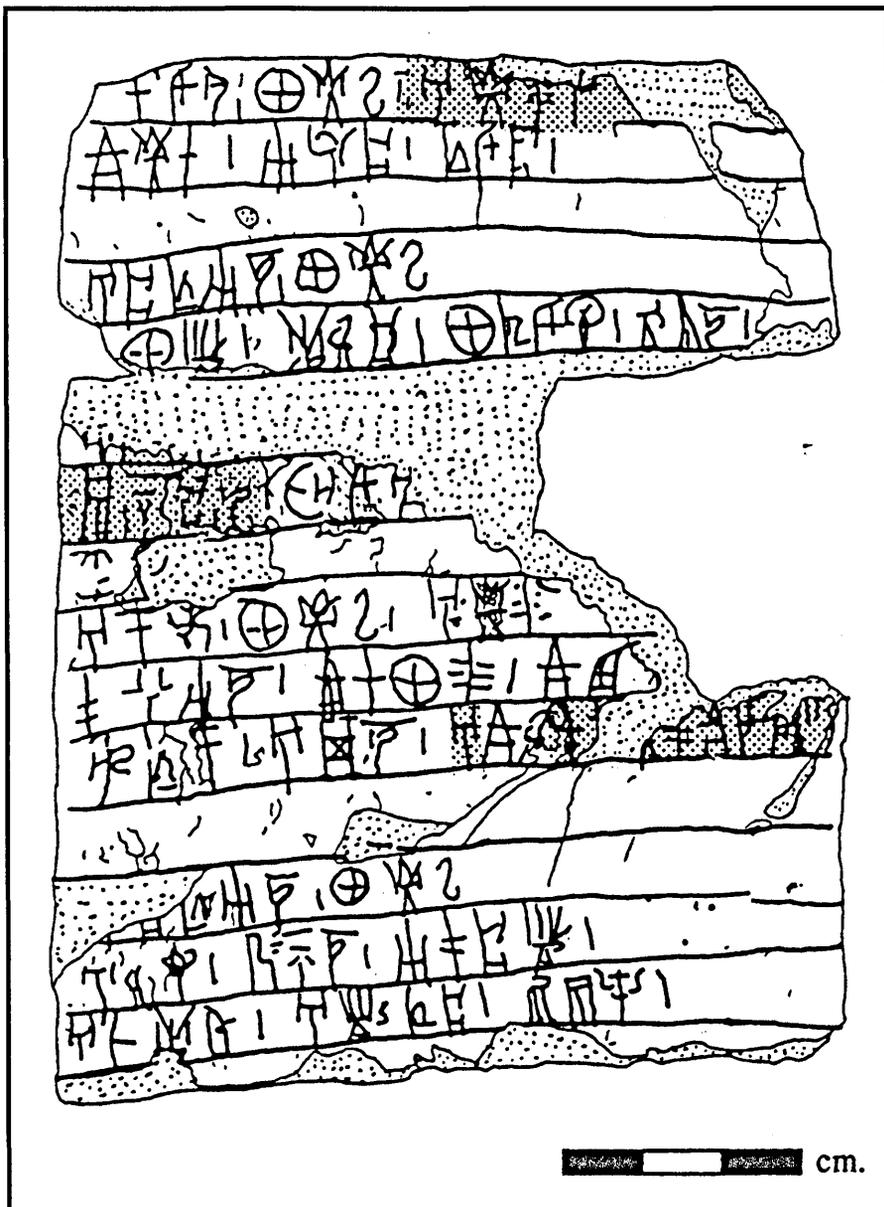


Jn 829 (Smith 1992-1993: 251, fig.32)

Jn 829

(S310 H2)

- .1 jo-do-so-si , ko-re-te-re , du-ma-te-qe ,
 .a -e-we-qe
- .2 po-ro-ko-re-te-re-qe , ka-ra-wi-po-ro-qe , o-pi-su-ko-qe , o-pi-ka-pe-
 .3 ka-ko , na-wi-jo , pa-ta-jo-i-qe , e-ke-si-qe , a₃-ka-sa-ma
 .4 pi-*82 , ko-re-te , AES M 2 po-ro-ko-re-te AES N 3
 .5 me-ta-pa , ko-re-te AES M 2 po-ro-ko-re-te AES N 3[] *vacat*
 .6 pe-to-no , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .7 pa-ki-ja-pi , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .8 a-pu₂-we , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .9 a-ke-re-wa , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .10 ro-u-so , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .11 ka-ra-do-ro , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .12 ri-ljq , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .13 ti-mi-to-a-ke-e , ko-re-te AES M 2 po-ro-ko-re-te AES N 3
 .14 ra-]wa-ra-ta₂ , ko-re-te AES M 2 N 3 po-ro-ko-re-te AES N 3
 .15 sa-]ma-ra , ko-re-te AES M 3 N 3 po-ro-ko-re-te N 3
 .16 a-si-ja-ti-ja , ko-re-te AES M 2 po-ro-ko-re-te N 3
 .17 e-ra-te-re-wa-pi , ko-re-te AES M 2 po-ro-ko-re-te N 3
 .18 za-ma-e-wi-ja , ko-re-te AES M 3 N 3 po-ro-ko-re-te N 3
 .19 e-re-i , ko-re-te AES M 3 N 3 po-ro-ko-re-te N 3
 .20 *vacat*
 .21 *vacat*
 .22 *vacat*

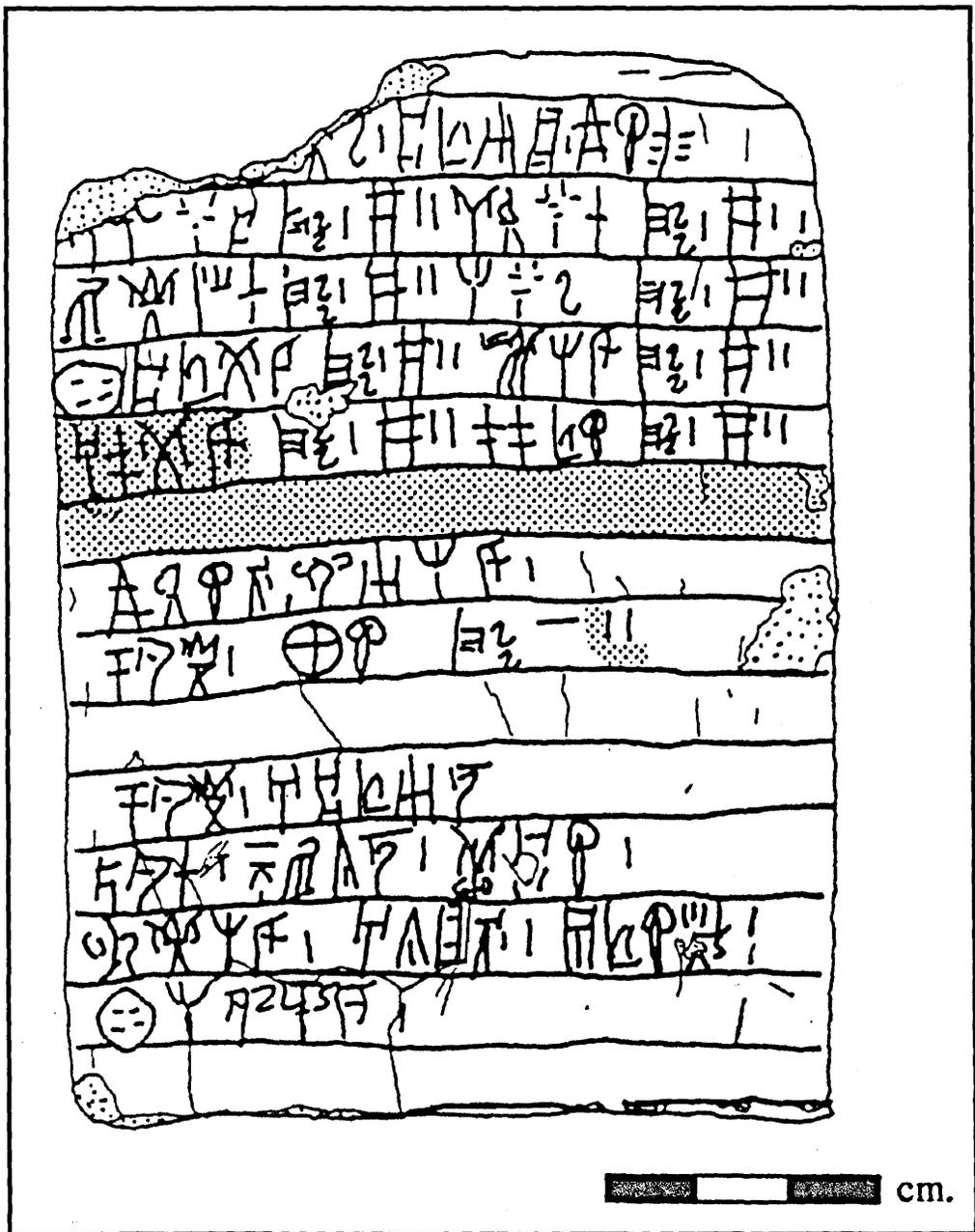


Jn 832 (Smith 1992-1993: 253, fig.33)

Jn 832

(S310 H2)

- .1 ro-u-so , ka-ke-we , a-ke-te-re
 .2 e-ke-ro l si-mu-ta l ra-u-ta l
 .3 vacat
 .4 a-ta-ra-si-jo , ka-ke-we
 .5]-ka-no l ma-ri-ta l ka-ra-u-ko l wo-ti-jo l
 .6 vest.[
 .7 wa-na-ta-jo l ka-u-so[l
 .8 ka-[] vacat [
 .9 a-to-mo , ka-ke-we , a-ke-te [
 .10 pa-qo-si-jo l pi-ro-ka-te l e-wi-ṭe[-u l
 .a -qe
 .11 me-ra-to l a-*64-jo , a-e-ri- l pa-ṛa-ke-te-e-u AES M 3
 .12 vacat
 .13 a-ta-ra-si-jo , ka-ke-we
 .14 a-ri-qa l o-na-jo l si-pa-ta-no l
 .15 a-da-ma-o l a-no-ra-ta l pu-wa-ne l

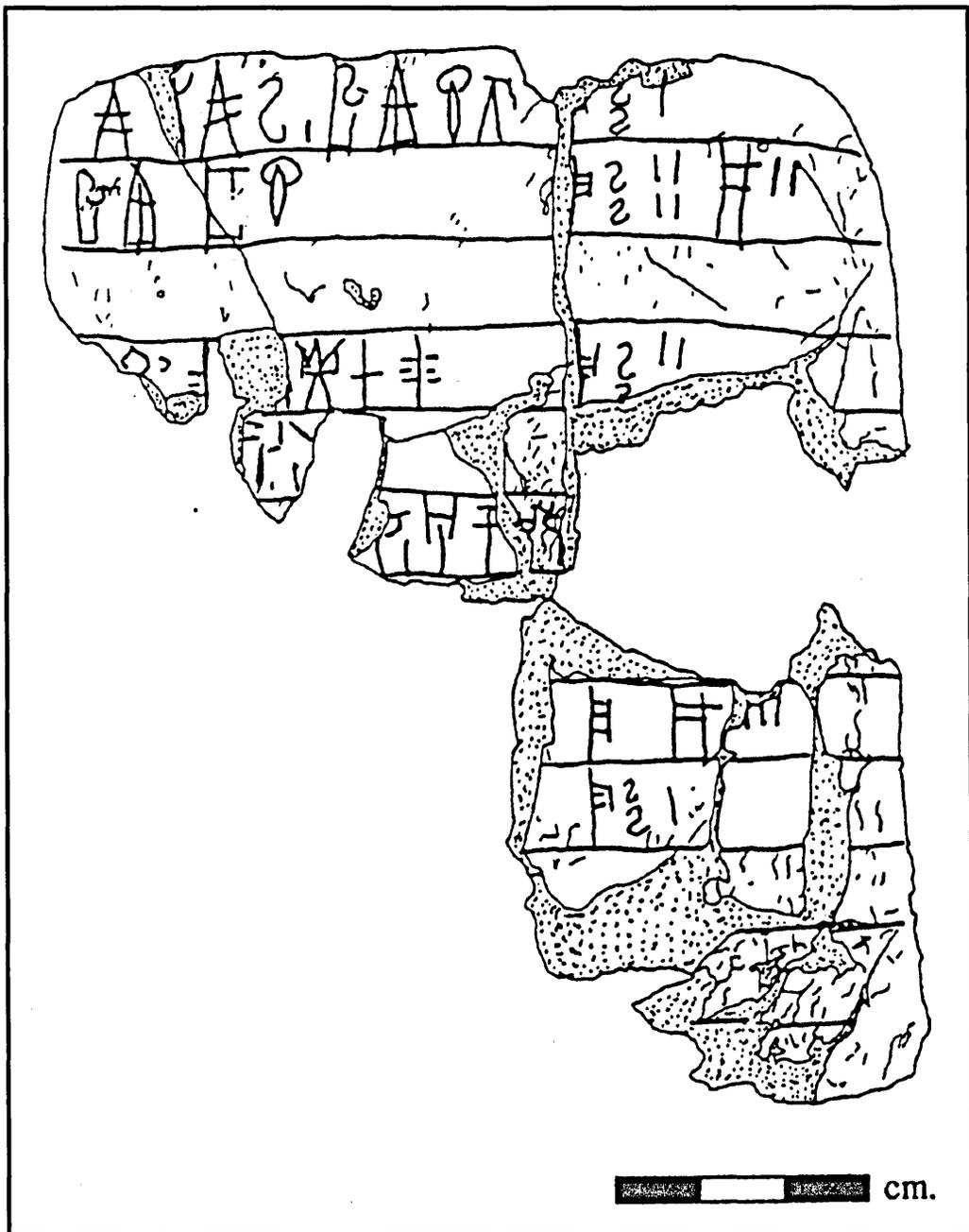


Jn 845 (Smith 1992-1993: 255, fig.34)

Jn 845

(S310 H2)

- .1]ka-ke-we , ta-ra-si-ja , e-ko-te
 .2 pø-ru-qo-ta AES M 1 N 2 sa-ri-qo-ro AES M 1 N 2
 .3 pu-ke-se-ro AES M 1 N 2 re-qo-we AES M 1 N 2
 .4 qe-ta-ra-je-u AES M 1 N 2 du-re-u AES M 1 N 2
 .5 a-pa-je-u AES M 1 N 2 pa-pa-ra-ko AES M 1 N 2
 .6 vacat
 .7 e-ri-ko-wo , qa-si-re-u 1
 .8 to-so-de ; ka-ko AES M 12
 .9 vacat
 .10 to-so-de , a-ta-ra-si-jo
 .11 po-so-ro 1 na-pu-ti-jo 1 ma-ta-ko 1
 .12 ku-ke-re-u 1 a-ti-ja-wo 1 wa-ra-ko-no 1
 .13 qe-re-me-ne-u 1

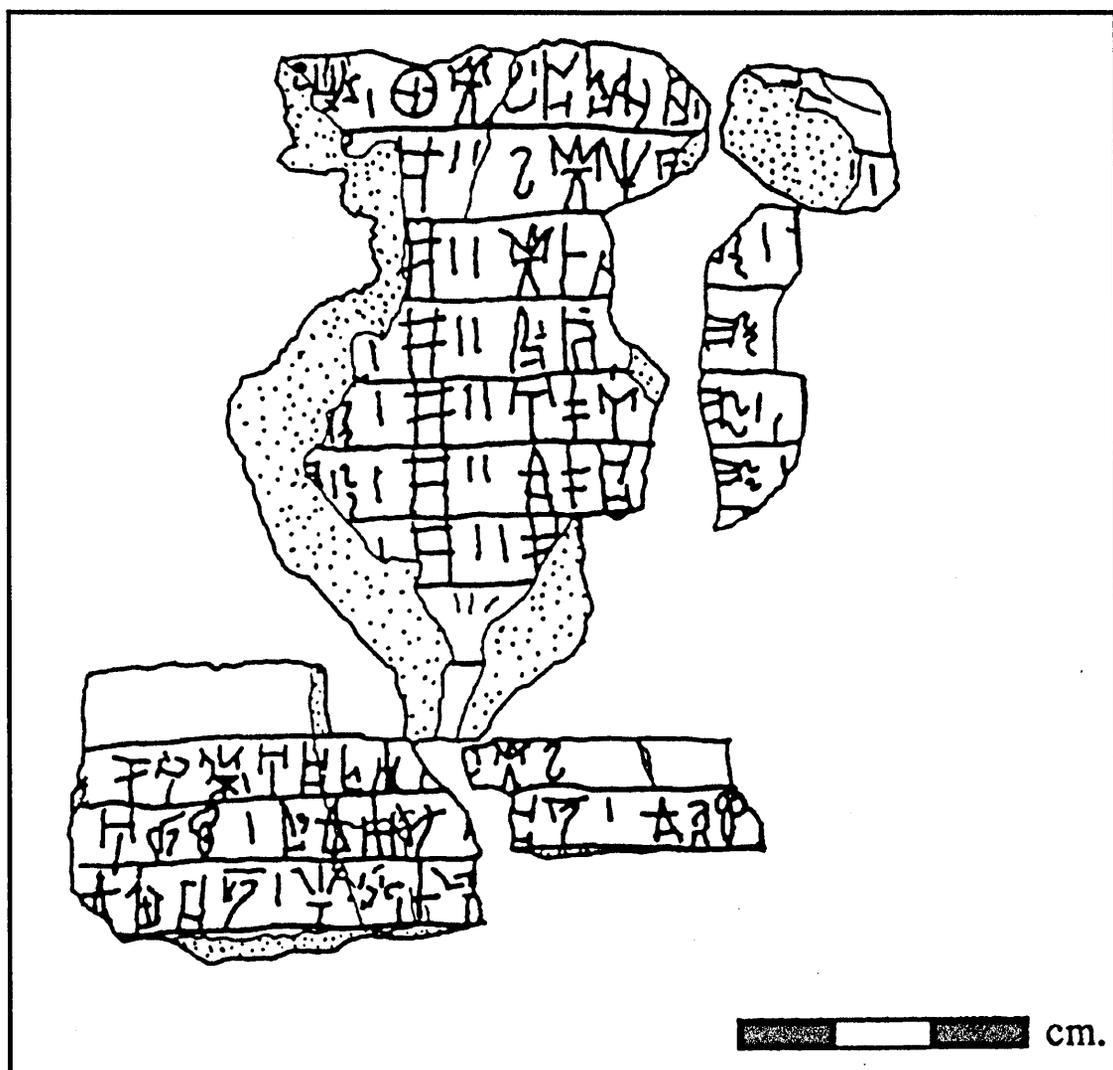


Jn 881 (Smith 1992-1993: 257, fig.35)

Jn 881

(S310 H2)

- .1 e-re-e-we, o-pi-ko-wo AES M 1
 .2 o-pi-su-ko AES M 4 N 2
 .3 vacat
 .4]qa-te[], ke-ro-te AES M 2
 .5]ja, []vac. [AES qs] v.
 .6]jo, a-to-mo[
 .7 deest
 .8] AES N qs [
 .9] AES N 3
 .10] AES M 1
 .11 vacat [] vacat
 .12 vacat [] vacat



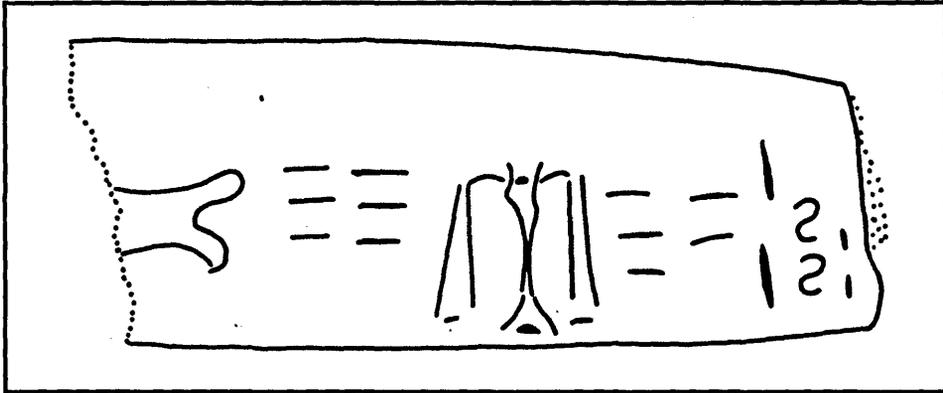
Jn 927 (Smith 1992-1993: 259, fig.36)

Jn 927

(S310 H2)

- .1]mę-no , ka-ke-we , ta-ra-si-ja , [e-]kọ-te
.2 AES]N2 we-ke-i-jo[]AES M 1 N[]2
.3 AES]N2 ke-da-si[]AES M 1[
.4 AES]M 1 N2 o-wa-[]AES M 1 N[qs
.5]AES M 1 N2 a-pa-sa-[]AES M 1[
.6]AES M 1 N2 e-pa-ta[AES
.7 AES M] 1 N2 pa-[AES
.8] vac.[
.9 vacat []v.[
.10 to-so-de³, a-ta-ra-si-jo[]ka-ke-we
.11 a-tu-ko 1 o-pi-si-jo 1[]ta-jo 1 e-ri-ko-wo[1
.12 e-pi-ta-jo 1 i-mo-ro-ne[-u 1
.13 deest

Knossos



Knossos Tablet Oa 730 (Chadwick *et al.* 1986: 277)

Oa 730

(-)

J*167 60

L 52 M 2

The principal documents discussed in,

Chapter VI

**The public face of palatial patronage? The
political sanctioning of social identities.**

Pylos

An 607

Ep 704

Tn 316

Eb 321

Un 443

Ua 1413

Un 1426

Knossos

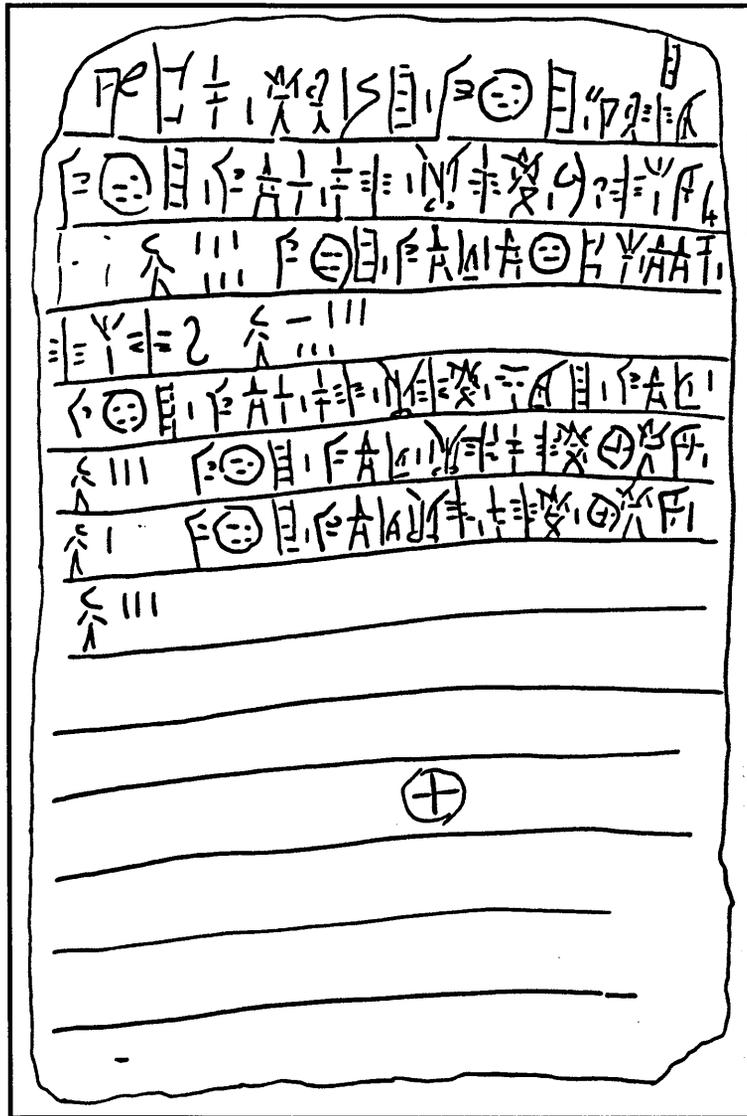
E 777

Fp(2) 363

Lc 535+538

Thebes

Of 26, 33, 36

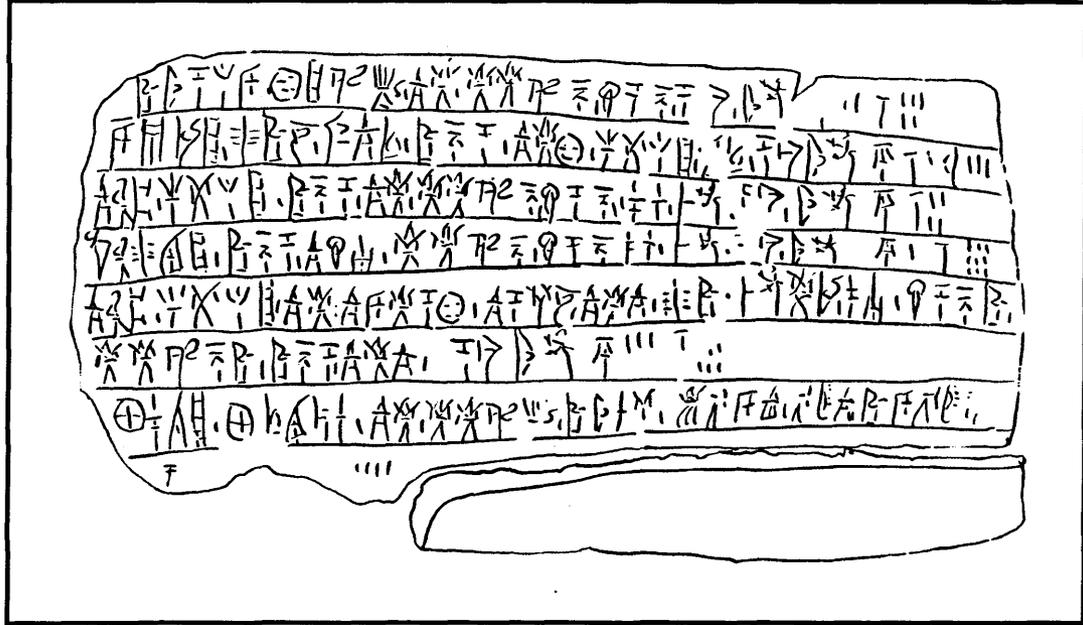


An 607 (Bennett 1955: 60)

An 607

(H1)

- | | |
|-----|--|
| .a | -ja |
| .1 | me-ta-pa , ke-ri-mi-ja , do-qe-ja , ki-ri-te-wi- |
| .2 | do-qe-ja , do-e-ro , pa-te , ma-te-de , ku-te-re-u-pi |
| .3 | MUL 6 do-qe-ja , do-e-ra , e-qe-ta-i , e-e-to , |
| .4 | te-re-te-we MUL 13 |
| .5 | do-qe-ja , do-e-ro , pa-te , ma-te-de , di-wi-ja , do-e-ra , |
| .6 | MUL 3 do-qe-ja , do-e-ra , ma-te , pa-te-de , ka-ke-u , |
| .7 | MUL 1 do-qe-ja , do-e-ra ma-te , pa-te-de , ka-ke-u , |
| .8 | MUL 3 |
| .9 | vacat |
| .10 | vacat |
| .11 | KA |
| .12 | vacat |
| .13 | vacat |

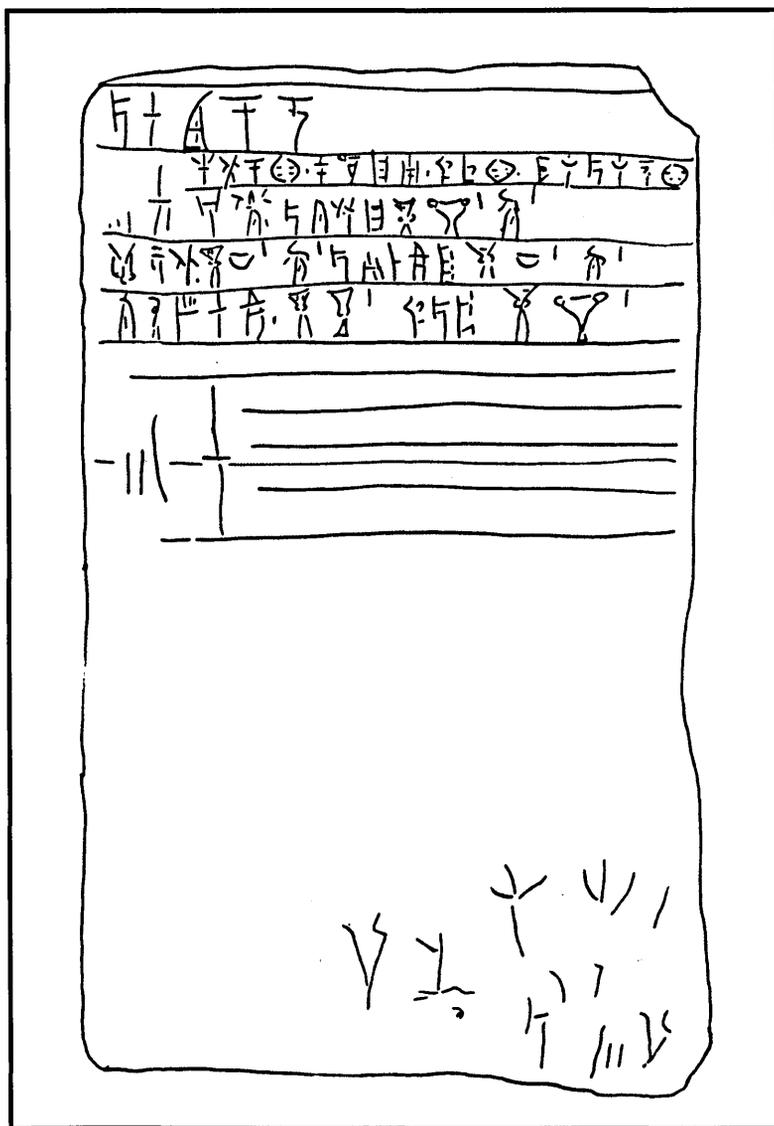


Ep 704 (Bennett 1955: 80)

Ep 704

(S74 H1)

- | | | |
|----|--|-------------|
| .1 | o-pe-to-re-u , qe-ja-me-no , e-ke , ke-ke-me-na , ko-to-na , to-so , pe-mo[] | GRA 2 T 5 |
| .2 | u-wa-mi-ja , te-o-jo , do-e-ra , o-na-to , e-ke-qe , i-je-re-ja , ke-ra , to-so pe-mo | GRA T 1 V 3 |
| .3 | e-ri-ta , i-je-re-ja , o-na-to , e-ke , ke-ke-me-na , ko-to-na , pa-ro , da-mo , to-so , pe-mo | GRA T 4 |
| .4 | ki-ri-te-wi-ja , o-na-to , e-ko-si , ke-ke-me-na , ko-to-na pa-ro , da-mo , to-so , pe-mo | GRA 1 T 9 |
| .5 | e-ri-ta , i-je-re-ja , e-ke , e-u-ke-to-qe , e-to-ni-jo , e-ke-e , te-o , da-mo-de-mi , pa-si , ko-to-na-o , | |
| .6 | ke-ke-me-na-o , o-na-to , e-ke-e , to-so pe-mo | GRA 3 T 9 |
| .7 | ka-pa-ti-ja , ka-ra-wi-po-ro , e-ke , ke-ke-me-no , o-pe-ro-sa , du-wo-u-pi , wo-ze-qe , o-u-wo-ze , [[]] | |
| .8 | to-šo[pe-mo | GRA]4 |



Tn 316 recto (Bennett 1955: 36)

Tn 316

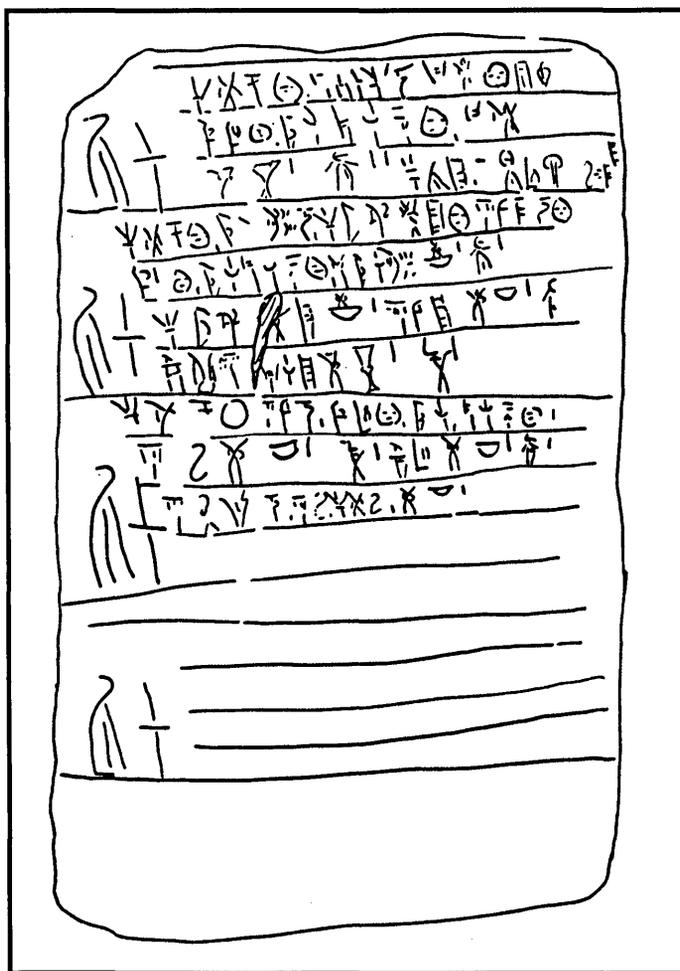
(H44)

Recto

- .1 po-ro-wi-to-jo ,
- .2 { i-je-to-qe , pa-ki-ja-si , do-ra-qe , pe-re , po-re-na-qe
- pu-ro {
- .3 { a-ke , po-ti-ni-ja AUR *215^{VAS} 1 MUL 1
- .4 ma-na-sa , AUR *213^{VAS} 1 MUL 1 pō-si-da-e-ja AUR *213^{VAS} 1 MUL 1
- .5 ti-ri-se-ro-e , AUR *216^{VAS} 1 do-po-ta AUR *215^{VAS} 1
- .6 angustum
- .7 { vacat
- .8 | vacat
- .9 pu-ro { vacat
- .10 | vacat

x

w



Tn 316 verso (Bennett 1955: 36)

(H44)

Tn 316

Verso

x

- | | | | |
|-----|---------|--|-----|
| .1 | | { i-je-to-qe, po-si-da-i-jo, a-ke-qe, wa-tu | |
| .2 | | { do-ra-qe, pe-re, po-re-na-qe, a-ke | |
| .3 | a pu-ro | { AUR *215 ^{VAS} 1 MUL 2 qo-wi-ja, na-[], ko-ma-we-te- | -ja |
| .4 | | { i-je-to-qe, pe-ṛe-*82-jo, i-pe-me-de-ja-qe di-u-ja-jo-qe | |
| .5 | | { do-ṛa-qe, pe-re-po-re-na-qe, a, pe-re-*82 AUR*213 ^{VAS} 1 MUL 1 | |
| .6 | pu-ro | { i-pe-me-ḡe-ja AUR*213 ^{VAS} 1 di-u-ja AUR *213 ^{VAS} 1 MUL 1 | |
| .7 | | { e-ma-a ₂ , a-re-ja AUR *216 ^{VAS} 1 VIR 1 | |
| .8 | | { i-je-to-qe, di-u-jo, do-ra-qe, pe-re, po-re-na-qe a-ke | |
| .9 | | { di-we AUR *213 ^{VAS} 1 VIR 1 e-ra AUR *213 ^{VAS} 1 MUL 1 | |
| .10 | pu-ro | { di-ri-mi-jo di-wo, i-je-we, AUR *213 ^{VAS} 1 [] vacat | |
| .11 | | { vacat | |
| .12 | | { angustum | |
| .13 | | { vacat | |
| .14 | | { vacat | |
| .15 | pu-ro | { vacat | |
| .16 | | { vacat | |
| w.. | | | |

Others

Eb 321 (H41)

- .A ki-ri-te-wi-ja , e-ko-si-qe , o[-]na[-to ke-ke-]me-na-o
.B ko[-to-na-o pa-ro da-mo to-so-]de , pe-mo GRA 1 T 9

Un 443 (H6)

- .1 ku-pi-ri-jo , tu-ru-pte-ri-ja , o-no LANA 10 *146 10
.2 po-re-no-zo-te-ri-ja LANA 3
.3]d̥o-ke , ka-pa-ti-ja , HORD 2 te-ri-ja GRA 1 LANA 5
.4 [[]]

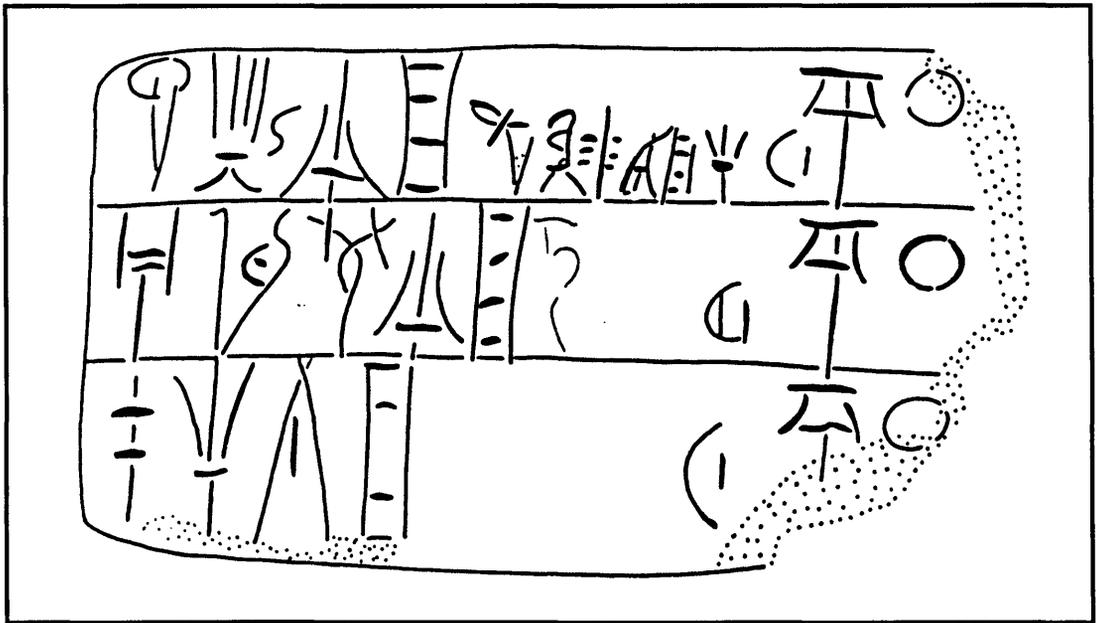
Ua 1413 (Ci)

- .a *146 7 *166+WE 1 [
ro-u-si-jo a-ko-ro , po-re-no-tu-ṭe[

Un 1426 (Ci)

- .1]e-we ,
.2 wa]-na-ka-te
.3]a₃-sa GRA 12
.4 *deest*
.5]ki-ri-te-wi-ja-pi ,
.6] NI 5

Knossos



Knossos Tablet E 777 (Chadwick *et al.* 1986: 295)

Kn E777⁴

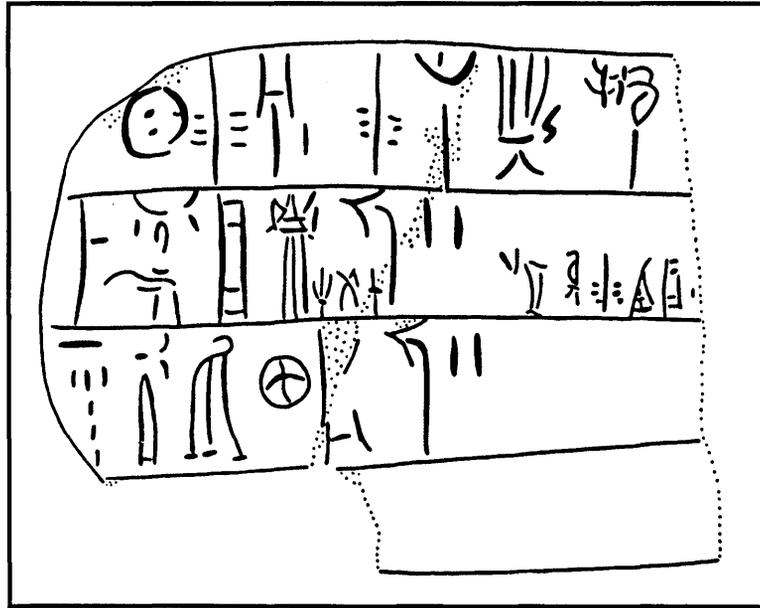
(-)

.1 ko-no-si-ja / ki-ri-te-wi-ja-i	LUNA 1	GRA 100[
.2 a-mi-ni-si-ja	LUNA 1	GRA 100 [
.3 pa-i-ti-ja	LUNA 1	GRA 100[

v.1 a-ze-ti-ri-ja	GRA 10
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reliqua pars sine reqlis

⁴ Modified following Killen (2004: 166).



Knossos Tablet Fp 363 (Chadwick *et al.* 1986: 139)

KN Fp (2) 363

(222)

- .1 qe-te-a , te-re-no OLE [
 .2 da-*83-ja-de / i-je-ro S 2 ki-ri-te-wi-ja , [
 .3 di-wo-pu-ka-ta S 2 [
 .4] *vacat*



Knossos Tablet Lc(1) 535 + 538 (Chadwick *et al.* 1986: 194)

Lc(1) 535 + 538

(103)

- .A ta-ra-si-ja pa-we-a [
 .B ke-ri-mi-ja tu-na-no [
 .C to-sa / pe-ko-to [

Thebes

Of 26

(303)

- .1 pu₂-re-wa ku LANA PA 1 ka-ka[] ku LANA PA 1
.2 su-me-ra-we-jo, ku LANA PA 1 ko-de-wa-o , do-de ku LANA PA 1
.3 di-u-ja-wo , do-de ku LANA PA 1 po-re-si ku LANA 1

Of 33

(-)

- .1 ku-ru-me-no ku LANA PA 1 o-*34-ta-o , do-de ku LANA PA 1
.2 qi-wo , di-u-ja-wo ku[LANA] PA 2

Of 36

(303)

- .1 no-ri-wo-ki-de ku LANA 1 a-ke-ti-ra₂
.2 po-ti-ni-ja , wo-ko-de , a-ke-ti-ra₂ ku LANA 1
 wa-na-ka[
 [

The principal documents discussed in,

Chapter VII

**From patronage to factionalism? Changing
identities in the Post-Mycenaean era.**

Pylos

On 300

Ta 711

