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THE "UNITY OF THE ARCHIVES" AT KNOSSOS

As part of his contribution to the book "On the Knossos tablets" Palmer uses the very detailed information he has gathered about the find-spots to try to prove conclusively that all the Knossian Linear B tablets belong to a period rather later than the end of the palace at Pylos. The present paper is concerned mainly with one of Palmer's arguments, the unity of the archives, but this is in a way fundamental to his whole case. Palmer uses the term "unity of the archives" to indicate that all our inscribed tablets (and sealings) from the Palace of Minos and its environs were preserved in one and the same fire. I examine below some of the conclusions Palmer draws from the internal evidence of the tablets; but first I should like to consider the general probability that all the Knossos tablets can be assigned confidently to the same destruction. The artefacts found with some of the tablet-deposits provide an obvious means of dating the tablets themselves, although the two authors of this book reach very different conclusions about the absolute date. But some deposits either are in a disturbed context or are not closely enough associated with dateable objects to make their own dating certain. When we are dealing with a site in which the deposits are so widely scattered it seems unwise to state dogmatically that all of them must result from the same fire. Such an assertion would not allow for the distinct possibility that the major destruction of the palace, whenever it took place, was preceded or followed by others. Certainly we must not conjure up a fire to help our case; but neither, I think, ought we to under-estimate the propensity of Bronze Age sites, both in Crete and on the mainland, to destruction by fire. If we assume a LM IIIa 1 date for the 'great' destruction at Knossos, we may still think it possible there was also a rather later fire contemporary with the one in the Little Palace, which even Boardman thinks may have come after the 'great' destruction. Once it is admitted that we cannot say whether the tablets in the Little Palace "are to be attributed to the first or the second of the destructions" (66), we may question also the attribution of, for example, the Arsenal deposit to the 'great' destruction; the context there gives us simply a vague *terminus ante quem* ("not

demonstrably later than LM II', 69), and it is legitimate to ask why the tablets in the Arsenal should necessarily come from the 'first' rather than from a 'second' destruction¹. On the other hand, we have no satisfactory *terminus post quem* for the introduction of the Linear B script at Knossos. Evans believed that at least one tablet, K 872, had been deliberately baked long before the main destruction of the palace; he gives good reasons for assuming an earlier date for this tablet², but its exact find-spot now seems to be uncertain³. It is less likely that any Linear B tablets were *accidentally* preserved at any time before the major destruction, since there does not seem to have been a fire at Knossos during the fifteenth century. Even this statement may have to be modified if Hood is right in seeing some signs of destruction involving LM Ib pottery⁴.

It seems to me most likely that the majority of our tablets date from a devastating fire c. 1400 but that others were baked hard by smaller, subsequent fires. We know there had been such fires at Knossos prior to, say, 1500, and it is arbitrary to deny the likelihood of similar fires during the two centuries or so of the LM III period. This is much the same view as that adopted by Hood, who accepts the possibility of a restored palace after 1400 rather than an occupation by Evans' 'squatters'⁵. Still, Evans himself had emphasized that the Minoan civilization at Knossos was not destroyed at one blow⁶. An unbroken development of culture is attested by the tombs in the Zapher Papoura cemetery, which contain LM IIIa 1, IIIa 2, and IIIb 1 pottery⁷, by the continued use in LM III of the

¹ The fact that impressions from the same seal are found in the Corridor of the Sword Tablets and in the Arsenal does not prove that the deposits in those two places are contemporary because, as Boardman observes, "seal-stones might continue in use for a long time" (72); according to Kenna, Cretan Seals 56 the impressions of a collared bitch derive from a LM I seal.

² Its clay is of "exceptionally hard-baked texture", of the same fabric as that of the Knossian Linear A tablets; and "the Vapheio shape of cup here illustrated, though of earlier tradition in Crete, seems to have been rather specially in vogue at that time [sc. LM Ib]" (PM IV 729).

³ Palmer 86

⁴ Gnomon xxxv (1963) 632. If this destruction at Knossos is confirmed, it might perhaps be brought into relationship with the destruction by fire of several other Cretan sites at about the same time, Furumark OpArch vi (1950) 254. See also JHS Archaeological Reports (1962—1963) 32—33 (Palaikastro and Zakro).

⁵ Antiquity xxxv (1961) 4

⁶ PM IV 944. See also Raison, Minos vii (1963) 166

⁷ Furumark, Chronology of Mycenaean pottery 104—105; cf. the tombs on Upper Gypsades, Hood, ABSA liii-liv. (1958—1959) 196.

typically Minoan 'house-sanctuary'⁸, and by the gradual evolution of native ceramic styles⁹. There is no reason to suppose that the art of writing, which had been practised at Knossos for several centuries, was lost at a time of fairly vigorous activity on other fields. Scribes were no longer needed to serve a restless and meticulous bureaucracy; but the inhabitants of Knossos were still engaged, for instance, in a considerable oversea trade¹⁰, which the use of writing would have greatly facilitated. Nothing we know about the conditions of life in the LM III palace at Knossos makes the existence of writing there unlikely¹¹. Writing had appeared first at the very beginning of the Cretan palaces, notably those at Knossos and Phaistos, at a time when each was occupied mainly with its own affairs, and scribal skill was not by any means exclusively the appanage of a great palatial centre. It may be objected that, even if some scraps of writing survive from LM III Knossos (such as the vase-fragment Z 1715¹²), *tablets* would hardly have figured in the palace economy of those days. Once again it is worth stating that we do not know enough about the place of writing in a Bronze Age community to be certain on this point. The earliest tablet from Enkomi does not occur in what we think of as a 'palatial' context¹³, and it is likely that the inhabitants of fourteenth century Knossos had no less need of records, and no less skill in writing them, than the Cypriots in the sixteenth century.

So far I have assumed that the major destruction at Knossos took place in LM IIIa 1; but, even if Palmer's much later date for this destruction is accepted, the possibility that it was preceded by another fire cannot be ruled out. Curiously enough, Palmer himself implies that this was so by suggesting that some tablets found

⁸ E. g. PM I 425—429 (especially 427 n. 1), 576; cf. Nilsson, *Minoan-Mycenaean religion*² 77—116. Continuity in the use of house-sanctuaries is seen even more clearly at Ayia Triadha, Luisa Banti, ASAA iii-v (1941—1943) 72.

⁹ Furumark, *Mycenaean pottery* 175

¹⁰ Especially with Cyprus, Catling and Karageorghis, *ABSA* lv (1960) 121—122, 126

¹¹ It is not legitimate to conclude with Evans, PM II 543 n. 1 that the makers of the 'fetishes' in the Little Palace could not have known the art of writing. This very crude type of concretion is not typical of Minoan cult in the Little Palace or elsewhere, cf. Nilsson *op. cit.* 91.

¹² Which Raison assigns with great plausibility to LM IIIa 2, *BCH* lxxxv (1961) 414—415; cf. Kadmos ii (1963) 137. Boardman 66 concedes the possibility that "the Linear B characters were still being written at Knossos" after the principal destruction.

¹³ Dikaïos, *Kadmos* ii (1963) 45 calls it a 'fortress'.

beneath a blocked doorway¹⁴ belong to an earlier archive. At one stroke therefore Palmer destroys the concept of a "unity of the archives" which he has built up with so great an accumulation of detail; since, if there were two fires within the lifetime of the 'Mycenaean' palace, both of which were capable of preserving tablets, there is no reason which compels us to assign a given deposit of tablets to the later rather than to the earlier fire. Palmer has been rebuked for his assumption of a fire which ante-dated the major destruction¹⁵, and indeed the contents of Dv 747 (one of the tablets found under the doorway) cannot be said to corroborate an earlier date¹⁶. At the same time, given Palmer's chronological framework, we have to reckon with a Mycenaean occupation of Knossos lasting for some 200 years; just as the culture of the LM III palace at Knossos was related to that of the mainland, so its destruction at the end of the period was caused by the same agencies which devastated the mainland in LH IIIb. It is well known that some of the most important mainland sites show evidence of two or even three separate fires during the LH IIIb—c period¹⁷; and if LM III Knossos is to be placed wholly within the ambit of Mycenaean culture it may well have suffered the double devastation that is attested on the mainland.

If their environment is so confused, do the tablets themselves present any criteria by which we can declare them to belong to the same archive? The more or less homogeneous writing-system used on all the tablets is no guarantee of their contemporaneity, since closely similar systems are in use at Mycenae and Pylos; whether or not the mainland tablets belong to the same chronological horizon as those from Knossos, it is clear that a script which was carried *in toto* to the mainland from Crete (or in the opposite direction), in so many minute peculiarities, *could* have survived virtually unchanged for a long period. Our problem, then, is to establish a system of cross-references among the major deposits at Knossos; there will always be a number of fragmentary tablets and tablets

¹⁴ In the Corridor of the Stone Basin, Palmer 110

¹⁵ Boardman, *Antiquity* xxxviii (1964) 46

¹⁶ "It is significant that this owner [*adijewo*] does not occur elsewhere in the very large number of CATTLE tablets" (Palmer 114). The point loses its significance when it is remembered that some other owners' names are of unique occurrence on the CATTLE tablets e. g. *kurumenijo* Da 1173, *oretewo* Dc 439, *sewoto* Da 1268.

¹⁷ There seem to have been two distinct fires at Mycenae before the final devastation in LH IIIc: Desborough, *The last Mycenaeans* 74; Älin, *Das Ende der mykenischen Fundstätten* 23—24. So also at Tiryns, Älin 36.

from doubtful contexts which have to be left out of account. So far as I can see, the only satisfactory system would rest on the identification of scribal hands. Bennett has drawn valuable conclusions from details of the hands of individuals scribes at Mycenae¹⁸; of course the problem at Knossos is far more complicated, but it should be possible to consider, for example, one of the large deposits in the Domestic Quarter and relate the handwriting of each scribe to the writing on tablets in other parts of the palace, and so on from deposit to deposit. In this way the postulated network of cross-references could be built up, which might embrace the majority of our tablets. In making his deductions from the contents of the tablets Palmer relies for the most part on the occurrence of the same personal name on tablets in different parts of the palace. This criterion is not nearly rigorous enough for our purposes. Even if the efficacy of the method is admitted (and I believe it is open to grave objections), only a very few tablets are involved, whereas precisely what is wanted is a system that will link a significant number from at least the principal deposits. We are justified in asking for much more than the simple coincidence of the same name in different find-spots; in default of any indication that two tablets bearing the same name were written by the same scribe, we need some further means of deciding whether the same person is being referred to.

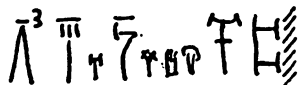
Palmer's equations of personal names may now be examined. Some of them do provide as satisfactory a link between two or more deposits as could be expected from this method; a striking example (Palmer 77) may be expressed as follows:

Room of the Chariot Tablets	Arsenal	Northern Entrance Passage
V 60.2 <i>kaparijo</i>	U 4478.14 <i>kaparijo</i>	
	.15 <i>wekadijo</i>	V 831.3 <i>wekadijo</i>
Vc 295 <i>matiko</i>		.4 <i>matiko</i>

Even so, this set of equations should be confined to the names; we are right to object to Palmer's further correlations of subject-matter. He writes on p. 161: "Attention may be drawn to the analysis of the names of the *anioko* from the Room of the Chariot Tablets (pp. 77ff.), and especially to the names in U 4478: *kaparijo*, *weka-*

¹⁸ MT II 89—95. His identification of hands at Pylos is more tentative.

dijo. The cluster echoes are unlikely to be due to chance, and U 4478 is in any case likely to deal with 'Arsenal' matters. *177 is an unidentified ideogram; but if the men concerned are charioteers, it must represent a piece of equipment drawn by such men and peculiar to their activities." Thus, the men listed on U 4478 are assumed to be charioteers because one of them (*kaparijo*) appears also on some texts in the Room of the Chariot Tablets. What evidence is there that the *kaparijo* of these texts is a charioteer? None, except that one of the tablets, V 60, is taken to be "a list of men's names headed *anioko* 'chariot drivers'" (77). An examination of the structure of V 60 will show what slight grounds there are for such a description. The first line of the tablet reads as follows: Minuscule signs, representing probably *anioko*¹⁹, are interspersed in a curious way among the signs which spell the first name on the tablet, *wodijo*:



It would be most surprising for the 'heading' to be expressed in this way, by minuscule signs which, by their very position, can surely refer only to *wodijo*²⁰. The description *anioko* (i. e. *ἀνιόχος*) is deliberately restricted by the scribe to this firstname, and we are not justified in extending it to any of the others²¹.

In his chapter III. 1 Palmer adduces eight personal names, each of which is found in at least two different parts of the palace. Here again it is important to insist on some means of identification apart from mere similarity of name. The name *sukere* does meet this further requirement, since on both As 40. 6 (Southwest door) and As 1516. 20 (Hall of the Colonnades) *sukere* is associated with

¹⁹ The v. II. *anipiko*, *aniwoko*, given by KT II, are now stigmatized as unlikely by Chadwick and Baumbach Glotta xli (1963) 200. But the third sign is not very clear; the drawing in SM II gives *anijako*.

²⁰ Admittedly the 'heading' on many of the Sd tablets appears in the lower of two (sometimes in the lowest of three) registers; but at least the heading is expressed by a majuscule *IQIJA* or *IQIJO*, such as we should expect.

²¹ An alternative line of interpretation is suggested by the structure of C 50. There also words are written minuscule between signs of normal size, and the two words so written are proper names, *tepara* and *pereqota*. It is therefore possible that *anioko* in V 60 may be simply one of the list of personal names recorded by the tablet; for similar 'meaningful' names cf Landau, Mykenisch-griechische Personennamen 188—215.

setoiija. It is of course reasonable to think this is the same man, especially as many of the personal names on As 1516 are not distinguished in this way; it is probable that more than one *sukere* was known to the scribe who wrote this tablet²². The association of *anogota* and *era* on Dv 45, Dv 440, and Da 1323 is equally convincing (the first and third of these tablets are structurally very similar); but we have no means of knowing, as Palmer assumes, that this is the same *anogota* who appears on Vc 173 and Ak 615 unconnected with *era*; the fragmentary tablet Vc 173 offers especially poor testimony since it records only: *anogota 1*. As a parallel to *sukere: setoiija:: anogota: era* we have *rawogono: dawo* on both Dl 928 and Dv 1650; we cannot tell whether this is the *rawogono* of B 798. 7 (a mere list of men's names, with nothing about their occupations or affinities). The only reason for identifying the *komaweto* on Dk 931 with a man of the same name on Dv 1272 is that in both cases he is recorded as an owner of beasts. A secure identification is dangerous because we do not know how many men of this name came within the purview of the bureaucrats; it is possible even that the *sajo* and *ekoso* on these two tablets respectively were intended to distinguish two men who had the same name. An even more serious objection to the use of *komaweto* as evidence for the unity of the archive is that it occurs twice in lists of men at Pylos (An 519. 10, Jn 750. 9). The fact that this name is preserved on Pylian texts makes it probable that it was reasonably common among the Greek archives of the Bronze Age Aegean and that it is therefore likely to occur in quite unrelated contexts. Even greater care is needed in dealing with the name *wewesijo*. As Palmer says, this occurs in a large number of texts at Knossos (to which we may now add Dv 8174, 8196, 8241); but it may appear so frequently just because it was a very common name and not because there was only one important 'owner' with this name. *wewesijo* is found no fewer than three times at Pylos (Jn 431. 18, 658. 2, 725. 3). Palmer's reference to *daminijo* I cannot understand. The name of this man, we are told, "is particularly clear since he appears as an owner exclusively at the place *kutato*: Dv 447 (Magazine III), and frequently in the Domestic Quarter (Dk 1076, etc.)" (171). The fact is that *daminijo* is not associated exclusively with *kutato*; on at least five tablets, including the very two mentioned by Palmer, *daminijo*

²² A similar contrast is seen at Pylos; *rouso aketirija* (Aa 717) appears to be distinguished from simple *aketirija* (Aa 85). Cf *kinidija* (Aa 792) and *puro kinidija* (Ab 189) etc.

appears without a mention of *kutato*. Add that this name too occurs at Pylos (An 610. 13), and Palmer's deductions from its appearance at Knossos begin to look very unimpressive. It may be worth noticing that yet another Knossian name cited by Palmer, *periqotao*, is found in a list of men at Pylos, Jn 693. 7. The most that can be said is this: if one is concerned above all to establish the unity of the archives the coincidence of some of the names may be adduced to help one's case; but if not, the personal names appear merely to confirm, what is suggested already by a reading of Landau, that there was quite a small stock of names available to the Linear B scribes and that such coincidences are only to be expected.