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### **II**

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## THE LAST DAYS OF THE PYLOS POLITY

What do the Linear B texts tell us about what Mycenaeans in the palatial territory of Pylos were doing during a very crucial period in the existence of their society, its final recorded days<sup>1</sup>? Archival and pinacological studies have never been used fully in order to understand what was going on at the Palace of Nestor and in the community of Pylos immediately preceding the destruction of the palatial center. Here I shall discuss how we can use tablet find-spots, scribal hands, scribal assignments and textual cross-references to propose a possible rough internal chronology for some of the surviving tablets. I shall then propose a tentative chronology of events during the last months before the palace was destroyed, while offering some criticism of the alternative hypotheses for when in the year the destruction occurred.

Understanding the factors that contributed to the destruction of the major Mycenaean palaces at the close of the Late Bronze Age is a major problem in Greek prehistory. The evidence from the Linear B tablets at Pylos has been used to propose that the Mycenaean kingdom of Messenia was facing an external military threat immediately before its final destruction and that this threat caused the palace to take extreme emergency measures<sup>2</sup>. The architectural history of the site of Pylos also supports the view that the palace center was responding over time to some sort of crisis: drawing installations for economic production in upon itself during the last half of LH IIIB and otherwise modifying the layout of the central palace in order to limit access and perhaps to increase security<sup>3</sup>. The most sensational piece of textual evidence is the offering tablet Tn 316, which according to the "state-of-emergency" thesis represents a last, desperate attempt to enlist the benevolence and support of the chief gods of the community (with the notable and problematical exception of its chief male god Poseidon) through precious offerings, including possibly human sacrifice<sup>4</sup>. According to this hypothesis, "The untidy execution of Tn 316 and its unfinished state suggest that the tablet was written in a hurry and that the writer was probably prevented from finishing it, or at least from making a fair copy of it, by the disaster which befell Pylos"<sup>5</sup>. Thus Tn 316 would be

- 1 I thank S. Hiller for his penetrating comments on my paper at the *Politeia* conference, and E. Stavrianopoulou for private discussion of the text of Tn 316 on the same occasion. P. Carlier and E. Sikkenga also spoke with me about topics in this paper. I benefitted from taking up different issues in this paper during seminars at the Universities of Uppsala and Lund in May-June, 1994. I am responsible for any final shortcomings in ideas or interpretations.
- 2 L. BAUMBACH, "An Examination of the Evidence for a State of Emergency at Pylos c. 1200 B.C.", in A. HEUBECK and G. NEUMANN eds., *Res Mycenaee* (1983), 28-40, and J.T. HOOKER, "The End of Pylos and the Linear B Evidence", *SMEA* 23 (1982), 209-217.
- 3 J. WRIGHT, "Change in Form and Function of the Palace of Nestor at Pylos", in C.W. SHELMEERDINE and T.G. PALAIMA eds., *Pylos Comes Alive* (1984), 19-29, and F.A. COOPER and E. SWAIN, "Minnesota Archaeological Researches at Pylos, 1991-1993 Seasons", *AIA Abstracts* 17 (1993), 11-12.
- 4 J. CHADWICK, *The Mycenaean World* (1976), 89-92. There is now a full and sober survey of the various views on Tn 316 in D.D. HUGHES, *Human Sacrifice in Ancient Greece* (1991), appendix, 199-202 with extensive notes.
- 5 BAUMBACH (*supra* n. 2), 33.

among the very last tablets written and would have been written in a panic. Finally the references to month names on tablets from the oil storage magazines and especially to the "month of sailing" there and on Tn 316 proper have been used to propose that the Palace of Nestor was destroyed in early spring <sup>6</sup>.

It will not be possible here to discuss all relevant data, tablets or theoretical implications in detail <sup>7</sup>. I shall focus mainly on the archival circumstances and epigraphical details of a few key tablets and series <sup>8</sup>. I shall try as much as possible to make clear the limitations of my arguments about relative chronology.

I start with Tn 316: where it was found, when it was written, how it was written, what conclusions we can draw from its characteristics as a clay-tablet record, and its interpretation. It is important to observe that Tn 316 was already archived in Room 8 (Pl. LXXIVa-b), in Grid 23 which is the same specific area where other standard or general reference tablets like Er 312 and several of the A- and E-series were found. However, it can be argued that after Tn 316 and other tablets were archived in Room 8, still more tablets reached the Archives Complex. These were still resting in Room 7 when the palace was destroyed. Especially crucial for our understanding of which tablets are likeliest to be chronologically latest is the identification of two areas that contain special texts. The first such area is Grid 52. Grid 52 contains many transport-basket labels. It also contains several whole series of tablets that are of the sort to have been written elsewhere <sup>9</sup> and then to have been brought to the central archives for final handling and/or filing and storage. The second special area is Grid 83 immediately to the left of the entrance into Room 7 (as reconstructed by Palaima and Wright). Grid 83 housed a small and special group of tablets (chiefly and significantly Un 718 and tablets of the Ta series) that also had not yet been processed and filed. These are the tablets which probably entered the archives last.

In my opinion, therefore, it is unlikely that Tn 316 "belongs to the last days of the palace". Other tablets and tablet series were delivered to the central archives after Tn 316 was processed. There are, however, two problems which will always prevent us from drawing firm conclusions. First, there is no way of knowing how long (days, weeks, months) after the archiving of Tn 316 the tablets in Grid 52 and/or in Grid 83 were written and delivered to the central archives. Second, it is even possible to propose that, given the extraordinary contents of Tn 316, it was processed immediately upon being brought to the Archives Complex,

6 CHADWICK (*supra* n. 4), 89-92 and especially 192: "Since for the ancients the sailing season opened about the end of March, this [*po-ro-wi-to-jo* on Tn 316] would be the name of a spring month; and since, as has been demonstrated, the document [Tn 316] very likely belongs to the last days of the palace, it follows that Pylos was destroyed in early spring". For a review of Mycenaean month names within the context of later Greek month names, see A. SAMUEL, *Greek and Roman Chronology* (1972), 64-65 with notes and references. The most comprehensive examination of the month names from Pylos is C. TRÜMPY, "Nochmals zu den Mykenischen Fr-Täfelchen. Die Zeitangaben innerhalb der Pyllischen Ölrationsserie", *SMEA* 27 (1989), 191-234.

7 For those who are interested, I propose to discuss Tn 316 in full detail at the 11th International Colloquium on Mycenaean Studies which will take place in Salzburg in 1994.

8 T.G. PALAIMA, *The Scribes of Pylos* (1988), *passim*. T.G. PALAIMA and J. WRIGHT, "Ins and Outs of the Archives Rooms at Pylos", *AJA* 89 (1985), 256-262.

9 Series Es: 15 tablets by 2 different scribes, Hand 11 (1 tablet) and the master scribe Hand 1, all of which were found in Grid 52 except for Es 703 and three out of seven components of Es 644 and Es 652 [+1453]. These latter seem to have spilled into Grids 64 and 8347 (one component each of Es 644 and Es 652 [+1453]). Series Sh: 12 tablets by S733-Cii, all in Grid 52 with transport-basket label Wa 732 by the same hand. Also many transport-basket labels of series Wa were found in Grid 52 separated from their tablets which had already been processed and stored in Room 7. On the internal chronology of the Sh series and the identification of Grid 52 as the area for label discards and temporary pre-processing storage of tablet baskets that have been brought to the Archives Complex, see T.G. PALAIMA, "'Contiguities' in the Linear B Tablets from Pylos", *Atti del II Congresso Internazionale di Micenologia* (forthcoming).

despite the fact that other tablets and series were already there in Room 7 awaiting processing. According to this line of reasoning, Tn 316 could have leap-frogged over other tablets ahead of it in the queue. When we analyze the relationship between the contents of Tn 316 and other text series, we shall find reasons for judging this scenario less likely <sup>10</sup>. But it cannot be dismissed out of hand.

If we accept the relative chronology just outlined, the scenario according to which Tn 316 and other tablets from the Palace of Nestor provide evidence for a dire emergency leading to extraordinary human sacrifices is no longer very attractive. At the very least, some of the activities recorded on tablets in Grid 52 and Grid 83 took place after the offerings on Tn 316 were performed and after the tablet itself was written and then filed away for future reference. When we consider what was done in the very last days of the Palace of Nestor after the events on Tn 316, we shall have further cause to doubt that Tn 316 was scribbled down in awe- and fear-inspired haste by the scribe identified as Hand 44.

State-of-emergency theorists have one solid piece of evidence on which to build: the Palace of Nestor was destroyed and we assume that some tablets must have been written sometime relatively close to the actual destruction. If the destruction was an anticipated event or the result of factors accumulating through time, the tablets should offer some clues as to what problems, external or internal, the palatial administration was confronting and what measures it adopted to address these problems.

The acknowledged difficulty has always been that the evidence is ambiguous. This is even so for relatively fully documented subjects like the organization and operation of the bronze industry at Pylos. Are the allotments of small quantities of raw material to large numbers of geographically scattered smiths and the 'recycling' of 'temple bronze' for military weapons (Jn 829) standard operating procedure for such a regional industry in the Late Bronze Age or extraordinary adjustments to emergency conditions? Is it an effective response to an external or internal threat to ship small bits of raw materials to work groups in scattered locations? Or are the small amounts themselves the result of economic, social, political or military problems? What were the cultural sensibilities of the Mycenaeans about items of bronze, whether utensils or offerings, that belonged in the 'temple sphere'? Would reuse of metal that had essentially become scrap have been a normal and sensibly frugal use—like our current recycling of aluminum, plastic and paper—of a hard-to-acquire raw material or would it have been undertaken only in a state of emergency <sup>11</sup>? The absence of any parallel texts dealing in such detail with bronze production and bronze working from other Mycenaean sites and other times in the Mycenaean palatial period makes these questions virtually unanswerable <sup>12</sup>.

The same unanswerable question as to whether procedures are normal or abnormal can be posed with regard to the rower texts, the *o-ka* coastal defense tablets, and ultimately Tn 316. That these military operations or religious practices are exceptional proceeds from the initial premise: the palace was destroyed and the conditions that led to this destruction should be evident, directly or indirectly, in our texts. But where? Tn 316, the Jn tablets, the rower

10 In particular, tablet Un 718 and tablets of the Es and Ta series and tablets On 300 and Ta 711 offer information about probable tablet sequence that makes this alternative less likely.

11 See T. LINDERS, "The Melting Down of Discarded Metal Offerings in Greek Sanctuaries", *Scienze dell'Antichità* 3-4 (1989-1990), 281-285, for a treatment of the regular procedures for the recycling of precious metals from old religious dedications in the Hellenistic period. Such metals could be melted down into ingots under the supervision of commissions made up of officials whom we would classify as 'secular' and 'religious'. The potential parallels with Jn 829 should be obvious.

12 A paper by J. SMITH, "The Pylos Jn Series", forthcoming in *Minos*, traces sequential ordering within the bronze distribution records (Jn series) and makes clear thereby that this, too, was no absolutely last-minute emergency procedure.

and *o-ka* tablets represent only a small portion of the overall tablet material from Pylos. Alongside these texts, most of which were archived in Room 8, are tablets that show a palatial center that can maintain normal operations and fulfill its economic obligations. The Aa, Ab, Ad series record the distribution of normal levels of rations to women workers, mostly in the cloth industry. They do not reflect shortages<sup>13</sup>. The assemblage and repair of chariots and the refurbishing of armor in the Northeast Workshop give every indication of being standard *o-pa* and perhaps *wo-ka* work. These records of military production would appear normal if transferred to another context, such as the Knossos Room of the Chariot Tablets archives. Moreover, the armor *o-pa* work was, according to my point of view, among the very last activities ever recorded at the site of Pylos<sup>14</sup>. It is recorded in a regular and systematic way.

Regional taxation records of the Ma series and the flax production records (Na series) do not show any huge shortages nor any exemptions granted to special groups (*ka-ke-we*, *ku-na-ke-ta*, *na-u-do-mo*) that they might not have had on a regular basis<sup>15</sup>. The palace was also concerned in its last administrative period with meticulous recording of landholdings (Eb, En, Eo, Ep) in the district of *pa-ki-ja-ne* for a large group of religious functionaries of varying standing (*do-e-ro/-ra* and *i-je-re-u/-ja* and *ka-ra-wi-po-ro*) and certain craftsmen associated with the *wanax* (potter, fuller, *e-te-do-mo*). It also recorded the distribution of oil from its stores, monitored the collection of food and other products for sacrificial and commensal purposes, and in general went about what looks like business as usual. Given what we know from Thucydides and Homer of the problems of piracy and from the archaeological record about fortification techniques and the military ethos of the Late Bronze Age, the need for some standing system of regional defense makes it possible to interpret even the military records as standard operating procedure<sup>16</sup>.

Within this picture then Tn 316 does stand out as a crucial text. If it can be demonstrated that it is reasonable to interpret its current form and contents as reflecting a highly irregular situation and event, then it becomes more credible to interpret the other state-of-emergency tablets in this way. I think, however<sup>17</sup>, that the epigraphical features of Tn 316 have been misunderstood by state-of-emergency hypothesisists. As we have seen, Baumbach believed that the tablet was written in haste and in the end was left unfinished. Chadwick<sup>18</sup> also promotes this view:

13 On the ration allotments and quantities, cf. R. PALMER, "Subsistence Rations at Pylos and Knossos", *Minos* 24 (1989), 89-124.

14 PALAIMA (*supra* n. 9).

15 On Messenian flax production, cf. A.L.H. ROBKin, "The Agricultural Year, the Commodity SA and the Linen Industry of Mycenaean Pylos", *AJA* 83 (1979), 469-474, which includes a detailed discussion of agricultural chronology. We should note that Robkin, p. 470, accepts Chadwick's identification (*supra* n. 4, 153-154) of the unit used to measure flax fiber based on a comparison of productivity in ancient and modern Messenia. This line of reasoning implies that the towns of Bronze Age Messenia recorded in the Na tablets were producing quantities of flax fiber equivalent to those produced by modern villages in a typical year. That is, the production in the last year of the palace, accepting this equation, *would have been absolutely normal*.

16 See T.G. PALAIMA, "Maritime Matters in the Linear B Tablets", *Thalassa. L'Egée préhistorique et la mer. Actes de la troisième Rencontre égéenne internationale de l'Université de Liège, Station de recherches sous-marines et océanographiques (StaReSO), Calvi, Corse (23-25 avril 1990)*, *Aegaeum* 7 (1991), 285-287, and the arguments of J.T. KILLEN therein.

17 PALAIMA (*supra* n. 8), 108-110, as cited by HUGHES (*supra* n. 4), 256 and n. 14. This opinion is shared, in a typically idiosyncratic way, by E.L. BENNETT, JR., "'PU-RO, vacant' (PY Tn 316.7-10, v. 13-16)", in E. RISCH and H. MÜHLESTEIN eds., *Colloquium Mycenaeanum* (1979), 221-234.

18 CHADWICK (*supra* n. 4), 90. I mark with [\*] assumptions or propositions that are highly speculative.

Not only is it an isolated document, with no similar ones to match it; it is also the most disgraceful piece of handwriting [\*] to have come down to us... The easiest explanation [\*] of this muddle is that the writer was trying to record the decisions of an unusually stormy meeting [\*]. But why did he not make a fair copy for storage in the archives and consign this draft to oblivion? The most likely answer [\*] is that he had no time; and that would make sense if the tablet were written within the last few days, perhaps the last few hours, of the existence of the palace. As we shall see later, this supposition fits well with what little we know about the fall of Pylos.

It is an unpinacological aesthetic judgment that Tn 316 (Pl. LXXIVc) is a disgraceful piece of work. This is predicated on the assumption that the Mycenaean scribal administration would—or should—have required what we consider to be an important document to be executed with an eye to its final appearance: that the ancient equivalent of a modern “fair copy” was necessary and would have been done *in clay*. These are untenable assumptions. Even important texts like Un 718 have flaws in layout and formatting, and I could cite other candidates for the prize of “most disgraceful piece of [Linear B] handwriting”<sup>19</sup>. Of primary importance to palatial administrators who employed Linear B clay tablets for *short-term* record-keeping was that information be set down accurately and in a way that could be accessed and understood correctly in the future, if so required. Tn 316 satisfies these standards.

Actually Tn 316 strikes me as a good example of clever and persistent experimentation by a scribe who eventually finds a way to record some rather complicated information. Experimentation—or what I have called ‘tentativeness of formulae’—is common in the work of even very accomplished scribes writing under what we assume to be regular conditions<sup>20</sup>. Comparable experimentation is seen on the only other tablet by the scribe of Tn 316 (Hand 44): Fr 1223 (Pl. LXXIVd) from the oil storage area Room 23. In producing its absolutely routine text of two entries of relatively small allotments of sage- and rose-scented anointing oil to the site of *ti-no*, the scribe shows some of the same pinacological tendencies that he was only supposed to produce under fear and terror of impending destruction and in trembling awe at the human sacrifices offered in a desperate attempt to forestall it. Fr 1223 shares the following features with Tn 316: majuscule headers, hesitation in structuring entries, allowing an ill-contrived header to do service in more than one line. The end result, however, is a very satisfactory and readable record. The same applies to Tn 316, which was judged to be acceptable enough to be archived in Room 8. Either one proposes then the preposterous: that the routine oil tablet Fr 1223 was written under the same state-of-emergency duress that produced the features that non-pinacological scholars view as defective. Or one admits that the state-of-emergency scenario is not necessary to understand how Fr 1223 and Tn 316 came to be written as they are. I strongly endorse the second alternative.

Moreover, I also think that the idea that the text of Tn 316 was left incomplete is erroneous. This proceeds from two hypotheses: 1. that the god Poseidon, clearly the primary male divinity for the community of Pylos, should have been listed on the tablet; 2. that the fourth section on the verso should have been filled in with additional information not already found on the tablet. Neither hypothesis is valid. Study of the chronology of texts indicates that major activities concerning Poseidon were being undertaken or lay still in the future after Tn 316 had been archived in Room 8. This explains his absence here from ceremonies that may not have concerned him directly. He gets his, as it were, whether in the same month or in

19 E. Sikkenga remarked to me that even within societies (Hittite, Luwian) that, unlike the Mycenaean, had monumental inscriptions that would impose some ordered aesthetic on writing procedures, important texts often are incredibly sloppy by modern standards. One should always remember the mnemonic function of Linear B documents and their essentially short-term and preliminary nature.

20 See PALAIMA (*supra* n. 8), 38-40 and 98-101, for a discussion of Hand 1's experimentation with tablet size and formulae in the E- series at Pylos and Hand 41's in the Eb, Eo and Ep series.

earlier or later months, in tablets Un 718, the Es tablets, and other texts like Un 6, Un 853, and Fr 343 and Fr 1224. I also believe that the vacant section on the verso was originally intended to be used for a transfer of the information on the recto, an action which Hand 44 eventually and wisely reconsidered and abandoned. The text presented some confusion to Mycenologists at first regarding which side was recto and which was verso, but I doubt whether it would have much troubled the scribes for whom it was intended. They understood the transactional activities to which the text made reference. If for some reason interpretive problems arose, they could have called in Hand 44 for a clarification <sup>21</sup>.

Finally, the "human sacrifice" scenario is based in large part on the item *po-re-na* and its relationship to the ideograms for MAN and WOMAN that occur in the various sections of tablet Tn 316. In my opinion it is preferable to interpret the term not as "victims" but as human sacristans, "porteurs" dedicated to the cult of a god or goddess <sup>22</sup>. Tn 316 is an exceptional text in that it is a *unicum*. However, the offerings that occur on Tn 316 are not so costly that they must be explained as an extraordinary response to abnormal circumstances. The Palace of Nestor and its community were certainly capable of providing gold cups, bowls and chalices in such numbers to their gods on a regular basis. I shall present more detailed and systematic arguments in support of my view elsewhere.

Our view that a fair number of tablets were written in the time after Tn 316 was placed in Archives Room 8 is reinforced by a consideration of the tablets found in Room 7 (Pl. LXXIVa-b) and specifically the tablet-basket arrival area Grid 52. In Grid 52 were found tablets such as the armor inventory of the Sh series as well as tablets of the Es series which list offerings to Poseidon and other key human figures in the Pylos kingdom. In a special location in Room 7 (Grid 83) was found the prospective text Un 718 that records the offerings *to be made* to Poseidon by *e-ke-ra<sub>2</sub>-wo* who in my opinion is the king of Pylos <sup>23</sup>, by the military commander, by the *damos*, and by a group called the *worgioneion*. The quantities of offerings on Un 718 may have been calculated, at least in part, from the information on the earlier and already archived tablets: Er 312 and Er 880, which deal with landholdings of these persons and groups. Components of Er 312 and Tn 316 were found close together in Grid 23 in Room 8. The label for Un 718 was also found in Grid 52, thus indicating that Un 718 arrived in the Archives Complex shortly before the destruction of the palace.

21 On the ancient contextual understanding of Linear B tablets, cf. O. PANAGL, "Textual Features in the Linear B Tablets", in E. RISCH and H. MÜHLESTEIN eds., *Colloquium Mycenaeum* (1979), 313-322.

22 Compare later sacristans or devotees designated by compounds in -φορος: κληφόρος, λουτροφόρος, etc. Cf. N. MAURICE, "Analogie et flexion nominale en Grec mycénien", *Minos* 23 (1988), 122-137, and M. GÉRARD-ROUSSEAU, *Les mentions religieuses dans les tablettes mycéniennes* (1968), 176-179. The term occurs otherwise on TH Of 26 and PY Un 443 and Ua 1413. In no case, is it necessary or even more probable to interpret the term as "victim". I do not at all doubt that *po-re-si* in TH Of 26 is the same word as *po-re-na*. But wool in this series is given out: directly to the 'houses' of specified divinities, e.g., *di-u-ja-wo do-de* (Of 26.3) and *po-ti-ni-ja wo-ko-de* (Of 36.2 but here earmarked for cloth workers known as *a-ke-ti-ra<sub>2</sub>*) (cf. also Of 31 fragmentary and Of 33); to servants or cloth workers: *a-pi-qo-ro, a-ra-ka-te-ja* (Of 34) and *a-ke-ti-ra<sub>2</sub>, wa-na-ka* (Of 36.1); and directly to likely divinities: *ko-ma-we-te-ja* (Of 35) and *e-ma-a<sub>2</sub>* (Of 31.3?). 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. On Un 443 I interpret the sequence *po-re-no-zo-te-ri-ja* as "festival for the girding [clothing] of the 'bearers' ". For sacred officials to have special clothing and rituals of garbing is commonplace in ancient and modern religious practice.

23 See discussion of archival, scribal and linguistic factors for this conclusion in my paper "Writing in the Service of the King: Hand 24 and Normal vs. Special Mycenaean" delivered at the APA Annual Meeting (Washington, DC 12-29-93) with full references.

Un 718 takes rather exceptional care to use the future of the verb δίδωμι to describe the contributions to be made by important members of the community of Pylos<sup>24</sup> to the god Poseidon. The Es tablets, which still remained in Grid 52, and Un 718, from special tablet find spot 83, imply that major offerings to Poseidon were still in the process of being made when the palace was destroyed. As mentioned above, the absence of Poseidon, the chief god of the Pylian kingdom, from Tn 316 has been a heretofore unsolved puzzle<sup>25</sup>. Our view of the archival context indicates that his absence can best be explained chronologically. The chief offerings (festival?) of Poseidon took place separately and subsequent to the offerings made to the many other divinities listed on Tn 316. Thus some length of time (whether or not within the month of *po-ro-wi-to*) must have passed between the writing of Tn 316 and the texts (Es and Un 718) relating to Poseidon. Moreover, Un 718 is *prospective* and refers to an event still to come. Perhaps for this reason the tablet was retained in Grid 83. The scribal supervisor(s)/archivist(s) might have left it there awaiting confirmation that the contributions had indeed been made.

The number of month references in the Pylos tablets (3 or 4 or 5)<sup>26</sup> has been taken to reinforce the idea that the "month of sailing" in March-April and the events on Tn 316 mark the last days and activities of the palace. The seas were now open exposing Pylos in the final month of its existence to enemy attack<sup>27</sup>. However, this theory fails to take into account that the year in various regions and communities of Greece in historical times, so far as we have information about it, begins at the most conspicuous observable points of the natural year<sup>28</sup>: the spring equinox, the summer solstice, the first crescent moon following the fall equinox (rarely, but in Crete), and, in one known case, at the time of the winter solstice<sup>29</sup>. That is, the sufficiently documented ancient Greek calendars begin in late March-early April, late June-early July, or late September-October. It is exceptional that the historical Boeotian calendar begins in late December-early January<sup>30</sup>.

How does this affect our chronological thinking about the destruction of Pylos? If the calendar of Pylos behaved like the overwhelming majority of historical calendars, the beginning of the year would not have been in December-January. Thus it would not be possible for *po-ro-wi-to* (the "month of sailing") to be in March-April and be the last recorded

24 At least *e-ke-ra<sub>2</sub>-wo* and the *ra-wa-ke-ta* have *do-se* 'he shall give' specified. The contributions of the *da-mo* and the *wo-ro-ki-jo-ne-jo ka-ma* are introduced shorthand without a verbal form; but the first follows *e-ke-ra<sub>2</sub>-wo*'s entry and the second follows the *ra-wa-ke-ta*'s entry, so they should be taken to have *do-se* understood.

25 Emphasized once again by S. HILLER, "Zu PY Tn 316" (forthcoming).

26 *po-ro-wi-to* ('in the month of sailing'): Fr 1218, Fr 1221, Fr 1232, Tn 316. *me-tu-wo ne-wo* ('in the period of the festival of the new wine'): Fr 1202. *pa-ki-ja-ni-jo-jo me-no* ('in the month of *Sphagianes*', i.e., in a month derived from the name of a chief religious district): Fr 1224 (here the offering of oil is to Poseidon). *re-ke-e-to-ro-te-ri-jo* ('in the period of the festival of the strewing of the couch'): Fr 1217, Fr 343. TRÜMPY (*supra* n. 6), 203-204, classifies forms such as *di-pi-si-we-wi-jo* (Fr 1217) as festival names used with the purpose of temporal designation. But she concludes that *wa-na-se-wi-ja* in the PY Fr series could be taken as a month-name: 'the month of the festival of the \**wanas*(?)*a*' or festival name.

27 CHADWICK (*supra* n. 4). By this theory *po-ro-wi-to* must be the last month and all the other temporal references listed in n. 26 must precede it or fall within it.

28 For the starting points of calendars, particularly the Athenian calendar, see A. SAMUEL (*supra* n. 6), 64 and n. 1, 65-138. A convenient chart of eleven Greek regions and Babylonia, geared to the Athenian calendar, which begins in July-August, is provided in E.J. BICKERMAN, *Chronology of the Ancient World* (1968), 20, cf. also 21-39 for details. M. BLOMBERG, "*Minos Enneoros*. Archaeoastronomical Light on the Priestly Role of the King in Crete" (forthcoming), 7, started me thinking about calendrical issues.

29 According to a reference in Plutarch, *Pelop.* 24. SAMUEL (*supra* n. 6), 67.

30 Miletus: March-April; Delphi, Delos, Epidauros, Athens: July-August; Aetolia: September-October; Thessaly: August-September; Rhodes, Macedonia, Kos: October-November. Babylonia: April-May.

month in a string of month names. One alternative would be that the "month of sailing" on the already processed and stored tablet Tn 316 represents the *first* month in the official Pylian administrative year (in March-April as in later Miletus) which would then be followed by the other 2 or 3 or 4 months mentioned in the oil offering texts from Room 23. Tn 316 then cannot have been written in the final days of the Palace of Nestor.

But it is not even necessary that *po-ro-wi-to* refer to the month of the year in which sailing becomes possible. It could also represent the month that falls during the peak season for good and safe sailing. According to Hesiod this season is the 50-day period of the etesian winds *ca.* July 20 to September 10<sup>31</sup>. So we could fit the Linear B month references from Pylos into a sequence which would begin in July-August (as later at Delphi, Delos, Epidaurus and Athens) and continue through a series of months leading to, or including, the month of *me-tu-wo ne-wo* (the month of 'new wine'). This month should be within 6 weeks to 3 months after the grape harvest in late August-early September<sup>32</sup>. In any event, there is no convincing reason to maintain the traditional view that *po-ro-wi-to* 'the month of sailing' marks the third or fourth month of the year, beginning at the winter solstice, or that it is the last recorded month and the month in which the Palace of Nestor was destroyed<sup>33</sup>.

We should also note that three tablets from the oil storage area Room 23 of the Palace of Nestor refer to *po-ro-wi-to*. These tablets perhaps belong to as many as three different scribes of Class ii and they record allotments of sage-scented and anointing oil. The tablets in this area were not systematically stored<sup>34</sup> and there are no secure grounds for conjecturing whether the tablets referring to *po-ro-wi-to* were written before or after those from the same room that refer to 'the month of *Sphagianes*' during which Poseidon received an oil offering (Fr 1224) and 'the period of the festival of the strewing of the couch' (Fr 1217: written by one of the scribes who also wrote a 'sailing month' text). The 'period of the couch strewing' is also mentioned on a tablet from near Rooms 37 and 41, and the 'period of the festival of the new wine' on a tablet from Room 38.

Were it not for the problems just discussed, we could argue that the multiple references to *po-ro-wi-to* both in Room 23 and on Tn 316 from the main archives make it likely that it is the month in progress. However, given our general calendrical considerations, I prefer to think that the filing of Tn 316 and the single reference to the month of *Sphagianes* imply that the latter month was newly begun (hence the fewness of references) and the former month ('of sailing') was completed (hence the multiple references). Without explicit indication of the word *me-no*, it is hazardous to argue that the 'period of the festival of the new wine' or the 'period of the strewing of the couch' or the 'period of the festival of \**wanas*(?)*a*' are months in the proper sense and not merely festivals that occur within months explicitly identified<sup>35</sup>.

31 A.N. ATHANASSAKIS, *Hesiod Theogony, Works and Days*, Shield (1983), 105-106.

32 R. PALMER, *Wine in the Mycenaean Palace Economy* (1994), 64.

33 Cf. ROBKin (*supra* n. 15), 472, regarding seasonal implications from the flax production records. She believes that flax was sown in late September-early October and harvested after an 80-100 day growing season (mid-January). Harvesting, drying, rippling, retting, and preparing the fibers would take *ca.* 9 weeks: thus making the fibers ready for spinning by end of March. However, she also entertains the possibility that a later sowing could produce a late March harvest which would then make the flax ready for collection and linen cloth production 9 weeks later in early June. This latter alternative would fit either scenario sketched out above: 1. *po-ro-wi-to* is March/April and followed by some other months named in the extant texts from Pylos; 2. *po-ro-wi-to* is July/August and followed by some of the other months. The latter works for the 'festival of the new wine' and could also fit the flax tablets (series Na, Ng, Nn) which have no temporal designation and might refer to the month preceding *po-ro-wi-to*. We can have no confidence in pinpointing the month of *po-ro-wi-to* exactly.

34 For discussion, see PALAIMA (*supra* n. 8), 141-145.

35 Cf. *di-pi-si-je-wi-jo* on Fr 1218 which may be a festival name contrasting with the month name *po-ro-wi-to* on the same tablet.

It should be noted, however, that the word *me-no* at Pylos only accompanies 'the month of *Sphagianes*' and never *po-ro-wi-to*, which is most reasonably taken as a month name because of its position on Tn 316. The most we can say then is that we have one or two certain months and perhaps two or three more. We at least have shown three things. First, there is no substantial evidence to conclude in what season the month of *po-ro-wi-to* occurred, but July-August is the best possibility, March-April second best. Second, *po-ro-wi-to* is not likely to have been the final month recorded in the Pylos texts. Third, there is a continuum of recorded time after the events in the month of *po-ro-wi-to* on Tn 316.

Regarding the chronology of activities surrounding Poseidon, it is interesting to note that the *po-ro-wi-to* texts in the Fr oil series agree with Tn 316 in omitting him. Among tablets that refer to Poseidon, Fr 1221 refers to *wa-na-se-wi-ja*; Fr 1232 to *di-pi-si-jo-i*. On Fr 1224, explicitly identified as pertaining to the 'month of *Sphagianes*', an offering is made to Poseidon. The same is true of Fr 343, where the time specification is 'the period of the festival of the strewing of the couch'. Thus Poseidon's absence from Tn 316 may well be both ceremonially and chronologically motivated.

Finally, some attention should be paid to the relative chronology of references to the king of Pylos and his activities. These help to reinforce the conclusion that some time had elapsed between the filing of texts in Room 8 and the delivery of as yet unprocessed tablets to Room 7. These texts also hint that the cause for instability at Pylos at this time might be linked to a situation hazardous to any society, the period in which one ruler succeeds another<sup>36</sup>. Of particular concern here is the Ta series which lists an inventory of costly furniture and vessels on the occasion when the *wanax* appointed *au-ke-wa* to the office of *da-mo-ko-ro*<sup>37</sup>. The Ta series was found together with Un 718 in Grid 83 of Room 7.

We must consider tablet On 300<sup>38</sup>. On 300 is fragmentary, but it has enough preserved of the format in its two sections to suggest that the *da-mo-ko-ro* for the Further Province of Pylos at the time On 300 was written was an individual named *te-po-se-u* and that the slot for the same official in the Hither Province was filled only by the entry *da-mo-ko-ro*, the name of that *da-mo-ko-ro* being unspecified or unknown. This Hither Province entry served as a marker that the holder of the office of *da-mo-ko-ro* of the Hither Province was indeed involved with the commodity (\*154) recorded on On 300 and responsible for it, but that his name could not be furnished on this occasion. Assuming that the proposed internal parallelism is valid, there are several possible explanations for such a situation. First, this variation could be absolutely arbitrary and result from the recording process<sup>39</sup>. However, in all cases except *te-po-se-u* and *da-mo-ko-ro* the entity associated with a given entry is designated by ethnic or the titles *ko-re-te* and *du-ma*. Thus it would seem that the designation of the last entry of the Further Province (the *da-mo-ko-ro* slot) by a personal name is meaningful.

Perhaps we can offer an explanation based on internal chronology of the tablets. It seems as if for some reason a *da-mo-ko-ro* for the Hither Province *had not yet been appointed*. In the Further Province, *te-po-se-u*, who on Jo 438 is identified as the *ti-nwa-si-jo ko-re-te*, would be simultaneously the *da-mo-ko-ro* for the entire province. Now On 300 was found in many component fragments in Room 8 mainly in Grids 13 and 23, i.e., in the same

36 The topic is much too enormous to embark on here, but I shall be willing to discuss it with any correspondent and to send out a copy of my paper cited *supra* n. 23.

37 See discussion of the term *da-mo-ko-ro* in M. LINDGREN, *The People of Pylos* (1973) I, 32-33. In my opinion, the argument that the Ta series refers to funerary offerings has less to recommend it.

38 I thank P. Carlier for discussing tablet On 300 with me during his research stay at PASP in November-December, 1993.

39 Cf. Jo 438 where place names, official titles, and personal names alternate as entries much more randomly than on On 300. On Jo 438, place names are even listed in a random, non-canonical order.

area where Tn 316 was stored (Grid 23). It is possible that it was archived when the Hither Province had no *da-mo-ko-ro*. In Room 8 also are texts Jo 438 and An 192 which refer to *au-ke-wa*, but he is not marked out in these texts as being *da-mo-ko-ro*.

We can make sense of these textual references on An 192, Jo 438, On 300 and Ta 711 as follows. During the time represented by all three tablets from Room 8, *au-ke-wa* is an individual of prominence. He is mentioned by personal name on An 192 and Jo 438, but he is not *da-mo-ko-ro* of the Hither Province. The *da-mo-ko-ro* of the Hither province is removed from office by some means, *e.g.*, his death. For an interim there is no *da-mo-ko-ro*. The generic entry *da-mo-ko-ro* in On 300 reflects this situation: *i.e.*, it assigns responsibility to the temporarily unfilled office of *da-mo-ko-ro*. Then *au-ke-wa* is appointed the new *da-mo-ko-ro* by the king (Ta 711), and the inventory undertaken at his appointment is duly recorded and delivered to Room 7 and put in Grid 83 along with Un 718. Thus we would have evidence that a regular administrative act requiring bureaucratic oversight and record-keeping took place after the texts in Room 8 (including Tn 316 with which On 300 is contextually associated) had been duly filed.

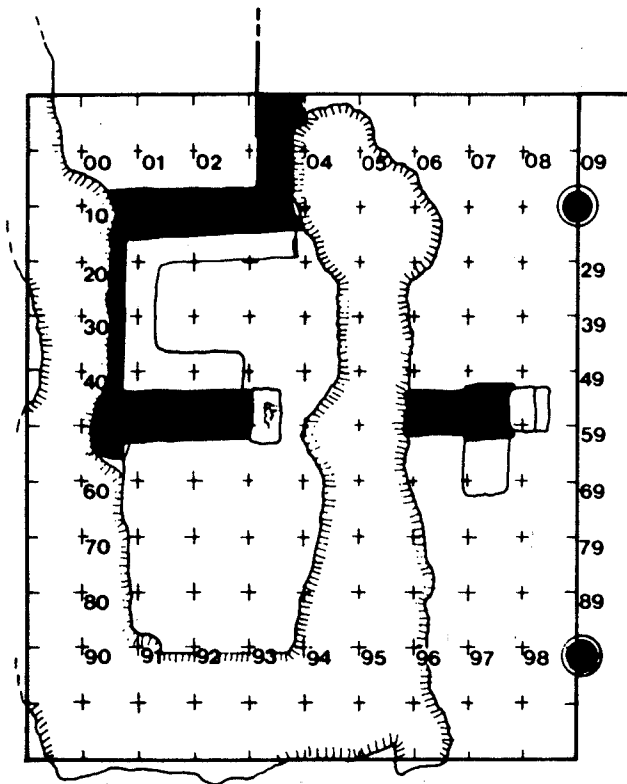
I have tried to make clear above how problematical it is to interpret the key texts pertinent to the last days of Pylos. It is essential to make use of all available evidence: archival, pinacological, linguistic, archaeological. This is especially so, since in interpreting the Linear B tablets, we shall always be dealing in relative probabilities. In sum, I hope to have made my arguments clear enough so that other scholars can identify problems and offer criticisms. Here then is what I think went on in the period before the Palace of Nestor was destroyed.

Golden vessels along with attendant human sacristans were offered to many of the chief deities of the community (and their associated minor deities) during the month of *po-ro-wi-to*, probably as a regular ceremony for this month in the Pylian calendar. The month of *po-ro-wi-to* is probably to be placed in July-August or March-April, but it is probably not the last chronological designation to be recorded before the palace was destroyed. The performance of these offerings was duly recorded on tablet Tn 316. Tn 316 is acceptable and unexceptional work by a scribe working within the Pylian administration. After the offerings made on Tn 316 were made and recorded and Tn 316 was filed in the Archives Complex, the palace at Pylos was busy with the following activities, of which its administrators kept what appear to be normal records: bringing armor into a finished state of repair (Sh series), current and prospective offerings to Poseidon by individuals (Es) and major figures and institutions in the community (Un 718), making other oil distributions (*e.g.*, Fr 1202 at the 'new wine festival' in September-October if *po-ro-wi-to* is July-August), and inventorying precious vessels and furniture following the appointment of a regional official (Ta series). Flax production was normal, and the recycling of 'temple' bronze might have been routine. Women workers were being fed as always. The offerings on Tn 316 then were not a *last* measure, nor were they a *desperate* measure. The causes and symptoms of the destruction of the Palace of Nestor remain unclear.

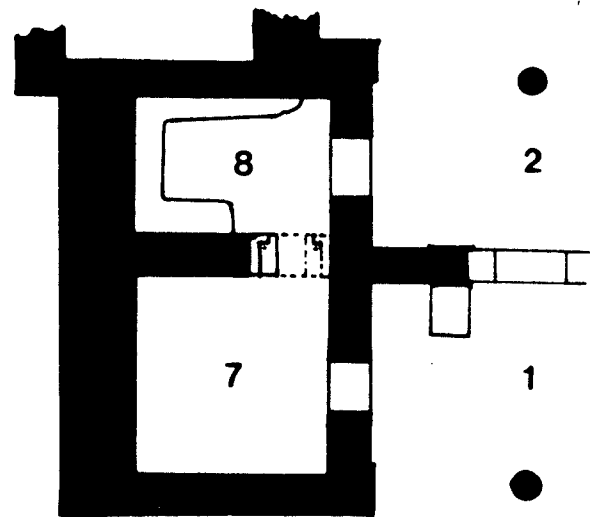
Thomas G. PALAIMA

## LIST OF ILLUSTRATIONS

- Pl. LXXIVa Palace of Nestor Archives Complex and Grid (after PALAIMA [*supra* n. 8], 184 fig. 25).  
Pl. LXXIVb Palace of Nestor Archives Complex Plan reconstructed by PALAIMA-WRIGHT (after PALAIMA [*supra* n. 8], 185 fig. 26b).  
Pl. LXXIVc Tn 316 obverse and reverse (after BENNETT [*supra* n. 17], 222 fig. 1).  
Pl. LXXIVd Fr 1223 (after E.L. BENNETT, Jr., *The Olive Oil Tablets of Pylos* [1958], plate XIII).

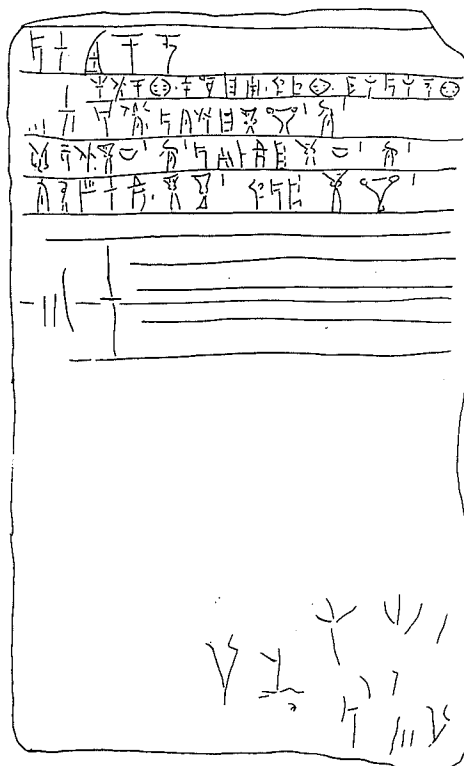


a

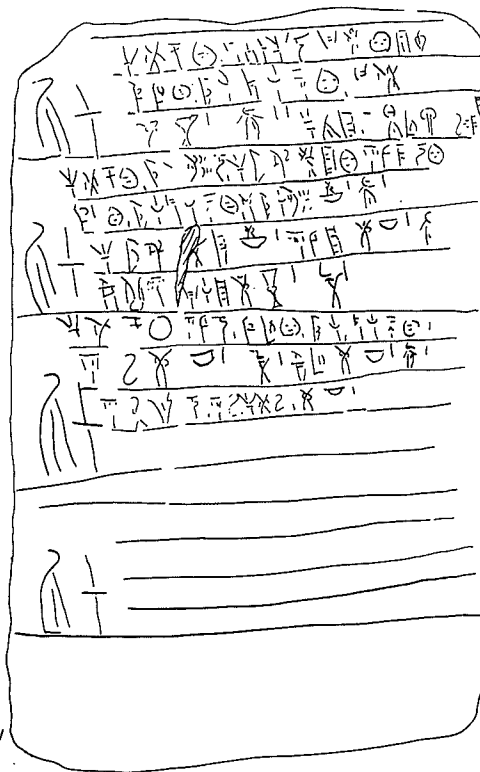


**b**  
Palaima-Wright

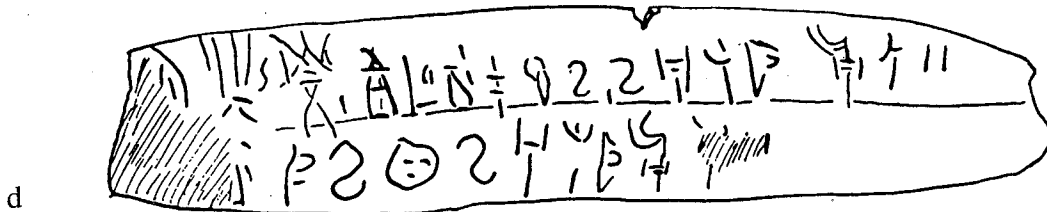
b



c



Tn 316  
obv rev



d

Fr1223

DISCUSSION ON J. BENNET'S, V. KAZANSKIENE'S, V. ARAVANTINOS'  
AND T.G. PALAIMA'S PAPERS

**C.G. Thomas:** I wanted to ask J. Bennet just for some clarification and that is in terms of the tombs. In Messenia no tholos tombs were constructed in LH IIIB. As to the tholos at Nichoria: I think that is a matter of interpretation that it might have been constructed when it was drawn into the Pylos orbit as you have suggested. W. Cavanagh and I were wondering if that could belong to stage 3 of your time-line, when there had been the coalition between the two later provinces. My other question is for T.G. Palaima: I certainly would agree with you in doing away the disasters, emergency compacts, perhaps particular tablets with human sacrifices, where people are scurrying for one and only time to various places on the coast because those sea-peoples were coming. But we should not ignore the archaeological indications that there were problems to which the palace was answering as J.C. Wright has demonstrated in the volume *Pylos comes alive* (1984).

**J. Bennet:** I agree that the tholos tombs are not actually constructed in LH IIIB, no. My point was that the numbers that continue in use in LH IIIB are very restricted. I do want to argue that their function at that stage within the overall picture is somewhat different. I think they are linked very closely to a small number of sites where they continue as elite burial places. At this time most strategies of elite display have been moved away from the tombs into the palace-structures themselves. As to C. Thomas' question about the tholos tomb at Nichoria: It was constructed in LH IIIA:2. I think it is too close - at least on our relatively coarse archaeological chronology - to separate the construction of the tholos tomb from the time when the megaron goes out of use. I hedged my bets a little bit on what I thought was happening at that stage when Nichoria was coming under the control of Ano Englianós or some other local center. I think those are a little too close together, perhaps, to reflect two separate shifts of power, though I do take your point.

**T.G. Palaima:** First, C.G. Thomas' question about what do we do about the archaeological indications that there were problems to which the palace was, in fact, responding architecturally over time. This is irrefutable. It is also indisputable that the palace was destroyed. Unless we opt for some disaster like an earthquake, we then have to come up with some other solution. I am not able to do this and I do not think the tablets can either. The point of my paper, though, was really to concentrate on the last days and to try to wrench our perspective of the tablets' information away from the way that this is prejudiced. I do this even though many of objections to the state of emergency hypothesis are not new. But with my wrenching us away from the absolute state of emergency we perhaps can get a better perspective over what responses were made. So as I mentioned, for example, let us take the Jn series. That shows allotments of small bits of bronze that have been scattered throughout the kingdom. I think it would be wiser to do that as part a long process, ten years, twenty years, of responding to the same problems that we see reflected in the architectural history, rather than to interpret it in the context of the last desperate attempt. The same with Jn 829. It is all too easy to say that here is an emergency operation in which the palace is calling in stuff from the temple sphere which ordinarily it would not call in. We have no understanding of how materials in the temple domain made out of this precious metal bronze would have been disposed of when they had lost their utility. In later times temples have different but regular ways of disposing of materials. And it seems to me that it would be quite understandable to have a natural recycling process of the metal to be reused. What is wrong with that? So long as you have the proper ceremonies to desanctify the dedicated metal objects. I do not think that they are preparing for a disaster which they expect to be coming. What has been thought to have been a sudden emergency, may in fact appear as a gradual emergency; it may appear over time.

**C.G. Thomas:** May it be called "gradual systems collapse"?

**T.G. Palaima:** This may be a good term.

**S. Deger-Jalkotzy:** I liked V. Kazanskiene's paper, because it was in itself methodologically consistent, although, as she may know, I hold a different position. But let us consider the text of PY An 607.3-4: *do-ge-ja, do-e-ra, e-ge-ta-i, e-e-to, te-re-te-we* MUL 13. I have never been able to interpret the word *te-re-te-we*. From Schwyzler (*Griechische Grammatik* I, 504-507) I learnt that the *nomina actionis* in -ti- (> -si-) may have had a very archaic earlier version in *-tus/*. So, if a word *tereo* in the sense of "to oversee" did, indeed, exist in Mycenaean Greek, it could be used for explaining *te-re-te-we*. On J. Bennet's paper: I very much like the way how you were able to demonstrate that the organisation of the kingdom of Pylos was based on an artificial administrative system which was again based on the artificial division of the territory into administrative centers or districts. I would like to ask you two questions. First, how do we have to correlate the term *dámos* within this system? Does it refer to a village or to a district? My second question concerns the tholos tombs which in the

core area of the kingdom of Pylos were used throughout the palace period, whereas in the peripheral areas (if you think e.g. of Peristeria) they went out of use at the beginning of that area. Do we have to correlate the tholoi of the core area with the palace, or were they related to some local communities or organizations?

**V. Kazanskiene:** Really I have to look at this very difficult and interesting text, PY AN 607: *me-ta-pa*, *ke-ri-mi-ja*, *do-qe-ja*, *ki-ri-te-wi-ja*. Before the *te-re-te-we* there is *e-qe-ta-i* and number 5 after the *te-re-te-we* is the number 13. That is not all right in this text. But it seems to be two different persons.

**J. Bennet:** I am not quite sure how to respond to S. Deger-Jalkotzy's first question about *dámos*. I wonder if perhaps the problem here is that we might be trying to apply a term which may in fact be limited to a phenomenon that exists only within the immediate region of the center itself at Ano Englianós, the *pu-ro* of the texts, because detailed land-tenure documents appear to be restricted to its immediate region, specifically *pa-ki-ja-ne*. So I would not want to speculate really on what these other communities might have been called or whether *da-mo* would have been an appropriate term for them. Secondly, I am not quite sure I understand your point about the tholos tombs. My impression is that, in fact, they are rather more widely spread in Messenia than one thinks. Although not all this information is fully published, I gather that there is a new tholos tomb over towards the North of Nichoria at a site called Strephi. There are some new ones even way down in the South of the peninsula. My point was that they are widespread; and then things change and a number of them continue in use in association with places that were closely associated with the administration. I do not see them as being just limited to the core area.

**G. Kopcke:** This is an observation, which is provoked by J. Bennet's contribution and in general by the investigations around Pylos as they traditionally are conducted. In light of what we do not have at Mycenae, is it important that somewhere there is comprehensive attention being paid to things other than citadels. We are, I feel, at this stage desperately in need of this kind of information. It is, of course, interesting to know now that around Pylos there is an ever greater concentration of population perhaps being attracted by the palace. What is difficult for me is to envisage the reality of this kind of life. I feel that in the long run it is not enough to add new dots on a map. I think it is, as I said, a question of prime importance. But it has to be followed up in some degree with tests that tell us: What do these dots mean? What does this increase in population mean in terms of the wider and more detailed archaeological picture? Otherwise we really do not learn anything about the people. We learn something about numbers, statistics. I am afraid they are in the long run of rather limited interest to the historian.

**J.C. Wright:** I would like to note that it is ten years since "Pylos Comes Alive"; and now we get the "Kingdom of Pylos Comes Alive". It has been very interesting and stimulating work and a real pleasure to see this co-ordination with the tablets. Is it the case that from the results so far reached that the distribution of the early Mycenaean sites represents the total, or more or less the total, infilling of the region of the future kingdom of Pylos, or is that something that is continuing down through the LH I and II. With respect to T.G. Palaima: I think we have to be really careful when we talk about the last days of the palace at Pylos. I never wanted people to think that the palace became in any way fortified, so much as they simply were trying to make access to it much more restricted. I was worried at the time about how far to push that. Although it is worth looking at the events that are happening throughout the LH IIIB period, the increased regionalisation of pottery that S. Sherratt has pointed to, that fortification of the other citadels, in addition to the extra workshops and so on and so forth. We still need to work on that. I think T.G. Palaima's idea of this being a more gradual thing is very helpful.

**J. Bennet:** In response to Jim: That is a very interesting question. I am afraid that our data are not quite at the stage when we can provide a definitive answer. What I would think I would stick my neck out and say is that we seem to have in the vicinities of some of these well-known, quite large sites smaller sites around them. What of course will be very interesting as we analyse the pottery of these will be whether these smaller sites exist already in LH I together with the larger sites, or whether they are in fact a new phenomenon in LH III. I really do not have an answer to that at the moment. It might be the former but I am not yet sure.

**S. Hiller:** To the paper of T.G. Palaima: Tn 316 is a single tablet, not belonging to a series. How long do you think would it take to turn a single tablet into an archive? If it is a single tablet, does it need a basket to be brought there; and if no, where would you put it there? My other problem is that you tell us that Poseidon will receive his gift at a later occasion, but as far as I can see these are totally other gifts. I think we have to distinguish between maybe two types of offerings. The routine offerings concern oil and honey and so on. To my mind Tn 316 remains an exceptional tablet because of its gifts. Maybe they were made every year, but since we also have from Knossos and also the other centers tablets, I think there is no comparable tablet. So something

special must be going on. The last question concerns the hand. In some way it seems to be a man who is not used to writing. He obviously has quite a problem with the layout of his tablet.

**T.G. Palaima:** I think there are about six points here, two of which I will have to think about seriously. The business with the layout is not so problematical for me. I think that if one looks at the work of many scribes, there is what I have called the tentativeness of formula principle. In many cases the scribes are presented with material which they do not know how to lay out correctly, for example in the Ea series at Pylos by hand 41 and even the Eb series by hand 43. A lot of times the information is being given in an oral fashion, and both J. Chadwick and L.R. Palmer have commented on this. In some cases these texts should be thought of not as anything that would ever necessarily have to get to the form of a fair copy; but rather they are simply records which contain information in a relatively conveniently accessible way. So I am not very much worried about the formatting business, even about Tn 316 with its various irregularities. The second question is very well taken. This is not something I really had considered, but you are quite right, that the texts of the Es series and texts of the Un 718, although they make offerings to Poseidon, are of quite a different sort from Tn 316. As far as the exceptional nature of Tn 316 in the entire collection of Mycenaean texts is concerned, I am also not so worried about it. As we know from Pylos, we at most have about six months covered. Again I would not call it an absolutely haphazard range but certainly a very selective view of what is going on. It is possible that a crucial text of the nature of Tn 316 with costly dedications to divinities could have been made in other months for other divinities in other areas of the kingdom and simply be processed very fast. What is happening with the tablets in Room 8, of course, is that they are sitting there until they can either be converted into parchment or papyrus for the information which has to stay for a longer period or are waiting for a final check by the administrator to make sure, for example, that the gold vessel store room really does have these particular vases missing. So its exceptional nature even within the entire corpus of Mycenaean texts does not really trouble me that much. But it certainly is of a different kind than these other offering texts. Along these lines in the break P.P. Betancourt brought up the even more remarkable problem. He said, "if I were sailing in the Mycenaean period, I would certainly never want to take to the sea without some offering to Poseidon". There is even a problem with *po-ro-wi-to-jo*. Does it mean "in the month of sailing"? We saw one of the texts yesterday from the Fr series. C. Trümper's chart that E. Stavrianopoulou showed us has *po-ro-wi-to-jo* in the category of recipients of oil. It does not occur with *me-no*. So this is my admission that there is a grim underbelly as there is to most Mycenaean arguments. But I think in the main the principle stands that one need not assume that the absence of Poseidon in this particular offering context is something crucial that needs to be taken into account as far as the incompleteness of the text is concerned. I have no doubt that this text is absolutely as complete as it needed to be at the time it was archived. On the problem how Tn 316 was archived: If it is a singleton, it is problematic. On the other hand we do have Un 718 which is brought in and laid to the left of the entrance door to Room 7. So this can perhaps be an indication that singleton texts would be laid there. We have the label for Un 718 in hand 24 and that sits in area 52. So I think the texts would be brought in and put in area 52 even if they were longer texts and singleton texts. Again it is something to be considered; and what one needs to do now is to look systematically at all these texts in this regard.