

Perhaps a parallel study of the material at Pylos possibly earlier than its final destruction would be revealing in that respect. There is, however, another possibility: that scribe H207 was also isolated. This is more difficult to prove, however, as (s)he left tablets in very common scribal areas and any attempt to show that his/her tablets are exceptional would be a very delicate and difficult operation.

The study of the roughness of hands has located a very small proportion of rough hands (manual workers?) preparing tablets. The fact that the number is very small could lend further support to our argument that the scribes were those who normally prepared the tablets. The hands that are not rough, moreover, may be scribes belonging to an administrative elite following the argument recently put forward by J. Bennet<sup>28</sup>.

Another interesting aspect of the study of Åström and Sjöquist is the recognition of children's hands. It seems that a great number of children prepared tablets for the "124" workshop. This, combined with the peculiar elements of that scribal workshop, could lead to many interesting hypotheses, which would depend on the dating of the workshop. Since these are mere speculations, I would rather not embark on an unfounded, though interesting discussion.

From the above, one can draw some conclusions. There seem to exist scribes who exchange tablets. They work together, have similar tablets and their specialisations are similar (which explains why they work together). With the help of palm and finger prints we have been able to deduce some working alliances which were not at first self-evident. There remain a few exceptions, however, of people who seem to work together, as they share tablets, but that has nothing to do with their specialisation or with their bureau (notably H141 and H110). In other words, their affiliation, no matter how weak it is, has to be explained in other terms. Moreover, although it is quite conceivable that several minor scribes gave their tablets to more important and busy ones, that will have to remain in the sphere of speculation.

CB2 ITP Cambridge, U.K.  
St John's College

EVANGELOS KYRIAKIDIS

<sup>28</sup> J. Bennet, «Agency and Bureaucracy. Thoughts on the Nature and Extent of the Pylos Administration», in J.T. Killen and S. Voutsaki (eds), *Economy and politics in Mycenaean palace states*, Cambridge 2000, pp. 25-37.

## SPECIAL VS. NORMAL MYCENAEAN HAND 24 AND WRITING IN THE SERVICE OF THE KING?\*

My paper here concerns the relationship between the written and the spoken word within the narrowly defined literate administrative record-keeping systems of Mycenaean palatial centers and will focus on some questions connected with socio-linguistic stratification and information-gathering. John Killen has contributed significantly to our understanding of both these topics, most recently through his work at reconstructing how primary information recorded on inscribed nodules relates to information compiled on tablets that record contributions to commensal banqueting ceremonies<sup>1</sup>. These topics also relate to the current reappraisal of the evidence for the existence of distinctive dialects within the Mycenaean corpus and how that evidence is best explained<sup>2</sup>. Mycenaean scribes have been called 'literate functionaries' — correctly, I think, since I am one of those who have called them that<sup>3</sup> — but we still do not know from what social classes those who wrote our extant records were drawn and whether any of the tablet-writers were identical with the officials or other parties who helped manage Mycenaean economic and other activities: *ko-re-te-re*, *po-ro-ko-re-te-re*, *qa-si-re-*

\* This paper began as a contribution to a session on the written word (ancient Greek and Latin scripts, languages, epigraphy and literacy) organised by Roger Woodard at the annual meetings of the American Philological Association in December 1993. I thank José Melena and Ted Somerville for discussing details of this paper with me. Any faults remaining herein are solely mine.

<sup>1</sup> In fact PY Un 718 by Hand 24 is one such commensal banqueting document. Cf. J.T. Killen, «Observations on the Thebes Sealings», *Mykenaiika*, pp. 365-380, esp. p. 369.

<sup>2</sup> Cf. R.J.E. Thompson, «Dialects in Mycenaean and Mycenaean among the Dialects», *Minos* 31-32, 1996-1997, pp. 313-333; Y. Duhoux, «Le mycénien connaissait-il la tmèse?» *Minos* 29-30, 1994-1995, pp. 177-186; C. Varias, «A Tentative Analysis of Dialectal Differences in the Linear B Texts from Mycenae», *Minos* 29-30, 1994-1995, pp. 135-157. The reader of the article by Varias will find references to four articles by John Killen that are essential for the correct interpretation of the inscriptions from Mycenae.

<sup>3</sup> T.G. Palaima, «Appendix», in K.-E. Sjöquist and P. Åström, *Pylos: Palmprints and Palmleaves*, Göteborg, 1985, p. 106.

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*we, du-ma-te*, 'collectors', or those named as performing inspections, e.g., *pu<sub>2</sub>-ke-qi-ri* on PY Ta 711<sup>4</sup>. Here we shall be looking at the work of several scribes from the site of Pylos to whom some of the most important subjects in the preserved records were entrusted. We might consider these particular scribes 'high-level literate functionaries'.

Fourteen years after the decipherment of Linear B, the late Ernst Risch<sup>5</sup> identified what he considered to be traces of two distinct dialects in the corpus of Mycenaean texts as then represented on tablets from Pylos, Knossos, Thebes and Mycenae. Risch isolated three alternations and classified them according to the frequency and prevalence with which they occurred. In each case, the variant that occurred most commonly was considered *normal* or *standard*. The rarer variant was termed *special*, i.e., somehow *non-standard*. Risch's designations (*normal, special*) and my glosses of them (*standard, non-standard*) are important and we shall return to them—and the assumptions that underlie them—later. The FEATURES that Risch detected are:

#### FEATURE 1.

The athematic dative singular spelled with *-e* (normal)<sup>6</sup> vs. *-i* (special): theonym *po-se-da-o-ne* (Es tablets Hand 1; Fr 1224 Hand 2; Fr 343 Hand 4) vs. *po-se-da-o-ni* (Un 718 Hand 24); other instances of special Mycenaean: anthroponym *me-za-wo-ni* (Un 138.5 Hand 42); titles *ko-re-te-ri* (On 300.3, .5 and On 1074 S 300-Cii) and *du-ma-ti* (On 300.6 S 300-Cii) vs. normal *po-ro-du-ma-te* (Fn 50.7 Ciii).

#### FEATURE 2.

The reflex of IE sonant nasals in the environment of bilabial consonants (*m, w, p*): *o* (normal) vs. *a* (special): most conspicuously *pe-mo* (over 200 times in the En, Eb, Ep, Eo tablets of Hands 1, 41 and Es 650 by Hand 11) vs. *pe-ma* (5 times in Er 312, 880 by Hand 24)<sup>7</sup>; σπέρμιο vs. σπέρμια; contrast the universal use of *a-mo* = ἄρμιο = 'wheel' in many hands at KN and PY.

<sup>4</sup> This last issue is examined in detail in T.G. Palaima, «Inside the Mind of a Mycenaean 'Scribe': How Hand 2 Wrote the Pylos Ta Series», a paper presented at the conference *Jornadas Micénicas* organised by F. Aura Jorro and held at the Universities of Alicante and Orihuela, Spain, February 17-19, 1999.

<sup>5</sup> «Les différences dialectales dans le mycénien», *Cambridge Colloquium*, pp. 150-157.

<sup>6</sup> Old I.-E. form of dative: *-ei*.

<sup>7</sup> Old I.-E. form of locative *-i*, used almost universally in historical Greek for dative.

<sup>8</sup> *pe-mo* found at Tiryns on Ef 2; *pe-ma* found at Knossos on E 1569.1, Ga 674.a, Ga 675, Ga 680.1a, X 8674.b. For an up-to-date analysis of the data, cf. Thompson *supra*, n. 2, 315-324 and Tables 1a and 1b.

#### FEATURE 3.

In certain words, the occurrence of *i* (normal) vs. *e* (special) in the environment of a labial consonant (*m, p, kw*): toponym *ti-mi-ti-ja* vs. *te-mi-ti-ja* (cf. *ti-mi-to a-ke-i*); theonym *a-ti-mi-te* (Un 219 Hand 15) vs. *a-te-mi-to* (Es 650 Hand 11); universal normal Mycenaean vocabulary items: *di-pa* (Ta 641 Hand 2) vs. Homeric δέπας, *i-qa* (Ta 722.1 Hand 2), *i-qa-jo* (Eq 59.5 Hand 43), *i-po-po-qa-i* (Fn 79.10 Hand 45), *i-qa-ja* (An 1281.1 Hand 12).

In the last thirty-three years the publication of better and fuller editions of the texts available to Risch and the discovery of new texts—including some at other sites: Tiryns, Midea, Thebes and Khania—have not significantly changed these data. However, there have been improvements over time in the sophistication with which these features have been analysed. Risch himself wrote a masterful update<sup>9</sup> which exploited the preliminary identifications of scribal hands within the Pylos corpus by E.L. Bennett, Jr. and J.-P. Olivier<sup>10</sup>. Roger Woodard<sup>11</sup> has studied the patterns of scribal usage at Knossos in order to see how many scribes exhibit *special* or *normal* features and whether scribes with like features cluster in particular administrative departments or bureaux as identified by Olivier<sup>12</sup> at the site.

Woodard also tested a fourth possible diagnostic alternation, the validity of which for the classification of *normal* vs. *special* dialect has been debated<sup>13</sup>:

#### FEATURE 4.

Assibilated *si* (normal) vs. unassibilated *ti* (special).

G. Nagy<sup>14</sup> thought that the relatively few instances of unassibilated spellings in the texts could be attributed to the same *special* dialect that employed consonant declension dative *-i*, *a* reflex of sonant nasals, and *e* vs. *i* in certain spellings. But Risch<sup>15</sup> categorically rejected this as a tool for distinguishing the two dialects. It could be pointed out that such unassibilated spellings were rare and limited to ethnics, anthroponyms, and toponymic adjectives, where the

<sup>9</sup> E. Risch, «Die griechischen Dialekte in 2. vorchristlichen Jahrtausend», *SMEA* 20, 1979, pp. 91-111.

<sup>10</sup> *PTT* II.

<sup>11</sup> R. Woodard, «Dialectal Differences at Knossos», *Kadmos* 25, 1986, pp. 49-74.

<sup>12</sup> *Scribes Knossos*.

<sup>13</sup> E.g., Thompson *supra*, n. 2, pp. 313 and 315, follows Risch in considering the isoglosses for lack of assibilation of *-ti-* and the alternation of spellings *e ~ i* to be 'phantoms' for purposes of identifying true dialects.

<sup>14</sup> G. Nagy, «On Dialectal Anomalies in Pylian Texts», *Atti Roma*, pp. 667-676.

<sup>15</sup> Risch *supra*, n. 9, p. 102.

possibility of the idiosyncratic or artificial preservation of traditional non-Greek or non-Hellenized spellings — rather than a true dialect choice — could not be dismissed. The unassibilated spellings also did not seem to co-occur with the other three features. Finally by this point Risch was also arguing against the proposal by the late John Chadwick<sup>16</sup> that *special* Mycenaean was a kind of proto-Doric, for which theory the unassibilated forms offered the most support.

In his update, Risch himself, however, used the presence or absence of forms displaying liquid metathesis as an analytical tool for testing the degree to which individual scribes were *normal* or *special*. He considered spellings such as *to-no* (*thornos*) to be *normal* and spellings such as *to-ro-no* (*thronos*) to be *special*. We might call this FEATURE 5.

#### FEATURE 5.

Spelling *to-no* (*thornos*) (normal) vs. *to-ro-no* (*thronos*) (special).

Woodard saw some support for using the unassibilated forms as characteristic of the *special* dialect in the fact that such forms occur most conspicuously at Knossos in the texts of scribes who work in Specialized Department II where other scribes — but not those whose texts contain *ti*-forms — use FEATURES 1 and 2.

Where do we go from here? One main point is agreed upon. There are *no* scribes who write purely in *special* Mycenaean, whether narrowly (FEATURES 1-3) or broadly (FEATURES 1-5) defined. Moreover, as Risch<sup>17</sup> has pointed out, none of the scribes whose work contains the supposedly 'Doric' forms (including the special *-ti*-forms like *ti-mi-ti-ja*) shows any other typical Doric features (e.g., *\*i-ja-ro*, *\*a-ta-mi*, etc.) and in fact their work shows strongly non-Doric forms such as *ra-wa-ke-si-jo* (not *-ti-jo*) and *o-te* (not *\*o-ka*) and common verbal forms in *-si*, not *-ti*, like *do-so-si* and *e-ko-si* etc. More problematical than the mere identification and evaluation of these diagnostic features has been deciding what exactly they diagnose. Because of the extremely restricted nature of Mycenaean literacy<sup>18</sup> and the *palatial* nature of our documentation, it has been natural to view the *normal* Mycenaean as a chancellery dialect, i.e., a kind of *hoch* Greek spoken by trained professionals and administrators within the orbits of the major palatial centres. This thesis attempts to account for the overwhelming homogeneity of dialect features at sites that are widely separated geographically — and to some degree even chronologically — and are located in regions which in later times showed distinctive dialect differentiation. If we accept this thesis, then

<sup>16</sup> J. Chadwick, «Who Were the Dorians?», *PP* 31, 1976, pp. 112-114.

<sup>17</sup> Risch *supra*, n. 9, pp. 102-103 and chart on p. 111.

<sup>18</sup> T.G. Palaima, «Comments on Mycenaean Literacy», *Studies Chadwick*, pp. 499-510.

the potentially *special* Mycenaean features — and let us include here FEATURES 4 and 5 in order to be comprehensive — become *non-standard* or in fact *sub-standard*, i.e., they might represent the occasional contamination of the uniform chancellery dialect by the *lower-class* or at least the 'extramural' dialect of the non-palatial segment of the population<sup>19</sup>.

The rarity with which the *special* Mycenaean forms occur notwithstanding, there are grounds for viewing the situation in this way. From what we know of the data-gathering process that produced the surviving Linear B records, the tablet-writing administrators were working regularly with information from outside the immediate palatial environs. There are clear instances of:

1. oral-aural *mistakes* and *corrections* in the texts (cf. studies of such phenomena by J.-L. Perpillou and N. Maurice<sup>20</sup>, and see below on *i-pa-sa-na-ti* vs. *e-pa-sa-na-ti*);

2. single entry records (especially series C-, D- and E- at Knossos and Pylos) that would have required at some point in the information-gathering process interaction with, for example, shepherds and/or collectors and landholders, *telestai*, and religious officials in specific religious districts;

3. lengthy page-shaped tablets focused on the activities of key regional administrators and important transactions (e.g., PY Jo 438: which deals with ca 5 extant kilograms of gold registered according to toponyms that are among the 9 and 7 major districts in Bronze Age Messenia and according to individuals and local officials, both palatial and non-palatial, like the *ko-re-te*, *po-ro-ko-re-te*, *mo-ro-qa* and *qa-si-re-u*) that, because of the lack of systematic arrangement in the listing of toponyms and official titles, appear to be the end results of some kind of *simultaneous recording*;

<sup>19</sup> Chadwick, «Traditional Spelling or Two Dialects», *Res Mycenaee*, pp. 78-87, esp. p. 86, later made a virtue of necessity by arguing that the very failure of any scribes to display all the *non-standard* dialect features or entirely *non-standard* dialect features is "precisely what we should expect if the scribe[s] in fact spoke two dialects; the palace dialect would be his normal standard, but constantly the other dialect would intrude". In arguing (p. 87) that "West Greek will continue basically the substandard speech, modified perhaps by the absorption of other elements, if in fact northern barbarians penetrated southern Greece in the Dark Age", Chadwick did not address Risch's most salient criticism, namely that the *non-standard* features are not particularly Doric or West Greek. Also now Thompson's complete statistical breakdown of the evidence for the reflexes of syllabic nasals in labial environments at Pylos (*supra*, n. 2, p. 323, Table 1a) shows how rare the attestations of either treatment are and how the Pylos E- series documents skew our identification of what treatment is 'normal' and what form is 'special'.

<sup>20</sup> J.-L. Perpillou, «Repentirs de scribes», *RPhil* 51, 1977, pp. 237-248; N. Maurice, «Fautes de scribes. Pour une critique verbale appliquée aux textes mycéniens», *Minos* 19, 1985, pp. 29-50, esp. pp. 33-36.

4. final recensions that are based on preliminary documents of the sort described in point 2 (e.g., in the Pylos En and Ep texts Hand 1 seems to have worked from information furnished in the Eb and Eo records of Hand 41 that might even have been transmitted orally to Hand 1<sup>21</sup>; and the PY Cn records might be compilations drawn from single-entry leaf-shaped records akin to those of the KN D- series);

5. compilations of preliminary information from sealings onto tablets (series Wu at Thebes and the Pylos tablets Un 2, Un 47 and Un 138)<sup>22</sup>;

6. reports of 'inspections', such as that of the 'collector' *a-ko-so-ta* on PY Eq 213, that imply a flow of 'outside' information into the palatial orbit<sup>23</sup>.

I would say that virtually no scribe would have been immune from the influence, direct or indirect, of *extra-palatial* language. Unless the scribes themselves (70+ at Knossos, ca 30 at Pylos, ca 4 in the published Thebes tablets and 10 in the Thebes sealings, and ca 14 at Mycenae<sup>24</sup>) were drawn entirely from a restricted social class within their respective palatial territories, we would also have to reckon with their own natural speech habits and patterns for which *their writing* the characteristics of the *normal* Mycenaean dialect would have been substituted. Their natural speech should creep into their texts occasionally, unless scribal training in the area of standard spellings is much more rigorous than has been thought. For example, in very casual moments I am liable to slip into my natural speech habits acquired in a second-generation Lithuanian-Polish immigrant family in Cleveland, whereby the word 'mirror' is *pronounced* indistinguishably from 'mere' and 'creek' is *pronounced* to rhyme with 'trick'. But I was well educated by Catholic nuns and Jesuit priests in standard orthodox spelling so that I would *never* spell 'mirror' as a monosyllable, no matter how casual the message I was writing.

<sup>21</sup> But see the case of *e-pa-sa-na-ti* and *i-pa-sa-na-ti* discussed *infra*, in nn. 45 and 47.

<sup>22</sup> See C. Piteros, J.-P. Olivier and J.L. Melena, «Les inscriptions en linéaire B des nodules de Thèbes (1982): la fouille, les documents, les possibilités d'interprétation», *BCH* 114, 1990, 103-184, esp. 166-183. For the relationship between sealings and tablets at different stages of the record-keeping and administrative process, cf. T.G. Palaima, «The Transactional Vocabulary of Mycenaean Sealings and the Mycenaean Administrative Process», in M. Perna (ed.), *Administrative Documents in the Aegean and Their Near Eastern Counterparts*, Turin 2000, pp. 261-276.

<sup>23</sup> The landholding information on texts such as Er 312 and 880 and the Ea and Eb sets had to be acquired through some process of interaction with individuals outside the palace, although the very fact that the individual landholdings were recorded on tablets at the palatial centre indicates that the 'holders' of the land might themselves have had in some way an association with the palatial linguistic milieu.

<sup>24</sup> Attributions taken from *TITHEMY*.

It is fair to point out, too, that this raises a double-edged major problem in assessing the evidence with which we are here dealing. First, if we are truly dealing with different dialects *AND* there was no rigid training in orthography, I would expect *special* Mycenaean to be much better attested than it is (although I do not know how I would go about quantifying this scholarly intuition of mine). Conversely, we cannot dismiss the theory of two main dialects without taking into account the influence that comprehensive scribal training in standardised spelling (commonly attested in lexical lists and school texts from the Near and Middle East) might have had in skewing the data, i.e., suppressing traces of the normal speech habits of the larger population (= *special* Mycenaean)<sup>25</sup>. I have often wondered whether the inculcation of canonical spelling in Mycenaean scribal education explains why *special Mycenaean* FEATURES 3 and 4 show up mostly in personal and place names and ethnics. Many such proper names would not be part of scribal 'primers', and so the scribes might have to spell them 'on their own', as it were. Such lexical items, along with highly technical loan words, would be the one class of words wherein the pressures of learned spellings and learned spelling rules might not come into play as much<sup>26</sup>.

We should also recognise that Chadwick tried to push the notion of *standard* chancellery dialect vs. *sub-standard* extra-palatial dialect too far. He wanted *special* Mycenaean to be proto-Doric in order to explain why archaeologists

<sup>25</sup> Thompson *supra*, n. 2, p. 313, remarks that *special Mycenaean* "looks like a 'normal' dialect, and [*normal* Mycenaean] is idiosyncratic". Thompson, p. 315, however, bases this observation entirely on FEATURES 1 and 2, for he rejects FEATURES 3 and 4 as valid evidence and nowhere addresses feature 5. He reasons that feature 3 concerns words of no convincing Greek etymology "which are in a number of cases demonstrably foreign borrowings". The *-ti-* forms (feature 4) "are all ethnics and anthroponyms". For the ethnics the lack of assimilation might be "by analogy with the toponym where there is no front vowel to condition assimilation". We should point out, however, that feature 3 does include some universal vocabulary items like *i-go* and that the fluctuation in the text of Hand 41 of spelling first with *e-* and then with *i-* indicates a conscious choice in phonetic representation that might be governed by the habitual auditory perceptions and speech patterns of the individual scribes. Re feature 4, is it not also possible for dialects to vary in applying and not applying analogy, e.g., Attic acc. plur. *vaῖς* (by analogy) vs. Doric and Hdt. *vāas* and *vēas* respectively? If all five potential features are taken into account, *special* Mycenaean still looks more 'normal' in 4 of 5 cases, if 'normality' is defined as looking very much like historical and Attic Greek. If *normal* were defined by looking like historical Arcado-Cypriote (and cf. the *i-* treatment of *e-* in lexemes like *év*), the opposite would be true. Of course, *-i* is prevalent in consonant stem datives of the historical dialects except in rare forms like compound names exhibiting *Diwei-*.

<sup>26</sup> See below on the many different ways in which individual scribes spell the name *e-ke-ra<sub>2</sub>-wo*.

have trouble documenting the arrival of Dorian speakers by means of material evidence. If the Dorians were in fact the silent *lower-class Volk*, whose speech patterns occasionally crept into the palace records, then they need not have arrived in post-palatial times. They would have already been in place throughout the Mycenaean and later Greek world, and they would have formed the bulk of the Peloponnesian population—and one assumes also the non-Minoan segment of the Cretan population—after the collapse of the mainland palatial system at the end of LH IIIB and the subsequent reversion to a more village-based system of social organisation.

The direction of research on these complicated issues then has followed a normal pattern for Mycenaean studies: initial observation based on an assessment of the general data (Risch 1966) followed by some refinement of or expansion upon the observation (Nagy 1968; Risch 1979) and then followed by further analysis of particularities (Risch 1979 taking more detailed account of scribal hands; Woodard 1986 considering not only hands, but their bureaucratic groupings; Varias 1994-95 studying the data from Mycenae from a global perspective), with some attempt to draw broader socio-historical conclusions (Chadwick 1976 and 1983; Duhoux 1994-95). With the recent work of Thompson 1996-97 (1998) and with this paper, we have moved into the phase of reevaluating the original approach to interpreting the evidence.

We can also test the assumptions and current results of this work by looking more closely at the work of individual scribes. This can, and indeed should, be done for all scribes, especially for the major Hands 1 and 41 at Pylos who are so closely involved in Woodard's conclusions about the significance of unassibilated forms (FEATURE 4). But here we shall concentrate mainly on the work of the scribe who shows most clearly not only *special* Mycenaean features but also other peculiarities of writing and spelling: Hand 24<sup>27</sup>.

The work of Hand 24 raises many questions about the explanation of *special* Mycenaean forms as the result of social layering and contamination of a palatial dialect. I should first make several general remarks that have a bearing on our interpretation of the data. At Pylos we have, with the possible exception of four earlier and palaeographically distinctive documents (assigned now to Hand 91 and Civ), a collection of some 1109 tablets or fragments, that represent the work of at least twenty-five or, more likely, but still conservatively estimated, thirty-two identifiable scribes<sup>28</sup>. The tablets date from a single administrative period,

<sup>27</sup> On Un 718, see T.G. Palaima, «The Last Days of the Pylos Polity», *Politeia*, pp. 631-633.

<sup>28</sup> *Scribes Pylos*, 111-113, 133, 171-177. The traditional number of 'tablets' is used here, adding the inscribed material presented in C.W. Shelmerdine and J. Bennet, «Two New Linear B Documents from Bronze Age Pylos», *Kadmos* 34, 1995, pp. 123-136.

perhaps a matter of months<sup>29</sup>, certainly no more than a single year, immediately preceding the destruction of the Palace of Nestor complex. The scribes of the 1109 tablets and fragments found in the final destruction stratum can be assigned with reasonable certainty to one of three general palaeographical schools, now identified, for reasons that are more than arbitrary, by the handwriting styles of the single most significant scribe in each class. It should also be noted that these distinct classes most likely correspond in some way to how the record-keepers were trained in the art of writing and administration. This is seen much more clearly at Knossos where the size of the palatial centre, and perhaps also the size of the territories under the control of the palace, made departmentalisation and instruction within departments and bureaux the norm<sup>30</sup>. As we have remarked above, we do not know whether scribal training included rigid instruction in traditional spelling of words (as opposed to basic principles of syllable representation)<sup>31</sup>, but this would be one way of explaining the overwhelming uniformity in the graphic representation of words that, according to the Risch theory, we explain as a uniform *normal* dialect. It is clear that the scribes laboured under no hard and fast regulations for formatting and presentation of information. In formatting and text-presentation, tablets at all sites show enough variety to support the conclusion that individual scribes had a good degree of freedom within the standard categories of tablet shapes and simple textual layouts.

The ongoing work of J.L. Melena has greatly reduced in a truly spectacular way the overall number of fragments at Pylos through joins with existing tablets. This work has been greatly facilitated over the last five years by the directorate and staff of the National Archaeological Museum in Athens (Dr. K. Demakopoulou, Maria Vlassopoulou, Dimitra Kokkevi, Lena Papazoglou-Manioudaki and Kostas Pantazis) who are owed the greatest thanks for making possible such significant advances in the reading of the Pylos tablets. The tablets of Hand 91 and Civ are now reduced from five to four by the join made between tablet Xn 1449 and Vn 1339 of Ciii. Cf. J.L. Melena, «40 Joins and Quasi-Joins of Fragments in the Linear B tablets from Pylos», *Minos* 31-32, 1996-97, pp. 165-167. Xn 1449 was of unknown findspot and of its two signs, the *so* was highly peculiar, but the *ne* lacked the circular element that was distinctive of 'Hand 91'. It is now clear that this fragment came from the NE Workshop and is part of the work with *a-ko-so-ne* recorded there and on tablet Vn 10. Its reassignment does not undercut the validity of assigning the four other fragments to Hand 91 or Civ, but it does remind me to be less categorical in statements about matters hypothetical.

<sup>29</sup> *Scribes Pylos*, pp. 629-630 and n. 26.

<sup>30</sup> Cf. now J. Driessen, «Le palais de Cnossos au MR II-III : Combien de destructions?», *Crète mycénienne*, pp. 113-134.

<sup>31</sup> Cf. R.G. Woodard, *Greek Writing from Knossos to Homer*, New York and Oxford, 1997, pp. 112-132.

Hand 24 shows two clear narrowly defined *special* features and one more broadly defined special feature. These are as follows:

FEATURE 1: *i* in dative singular: *po-se-da-o-ni* (Un 718.1)<sup>32</sup>;

FEATURE 2: *a* treatment of sonant nasals: *pe-ma* (Er 312.2, .5, .8, Er 880.4, .8)<sup>33</sup>;

FEATURE 5: unmetathesised forms: *ku-su-to-ro-qa* (Er 880.8 shared with KN B 817 and PY Ed 411.1 of Hand 41 who elsewhere writes *wo-ze* commonly, not unattested *\*wo-ro-ze*) and perhaps, depending on etymological reconstruction, *wo-ro-ki-jo-ne-jo* (Er 312.7 and Un 718.11)<sup>34</sup>.

Hand 24 also exhibits other peculiarities, such as the much-debated form *to-so-jo* in the phrase *to-so-jo pe-ma* in Er 312.2 and .8<sup>35</sup>. To make matters more complicated, the scribe uses *to-so* probably on Er 312.5 and definitely on Er 880.3, .8 and Un 718.2 (*to-so-de* on Er 312.6, Er 880.4, and Un 718.9). Hand 24 also shares a special spelling of the anthroponym *e-ke-ra<sub>2</sub>-wo* (nom. sing. Er 880.1 and Un 718.2) with Hand 1 (*e-ke-ra<sub>2</sub>-wo-no* gen. sing.: An 610.13 and *e-ke-ra<sub>2</sub>-wo-ne* plene spelling with *e* of the nom. sing.: An 724.5) over against *e|ke-ri-ja-wo* (nom. sing.: Hand 15: Qa 1292)<sup>36</sup> and more stunningly *e|ke-ra<sub>2</sub>-u-na* (plene spelling with *a* of the nom. sing.?: Hand 6: Un 853.1) and *e-ke-ra-ne* (dat. sing.: Hand 15: Un 219.1, perhaps to be restored as *e-ke-ra-<wo>-ne*, although Leukart, wrongly it seems [see below], would view this form as a syncopated spelling using one sign per syllable: *He.khe[l].lau.nei*)<sup>37</sup>.

<sup>32</sup> For a tabular analysis of this feature at Pylos, cf. Thompson *supra*, n. 2, p. 328, Table 2.

<sup>33</sup> The monographic ideogram *AREPA* (Un 718.8) is attested elsewhere at Pylos: PY Fr 1198 Hand 2; Un 853.4 Hand 6; Wr 1437 an unassigned sealing from oil magazine Room 24. According to C.J. Ruijgh (*Atti Roma*, p. 813), the normal nominative of this word would be *aleiphor* (from sonant *r*) and the oblique and compositional form (from sonant *n*) would be *aleiphat-*. Instances of compositional forms with *-o-* would be by analogy with the nominative. Instances of nominative in *-a-* would be by analogy with the oblique cases. If this elaborate explanation via bidirectional leveling of the forms were correct, the monographic ideogram would be indirect evidence that Hand 24 could write in some cases a form that ultimately derived from *a*-treatment of sonant *n*. However, it is likely that this monographic ideogram was standard currency among scribes and might no longer be viewed phonetically. Risch's citation (*supra*, n. 9, p. 100) of the form *ko-wo* (historical Greek κῶας) as relevant here is difficult to understand since the *-os* form in *s*-stem nominative/accusatives seems securely Proto-Indo-European and the *-as* form is taken eventually as analogical to Greek neuters in *-ας*.

<sup>34</sup> Generally explained as unmetathesised from a root *\*F<sub>2</sub>ρωγ-* or *\*F<sub>3</sub>ρωγ-*. Cf. *DMic s.v.*

<sup>35</sup> Cf. *DMic s. to-so*.

<sup>36</sup> The latest reading of the tablet for the projected definitive corpus volume is *je-ke-ri-ja-wo*.

<sup>37</sup> M. Lejeune, «Doublets et complexes», *Cambridge Colloquium*, pp. 135-149, esp. pp. 144-148, explained the forms spelled with *ra<sub>2</sub>* as examples of the phenomenon of *graphie inverse* where *ra<sub>2</sub>* which originally had the value */rja/* could be used to spell the clusters

Alex Leukart has explained the Hand 1 and 24 spellings of this name as 'modern upper class standard', i.e., the current and most correct spelling in palatal circles, while the Hand 6 version would be a variant spelling, involving a syncopation produced by a rapid or lax pronunciation of the final syllables: */\*Hekhé-l(l)āwōn/ > /\*Hekhé-l(l)aun/*<sup>38</sup>. However, Leukart shies away from explaining these two variants in terms of social stratification. According to Leukart, the Hand 6 variant */\*Hekhé-l(l)aun/* is not substandard, but 'casual/lax/latest modern upper class'. That is, it represents a version of *normal* Mycenaean, practised at the same social level, but exhibiting a recently developed tendency, within the same social class, toward lax pronunciation<sup>39</sup>.

Also remarkable here is the sheer number of variant spellings of this name within a limited repertory of attestations, particularly since some scholars, myself included, believe that it is the name of the king of Pylos<sup>40</sup>. Besides the variant treatment, full and syncopated, of *-āwōn* vs. *-aun*, we have apparently two different instances of plene spellings, by which the scribes have tried to represent the final simple consonant in the name by using dummy vowels: *e* (as in later Cypriote Syllabic script) and *a* (as is normal in Mycenaean for *ks* clusters: *wa-na-ka* etc.). Such *plene* spellings of single final consonants are exceedingly rare, in fact almost non-existent, in the Mycenaean corpus<sup>41</sup>. There

*ppa* and *lla* even in instances where *pp* and *ll* did not arise from original combinations of liquid and yod. Subsequent discussion is reviewed by A. Leukart, «Les signes \*76 (*ra<sub>2</sub>*, <<*rja*>>) et \*68 (*ro<sub>2</sub>*, <<*rjo*>>) et le nom du grand prêtre de Poséidon (sinon du roi) à Pylos», *Mykenaitika*, pp. 388-390. See Leukart, pp. 394-395, for explanation of the form *e-ke-ra-ne*.

<sup>38</sup> Leukart *supra*, n. 37, p. 394.

<sup>39</sup> Leukart *supra*, n. 37, p. 398.

<sup>40</sup> For a succinct overview of opinions on this point, see *DMic*, s. *e-ke-ra<sub>2</sub>-wo*.

<sup>41</sup> A. Leukart, «*PO-RO-QA-TA-JO, TO-SA-PE-MO, A-MO-RA-MA* and Others: Further Evidence for Proto-Greek Collective Formations in Mycenaean and Early Alphabetic Greek», *Studies Chadwick*, p. 361, n. 61, cites *ki-to-na* in KN Ld 785.2, *ka-ra-te-ra* in MY Ue 611.2 (where one finds conspicuous nominatives in parallel entries: *a-pore-we*, *pe-ri-ke*, *ipa-ke-te-re* which themselves may have induced the scribe to produce a *plene* nominative singular), and the personal name [*?wi*]-*ja-wo-ne* on KN Ws 1707 as parallels, in agreement with John Chadwick, «The Muster of the Pylian Fleet», *Tractata Mycenaeanae*, p. 80 and n. 20. It should be noted that *ki-to-na* on KN Ld 785 might be an error for the nominative plural or a simple scribal mistake induced by the forms *ke-ro-ia* and *po-ki-ro-nu-ka* in other entry slots on the tablet. Cf. on MY Ue 611 the possible influence of plural entries *ipe-ra*, *a-ta-ra*, and *qe-ti-ja* in the same and immediately preceding and following lines. This would leave only the personal names as potential examples of pure and intentional *plene* spellings. Cf. my discussion above of personal and place names and ethnics as the one category of words in which FEATURES 3 and 4 appear. These few exceptions to the hard and fast rule in Linear B not to represent single final consonants are so unusual that they are not discussed at all by Woodard *supra*, n. 31, pp. 11, 82-83.

is also clear variation in how to represent the simple or geminate liquid that begins the second element of the name. Here Leukart has proposed that  $ra_2$  at this stage may be reserved for representing simple /la/ while  $ri$ - $ja$  would be an inverse spelling reserved for spelling /lla/. Whatever the correct interpretation of these particular variants, Leukart's explanation is slightly flawed in that he mis-assigns the version  $e$ - $ke$ - $ri$ - $ja$ - $wo$  (nom. sing.: Hand 15: Qa 1292: now  $je$ - $ke$ - $ri$ - $ja$ - $wo$ ) to Hand 13, and therefore does not notice the further complication that this very scribe (Hand 15) also writes the name as  $e$ - $ke$ - $ra$ - $ne$  in the dat. sing on Un 219.1<sup>42</sup>. This might make it more likely that  $e$ - $ke$ - $ra$ - $ne$  is to be viewed as scribal haplography and restored as  $e$ - $ke$ - $ra$ - $\langle wo \rangle$ - $ne$ . If it is to be viewed as a simplified syncopated spelling, it might best be attributable to the oral or written source of the information Hand 15 is compiling on Un 219. The correct assignment of these variants to one and the same hand further supports Leukart's analysis of  $ra_2$ ,  $ri$ - $ja$ , and  $ra$  as alternative attempts to spell /lla/ or /la/<sup>43</sup>.

Leukart's view of the particular Hand 1 and Hand 24 spellings over against the syncopated spelling would correspond to what we know of the status and functions of the scribes involved. Hand 1 is the master scribe at Pylos and is in charge of administratively important assignments and of corrections, recensions and archiving<sup>44</sup>. His tablets show *normal* - $e$  dat. sing. universally; *normal*  $pe$ - $mo$  and  $a$ - $mo$  (but also  $a$ - $re$ - $pa$ - $te$  and  $a$ - $re$ - $pa$ - $zo$ - $o$ ); *normal*  $ti$ - $mi$ - $t$ -,  $i$ - $qo$  (but also  $e$ - $pa$ - $sa$ - $na$ - $ti$ <sup>45</sup> En 74.13, Ep 212.5); *normal*  $e$ - $ko$ - $si$ ,  $ti$ - $nwa$ - $si$ - $ja$ ,  $tu$ - $si$ - $je$ - $u$  (but  $mi$ - $ra$ - $ti$ - $ja$  and  $ka$ - $pa$ - $ti$ - $ja$ <sup>46</sup>); *normal*  $wo$ - $ze$ , but also *special*  $to$ - $ro$ - $qe$ - $jo$ - $me$ - $no$

<sup>42</sup> Leukart *supra*, n. 37, pp. 395 and 400, misled no doubt by the typographical error on pp. 60-61 in *PTT* II. Leukart bases some of his argument on the fact that Hand 13 elsewhere employs complex signs ( $pte$  on La 625 and  $nwa$  on La 633), but Hand 15 only employs the single complex sign  $pte$  on Un 219.6 and otherwise the doublet signs  $pu_2$  and  $a_2$  on Qa 1293, 1294, 1297, 1301, and Un 219, thus displaying an acute sensitivity to intervocalic aspiration.

<sup>43</sup> It also further supports Lejeune's initial observations (*supra*, n. 37) that  $ra_2$  can stand for  $rja$ ,  $lja$  and  $rra$  and  $lla$ . Partial dossier of data pertinent here:  $a$ - $ke$ - $ti$ - $ri$ - $ja$  and  $a$ - $ke$ - $ti$ - $ra_2$  (both Hand 1),  $ku$ - $te$ - $ra$ - $o$  and  $ze$ - $pu_2$ - $ra$ - $o$  (both Hand 23),  $a$ - $ke$ - $ra_2$ - $te$  = ἀγέραςτες < ἀγέραςτες. The name  $e$ - $ke$ - $ra_2$ - $wo$  can also be explained as a compound of oblique form  $egkhes$  and participial form of the verb \* $law$ , i.e., 'he who delights in the spear'. This in fact has a cluster /sll/ that produces geminate /ll/ for which  $ra_2$  and inverse spelling  $ri$ - $ja$  can be used.

<sup>44</sup> *Scribes Pylos*, pp. 50-58, 188-189.

<sup>45</sup> Risch *supra*, n. 9, p. 111, notes that this word is spelled  $i$ - $pa$ - $sa$ - $na$ - $ti$  twice in the E-texts of Hand 41, but in one case (Eo 247.4!) the initial  $i$ - was a correction of  $e$ -. This most likely indicates an overriding of one spelling/pronunciation by another on a 'primary level' document.

<sup>46</sup> The first ethnic adjective is always written  $mi$ - $ra$ - $ti$ - $ja$  in the Aa, Ab, and Ad texts of Hands 1, 21 and 23. The second is rendered as  $ka$ - $pa$ - $ti$ - $ja$  in *normal* Hands 41 and 1: Eb 338.A, Ep 704.7 and 539.9 (referring to the 'key-bearer' of  $pa$ - $ki$ - $ja$ - $ne$ ), and also

(= \*τροκωεομενος). Thus Hand 1 is solidly *normal* Mycenaean, but shows possible exceptions in FEATURES 2, 3, 4 and 5. I would explain his forms  $a$ - $re$ - $pa$ - $te$  and  $a$ - $re$ - $pa$ - $zo$ - $o$  (as opposed to Hand 21 and Hand 43:  $a$ - $re$ - $po$ - $zo$ - $o$ ) either as induced by the standard established and accepted ideographic usage of AREPA or by analogical extension from the normal  $a$ - treatment of the nominative.

The particular source of his information may also have influenced Hand 1 in a few instances to write in special Mycenaean. The spellings  $ka$ - $pa$ - $ti$ - $ja$  and  $mi$ - $ra$ - $ti$ - $ja$  in the Ep and Aa tablets may have been preserved from preliminary sets. The spelling  $to$ - $ro$ - $qe$ - $jo$ - $me$ - $no$  in the remarkable text that records the results of a visual inspection of fields by  $a$ - $ko$ - $so$ - $ta$ , a prominent collector-inspector with clear extra-palatial concerns in the Pylos tablets, may have been induced by dealing with this individual or with speech and speakers in the locale of the inspection. The personal name spelling  $e$ - $pa$ - $sa$ - $na$ - $ti$  on En 74.13 and Ep 212.5 is most puzzling<sup>47</sup>. It is not taken from the preliminary tablets of Hand 41 who uses the *normal* Mycenaean spelling  $i$ - $pa$ - $sa$ - $na$ - $ti$  on those tablets PY Eb 1350.A and Eo 247.4. But the correction on Eo 247.4 from an original spelling with  $e$ - indicates that even Hand 41 had an initial impulse to render the name of this  $te$ - $o$ - $jo$   $do$ - $e$ - $ra$  in *special* Mycenaean. Is this an indication that both of these highly placed scribes were here dealing directly with an individual whose name they heard pronounced in a way we classify as *special* Mycenaean, but that only Hand 41 overrode what he heard and spelled the name in a *normal* Mycenaean way? In any event, we should note how few are the examples of non-*normal* forms in the numerous and full texts of Hand 1, the most significant body of work attributed to a single scribe at Pylos.

What social and administrative milieu does Hand 24 move in? His three full tablets deal with matters relating to the most prestigious officials and institutions in the community of Pylos, specifically with the *temenē* of the  $wa$ - $na$ - $ka$  and  $ra$ - $wa$ - $ke$ - $ta$ , the landholdings of three  $te$ - $re$ - $ta$  and the  $wo$ - $ro$ - $ki$ - $jo$ - $ne$ - $jo$   $ka$ - $ma$ , the orchards and vineyards of  $e$ - $ke$ - $ra_2$ - $wo$  (possibly the *wanax* of Pylos), and the contributions to be made by  $e$ - $ke$ - $ra_2$ - $wo$ , the  $ra$ - $wa$ - $ke$ - $ta$ , the  $da$ - $mo$  and the  $wo$ -

in Hand 6: Un 443.3, but is found as  $ka$ - $pa$ - $si$ - $ja$  in Vn 851.12 by Hand 12, a list of  $de$ - $mi$ - $ni$ - $ja$  distributed to individuals. It is doubtful that the individual referred to in Vn 851 is the key-bearer, and we may be seeing the spellings of personal names here differentiated by social class. Pace Lindgren, *People* I, p. 60, II, pp. 27, 38, 56, 72.

<sup>47</sup> En 74.13 is thought to be based on Eo 247.4, where Hand 41 had originally also written  $e$ - $pa$ - $sa$ - $na$ - $ti$  only to correct this spelling to  $i$ - $pa$ - $sa$ - $na$ - $ti$ . Ep 212.5 corresponds to Eb 1350. The whole tablet Eb 1350 is palimpsestic, thus also allowing for the possibility that the scribe (Hand 41) had made a change to counter his natural tendency toward spelling this name with  $e$ -. Hand 41 is otherwise firmly *normal* except for unmetathesized  $ku$ - $su$ - $to$ - $ro$ - $qa$  and the feminine ethnic name  $ka$ - $pa$ - $ti$ - $ja$ . On  $a$ - $ko$ - $so$ - $ta$ , cf. Lindgren *supra*, n. 46, pp. 185-187.

*ro-ki-jo-ne-jo ka-ma* to a major ceremonial banquet in honour of Poseidon. Moreover, Un 718 which records these contributions was found in a special location in Room 7 Grid 83, apart from the bulk of tablets in the Archives Complex, but associated directly with the Ta series (by Hand 2, the most securely normal Mycenaean of all Pylian scribes throughout his large dossier of work and the direct associate and probably the prize pupil of Hand 1). The Ta series records an inventory of costly vases and inlaid furniture on an occasion when the *wanax* appointed *au-ke-wa* to the position of *da-mo-ko-ro*<sup>48</sup>. It seems that this series, like Un 718, had been brought to the central archives, but had not yet been processed or systematically filed in Room 8 when the palace was destroyed. By contrast, Er 312 (*temenē* of the *wa-na-ka* and the *ra-wa-ke-ta*) was found already archived in Room 8 in close association with the notorious offering text Tn 316. Thus so far as the contents and importance of his attested work, its archival associations, and the sharing of the very spelling of the name (of the *wanax*?) *e-ke-ra<sub>2</sub>-wo* with the most important scribe, on the evidence of the extant records, in the palatial administration at Pylos, Hand 24 has a clearly elevated status. We cannot then leave unexplained why the scribe who specializes in 'royal'-palatial subject matter uses forms that are supposed to derive from lower-class contamination. One would think that like Hand 2, and for the most part like Hand 1, this scribe also would write in the elite chancellery dialect.

How might we explain this situation? First, let us consider Hand 24's peculiarities. He is *special* in the one instance of a dative singular consonant declension in his texts: *po-se-da-o-ni* in Un 718.1. He is special in a-treatment of sonants in all five instances of the word for seed grain: *pe-ma* on tablets Er 312 and Er 880. He likewise uses the ideogram AREPA and undoubtedly produced *a-re-ro* on Un 718.8 by a simple and well-paralleled graphic slip for *a-re-pa*<sup>49</sup>. Hand 24 is special in using the non-metathesised form *ku-su-to-ro-qa* in Er 880.8 and perhaps in *wo-ro-ki-jo-ne-jo* (Er 312.7 and Un 718). If, with Risch and Thompson, we reject the rare instances of *ti* vs. *si* as being indicative of dialect variation within Mycenaean, and explain them rather as isolated and preserved forms of non-Greek toponyms, ethnics, personal names, then the single instance of *ra-wa-ke-si-jo* (normal feature 4) in tablet Er 312 of Hand 24 would not

<sup>48</sup> This appointment was not made at the time when On 300 (from Grids 13, 22, and 23 in Room 8) was written and filed. For the historical circumstances and purpose of the Ta series, cf. Palaima *supra*, n. 4.

<sup>49</sup> The elaborate theory of Leukart *supra*, n. 41, pp. 362-3, to explain *a-re-ro* as a faulty or failed attempt to render *\*a-re-<po->ro*, i.e., */aleiphor/* written *plene* is hyper-convoluted in that it requires that the scribe be making an unmotivated and unexplained and botched attempt at a rare *plene* spelling that would produce an *o*-treatment in direct contrast to *pe-ma* and AREPA. It is far more reasonable to accept a simple *lapsus styli* of a single horizontal stroke. Cf. the parallel of the omission of two horizontals marking the male sex of bovids on Knossos tablet Ce 59.

concern us here, and Hand 24 would be decidedly special Mycenaean. If we include this *datum* for purposes of understanding the scribe's idiosyncrasies, then he would still be the most '*special*' Mycenaean of any scribe.

The other main peculiarity of Hand 24 is the fluctuation between spellings *to-so* and *to-so-jo*. The form *to-so-jo* is the exception, seen only on Er 312.2, .8 and not at all on Er 880 and Un 718 where spelling with *-so* or *-sa* prevails (7 times, plus 2 times on Er 312). Martín Ruipérez has recently explained the alternation between *to-so* and *to-so-jo* in Hand 24 as alternative spellings of palatalised *s* as an intermediate phonetic stage between *\*totyos* and *tosos*. According to this theory, palatalised *s* could be spelled with plain *so* (cf. *ku-ru-so* in Ta 716.1 where it must stand for nominative dual χρυσυω) or with *so-jo* (as in *to-so-jo*)<sup>50</sup>. Ruipérez's theory has considerable merits on other grounds. It is consistent with the usage of *ra = lja, rja* or even *lla, rra* discussed above. Here it offers a much more reasonable explanation of the exceptional forms *to-so-jo* than alternative theories that viewed them as genitives of measure or the forms in *-so* as long *o* genitives. The scribe might have been induced to be especially correct in making his first entry regarding the *temenos* of the *wa-na-ka* (cf. his careful spelling of the king's name in the same precise manner, using the doublet sign *ra<sub>2</sub>*, as Hand 1) but this does not then explain why he abandons such a spelling in the case of the *te-re-ta*, only to revert to it again with regard to the *wo-ri-ki-jo-ne-jo e-re-mo*.

More important still is the nature of these documents. They are the only documents by Hand 24. Un 718 was as yet unprocessed and set aside in Room 7 along with the tablets of the Ta series which recorded an inventory-inspection of precious furniture and artifacts when the king made *au-ke-wa* the *da-mo-ko-ro* (of the Hither Province?)<sup>51</sup>. Here the tablets sat when the Palace of Nestor was destroyed. They are undoubtedly among the last ever written at the site. Un 718 even has a corresponding basket/shipping label (Wa 731) written by Hand 24 and located in the basket arrival area Room 7 grid 52. This means that the tablet was delivered to the Archives Complex from outside. From where outside?

Un 718 and Er 880 specifically mention the term *sa-ra-pe-da / sa-ra-pe-do* for which there are two alternative explanations<sup>52</sup>:

1. a technical term for landholding and
2. a toponym specifying location (in the neuter plural and either genitive plural or locative plural).

<sup>50</sup> M.S. Ruipérez, «À propos de *to-so-jo* de PY Er 312», *Mykenaiika*, pp. 563-567.

<sup>51</sup> Cf. T.G. Palaima and J.C. Wright, «Ins and Outs of the Archives Rooms at Pylos: Form and Function in a Mycenaean Palace», *AJA* 89, 1985, pp. 251-262, and Palaima *supra*, n. 4.

<sup>52</sup> Cf. *DMic*, s. *sa-ra-pe-da*.



Neither explanation is without difficulties. The first requires that the term function on Un 718 as a kind of technical header for a classification of landholding under which the four different sets of individuals or groups of individuals listed on the tablet make contributions to the commensal ceremony for Poseidon. It strikes me as very harsh usage to have persons stand in parallel position to a land category. I also would have expected some reference then to the term on Er 312 which lists 4 categories of *landholdings*. It might also be thought more probable that a special location would be recorded only twice in the tablets, rather than that a term for a special class of landholdings would appear only twice.

But the explanation as a toponym is equally problematical. The neuter plural usage on Un 718 must be explained as a rubric form, to my mind the last refuge of a text-exegetical scoundrel, and the form on Er 880 requires restoration of *-i* to produce the soundest explanation (dat.-loc. plur.). If we take the term as a locative, it is at least not implausible that we should take Er 312 to refer to the same location. If it is not a locative, then it is still best to assume that all three texts refer to the same local area, which would need not be named by the scribe because its location, containing as it does special landholdings and orchards of important individuals and social groups, would be well known. In either case, we would be able to advance the same kind of argument as follows, *mutatis mutandis*. So I shall hereafter assume that *sa-ra-pe-da* refers to a specific location. If it does not, then the tablets themselves focus on one area in which a special category of landholding is found.

The site *sa-ra-pe-da* is mentioned only in these texts, and it contains a *temenos* of the king, if not *the temenos* of the king. Un 718 with its poor use of space, resultant overcrowding and informational run-ons in lines .11 and .13, and its abbreviated non-parallelism of entries in lines .7 and .11 has the appearance of a preliminary report delivered, as it was, to the central archives (with its transport basket and label). The text is also prospective, stipulating contributions to be made and based probably on the information earlier compiled in Er 312 and perhaps, in part, on Er 880. Thus we have here epigraphical and archival circumstances that make it possible that Hand 24 was a specialist in the district of *sa-ra-pe-da*, or in whatever district these landholdings are situated, and to some degree in the affairs of *e-ke-ra<sub>2</sub>-wo* and the estate of the *wanax* located there. This would explain the rarity of his texts within the archives. Keep in mind that there were something like 360 days to the normal year in the regular Mycenaean lunar calendar, and for Hand 24 we have a mere three tablets and a transport basket label. What did he do, and how did he use his hard-learned skills as a writer of Linear B, the rest of the administrative year? Perhaps monitoring activities in *sa-ra-pe-da*. The implication would be that the dialect spoken in this particular district, at least by the officials or persons from whom Hand 24 would have obtained his detailed information,

might well have been related to what we call *special* Mycenaean. Might we then contemplate what dialect the individual who was the *wanax* and who held important lands and performed important sacrifices in this area might have spoken?

In conclusion, I would like to return to the implications of the various spellings of the name of */\*Hekhé-l(l)āwōn/*: *e-ke-ra<sub>2</sub>-wo*, *e-ke-ra<sub>2</sub>-wo-ne plene*, *le-ke-ri-ja-wo*, *e]ke-ra<sub>2</sub>-u-na plene*, *e-ke-ra-ne = e-ke-ra-<wo>-ne*. First the unprecedented concern here with *plene* spellings certainly must be taken as yet another bit of evidence in support of identifying this individual as the *wanax*. Second, the various treatments (*ra<sub>2</sub>*, *ri-ja*, *ra* and *a-wo*, *a-u*) clearly indicate that no consensus had yet been reached within the palatial bureaucracy about how to spell the name of one of the most, if not *the* most, important individuals within the community. This can be taken as yet another indication of the lack of an imposed and prescribed scribal training in orthography, but I think, and I have argued above, that there was such a training and that it was responsible for so consistent an obliteration of any but the smallest traces of dialectal diversity. I rather think the variant spellings of this personal name reflect something for which one can build a case on other grounds: namely, that the scribes had not yet had time to form a consensus because they had not yet been writing the name of this king for a very long time. I think that */\*Hekhé-l(l)āwōn/* was not only the *wanax* of Pylos, but he was also the *new wanax* of Pylos. Hand 24 not only wrote in service of the king, but in service of the new king. Such an argument, however, is not part of the brief of this paper.

I hope to have shown that the question of dialect divisions in the Linear B texts will still reward further attention, especially on the level of individual scribes, their sources for accessing information, the social milieu in which they were working, and the influence of stricter or looser training in standardised orthography. If, with Thompson, one prefers an alternative explanation for the data amassed by Risch and subsequent scholars, i.e., 'sound change in progress spreading by a process of lexical diffusion'<sup>53</sup>, one would have even more incentive now to burrow in on the level of the individual scribes and to study what is going on with their idiolects and why<sup>54</sup>.

Austin, TX 78712-1181, U.S.A.  
University of Texas at Austin  
PASP Classics  
WAG 123 (C3400)  
pasptgp@utxvms.cc.utexas.edu

THOMAS G. PALAIMA

<sup>53</sup> Thompson *supra*, n. 4, pp. 325-327.

<sup>54</sup> And to study *all five* FEATURES and other indications of idiosyncrasy within scribal work.