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. Bennet28.

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.

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SPECIAL VS. NORMAL MYCENAEN 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 communal banqueting ceremonies1. 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. Mycenaean scribes have been called ‘literate functionaries’ — correctly, I think, since I am one of those who have called them that — 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-ka-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 Joa Melen and Ted Somerville for discussing details of this paper with me. Any faults remaining herein are solely mine.


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we, du-ma-te, 'collectors', or those named as performing inspections, e.g., pu-ke-qiri on PY Ta 711. 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 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 thematic dative singular spelled with -e (normal) 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-rî on (300.3, 5 and On 1074 S 300-Cii) and du-ma-rî (On 300.6 S 300-Cii) vs. normal po-ro-du-ma-te (Ft 50.7 Cii).

**Feature 2.**

The reflex of IE sonant nasals in the environment of bilabial consonants (m, n, 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-mo (5 times in Fr 312, 880 by Hand 24); στεριαο vs. στεριαο; contrast the universal use of a-mo = ὅμο = 'wheel' in many hands at KN and PY.

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4 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 Jorco and held at the Universities of Alicante and Orthruela, Spain, February 17-19, 1999.


6 Old I.-E. form of dative: -el.

7 Old I.-E. form of locative -i, used almost universally in historical Greek for dative.

8 pe-mo found at Pylos on Eb 2; pe-mo 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.

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**Feature 3.**

In certain words, the occurrence of i (normal) vs. e (special) in the environment of a labial consonant (m, n, p): toponym ti-mi-ti-ja vs. te-mi-ti-ja (cf. ti-mi-to a-ke-i); theonym a-ii-ti-me (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 δήνες, i-qo (Ta 722.1 Hand 2), i-qo-jo (Eq 59.5 Hand 43), i-pe-po-po-i (Ft 75.10 Hand 45), i-qe-ra (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 which exploited the preliminary identifications of scribal hands within the Pylos corpus by E.L. Bennett, Jr. and J.-P. Olivier. Roger Woodard 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 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:

**Feature 4.**

Assibilated ñi (normal) vs. unassibilated ñi (special).

G. Nagy 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 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...
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.

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, and see below on t-pa-so-na-ti vs. e-pa-so-na-si);
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, telestial, and religious officials in specific religious districts;
3. lengthy page-shaped tablets focused on the activities of key regions and influential 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;

19 Chadwick, «Traditional Spelling or Two Dialects», Res Mycenaen, pp. 78-87, esp. p. 86, later made a virtue of necessity by arguing that the very failure of any scribe 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’.

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., supposing traces of the normal speech habits of the larger population (= special Mycenaean)²⁵. I have often wondered whether the incursion 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 much²⁶.

We should also recognize 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

²⁵ 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 -i 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, it is not also possible for dialects to vary in applying and not applying analogy, e.g., Attic acc. plur. vaîg (by analogy) vs. Doric and Hdt. vàk and vàk 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 ék), the opposite would be true. Of course, -i is prevalent in consonant stem derivatives of the historical dialects except in rare forms like compound names exhibiting Dim. -Elements of the historical dialects except in rare forms like compound names

²⁶ See below on the many different ways in which individual scribes spell the name e-ke-ke-ke-wo.
have trouble documenting the arrival of Dorian speakers by means of material
evidence. If the Dorians were in fact the silent lower-class Volks, 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 III B 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
Pylaios who are so closely involved in Woodard’s conclusions about the
significance of unassemblable forms (FEATURE 4). But here we shall concen-
trate 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
24.27

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 Pylaios 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.28 The tablets date from a single period, perhaps a matter of months,29 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.30 As we have remarked
above, we do not know whether scribes included rigid instruction in
traditional spelling of words (as opposed to basic principles of syllabic
representation), 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 Pylaios 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 Pylaios tablets. The tablets of Hand 91 and Civ are
now reduced from five to four by the join made between tablet Xa 1449 and Vn
1339 of Chl. Cf. J.L. Melena, «40 Joins and Quasi-Joins of Fragments in the Linear
B tablets from Pylaios», Minos 31-32, 1996-97, pp. 165-167. Xa 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.

27 On Un 718, see T.G. Palaima, «The Last Days of the Pylaios Policy», Politeia, pp. 631-
633.

28 Scribes Pylaios, 111-113, 133, 171-177. The traditional number of ‘tablets’ is used here,
adding the inscribed material presented in C.W. Shelmerdine and J. Benet, «Two


30 Cf. now J. Driessen, «Le palais de Cnosos au MR II-III : Combien de destructions?»,
Côrte mycénienne, pp. 113-134.

31 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 is dative singular: *po-se-da-o-ri* (Un 718.1)33;

**Feature 2:** a treatment of sonant nasals: *pe-ma* (Er 312.2, .5, 8, Er 880.4, 8)34;

**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-ro-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)35.

Hand 24 also exhibits other peculiarities, such as the much-debated form *tso-so-jo* in the phrase *tso-so-jo pe-ma* in Er 312.2 and .836. To make matters more complicated, the scribe uses *tso-so* probably on Er 312.5 and definitely on Er 880.5, 8 and Un 718.2 (*tso-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-wo* (nom. sing.: Er 880.1 and Un 718.2) with Hand 1 (*e-ke-ra-wo* gen. sing.: An 610.13 and *e-ke-ra-wo* plene spelling with *e* of the nom. sing.: An 724.5) over against *e-ke-ri-ja-wo* (nom. sing.: Hand 15: Qa 1292)37 and more strikingly *e-ke-ra-wo-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-<c>wo-<c>-ne*, although Leukart, wrongly it seems [see below], would view this form as a syncopated spelling using one sign per syllable: *He.khe[li].lau.nei*38.

For a tabular analysis of this feature at Pylus, cf. Thompson, supra, n. 2, p. 328, Table 2.

The monographic ideogram *AREPA* (Un 718.8) is attested elsewhere at Pylus: PY Fr 1196 Hand 2; Un 853.4 Hand 6; WR 1437 an unassigned sealing from oil magazine Room 24, According to C.J. Rutigli (Antti Romu, p. 813), the normal nominative of this word would be *aleiphar* (from sonant *n*) and the oblique and compositional form (from sonant *n*) would be *aleiphar*. Instances of compositional forms with *-a* would be by analogy with the nominative. Instances of nominative in *-a* would be by analogy with the oblique case. If this elaborate explanation via bidirectional leveling of the forms were correct, the monographic ideograms would be indirect evidence that Hand 24 could write in some cases a form that ultimately derived from *e*-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 *kώς*) as relevant here is difficult to understand since the -ar form in a-stem nominative/accusative seems securely Proto-Indo-European and the -ar form is taken eventually as analogical to Greek neuter in -a or -os.

Generally explained as unmetathesised from a root *-pory or *-poros. Cf. DMe s. *to-so*.

The latest reading of the tablet for the projected definitive corpus volume is *le-ke-ra-jo-wo*.

M. Lejeune, *Doublets et complexes*, Cambridge Colloquium, pp. 135-149, esp. pp. 144-148, explained the forms spelled with *ra* as examples of the phenomenon of graphie inverse where *ra* which originally had the value /ra/ could be used to spell the clusters *po* and *la* even in instances where *po* and *la* did not arise from original combinations of liquid and yod. Subsequent discussion is reviewed by A. Leukart, «Les signes ʔ/sa, <c> es/ <c> es ʔ 68 (93, ʔ/c> > ʔ/c> > ʔ/c>) et le nom du grand père de Posidôn (sinon du roi) à Pylôs», Mykenais, pp. 398-399; See Leukart, pp. 394-395, for explanation of the form *e-ke-ra-ne*.

Leukart supra, n. 37, p. 394.

Leukart supra, n. 37, p. 398.

For a succinct overview of opinions on this point, see DMie s. *e-ke-ra-wo*.

A. Leukart, *PO-RO-QA-TO-JO, TO-SA-PÉ-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-po-re-wo, pe-ri-ke, *ipa-ke-te-re* which themselves may have induced the scribe to produce a plene nominative singular), and the personal name *ʔ/ʔ-*wa-wo-ne on KN Ld 785.17 as parallels, in agreement with John Chadwick, «The Muster of the Pylian Fleet», Tractata Mycenaene, 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-ru-ta and po-ki-ru-ru-ka in other entry slots on the tablet. Cf. on MY Ue 611 the possible influence of plural entries *ipa-*ra, a-ta-*ra, and pe-er-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 ethics 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 rə as at this stage may be reserved for representing simple ɾa while ri-ja would be an inverse spelling reserved for spelling ɾla. 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 219.14. This might make it more likely that e-ke-ra-ne is to be viewed as scribal haplography and restored as e-ke-ra<wo>-ne. If it is to be viewed as a simplified syncopated spelling, it might best be attributable to the oral or written version 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, ri-ja, and ra as alternative attempts to spell ɾla’or ɾa.43

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 Pylus and is in charge of administratively important assignments and of corrections, revisions and archiving. His tablets show normal -e dat. sing. universally; normal pe-mo and a-mo (but also a-re-pa-te and a-re-pa-za-o); normal mi-ti, i-go (but also e-pa-sa-na-ti) on En 74.13, Ep 212.5; normal e-ko-si, ti-nwa-si-ja, ti-sti-je-u (but mi-ra-ti-ja and ka-pa-ti-ja); normal wo-ze, but also special to-vo-jo-jo-me-no.

42 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 mao on La 633), but Hand 15 only employs the single complex sign pte on Un 219.6. and otherwise the doublet signs pui and a on Qa 1293, 1294, 1297, 1301, and Un 219, thus displaying an acute sensitivity to intervocalic aspiration.

43 It also further supports Lejeune’s initial observations (supra, n. 37) that ra can stand for rja, ɾa and ra, and la. Partial dossier of data pertinent here: ake-ri-ja and ake-ti-ra, (both Hand 1), ku-te-ra-o and ze-pui-za-o (both Hand 13), a-ke-ra-ze = ḏe-r̥pa-raṣe < ḏe-r̥pa-raṣe. The name e-ke-ra-ze can be also explained as a compound of oblique form ephes and participial form of the verb ḏaw, i.e. ‘he who delights in the spear’. This in fact has a cluster /sl/ that produces geminate /ll/ for which ra and inverse spelling ri-ja can be used.

44 Scribes Pyloros, pp. 50-58, 188-189.

45 Leukart supra, n. 5, 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 (Es 247.41) the initial i was a correction of e. This most likely indicates an overwriting of one spelling/pronunciation by another on a ‘primary level’ document.

46 The first ethic 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 (= ḏo-pa-ti-ja). 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-za-o (as opposed to Hand 21 and Hand 43: a-re-pa-za-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 ro-vo-o-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 on 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. 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 Ep 247.4. But the correction on Ep 247.4 from an original spelling with e- indicates that even Hand 41 had an initial impulse to render the name of this te-ko-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 Pylus.

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 Pylus, specifically with the tenemets of the wa-na-kas and ra-wa-ke-ta, the landholdings of three te-re-ta and the wo-ro-ki-ne-jo ka-ma, the orchards and vineyards of e-ke-ra-wo (possibly the winax of Pylus), and the contributions to be made by e-ke-ra-wa, the ra-wa-ke-ta, the da-mo and the wo-

in Hand 6: Un 443.3, but is found as ka-pa-ti-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.

47 En 74.13 is thought to be based on Es 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 unnetatthesized ka-su-to-ra 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 Mycenaean 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. 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 (temerē of the wa-na-ka and the ra-wa-ke-ka) was found already archived in Room 5 in close association with the notorius 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-ro2-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 chancellerie 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-n in Un 718.1. He is special in a treatment of sonants in all five instances of the word for seed grain: pe-na on tablets Er 312 and Er 880. He likewise uses the ideogram AREPA 4 and undoubtedly produced a-re-ro on Un 718.8 by a simple and well-paralleled graphic slip for a-re-pa. Hand 24 is special in using the non-metathesised form ku-su-to-ro-ga 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, ethnic names, personal names, then the single instance of ra-wa-ke-si-jo (normal feature 4) in tablet Er 312 of Hand 24 would not 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 -su prevails (7 times, plus 2 times on Er 312). Martín Ruizpérez 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 *tioyos and toisos. According to this theory, palatalised s could be spelled with plain s (cf. ku-su-ro in Tn 716.1 where it must stand for nominative dual *pi-su) or with so-jo (as in to-so-jo). Ruizpérez's theory has considerable merits on other grounds. It is consistent with the usage of ra = *la, rja or even *la, in the discussion 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 ra2, 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 Higher Province). 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-d036 for which there are two alternative explanations:

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

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 soured, 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 those 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-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-li(awoi)/ e-ke-ra-wo, e-ke-ra-wo-ne plene, jé-ke-ri-ja-wo, e'ke-ra-una plene, e-ke-ra-ne = e-ke-ra-[<w.o>-]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, 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-li(awoi)/ 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, one would have even more incentive now to burrow into on the level of the individual scribes and to study what is going on with their idiolects and why.

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53 Thompson supra, n. 4, pp. 325-327.
54 And to study all five PLU SERIES and other indications of idiosyncrasy within scribal work.