

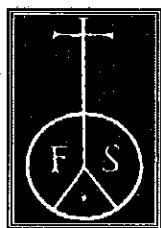
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OPEN PROBLEMS IN MYCENAEAN PHONOLOGY AND THE INPUT OF MORPHOLOGY

1. *Phonological uncertainty.*¹

We have had more than fifty years of discussions about Mycenaean phonology and we may well feel that the law of diminishing returns begins to operate. On the other hand in the editorial work for the first and second volume of our *Companion to Linear B* Yves Duhoux and I have been struck by how much those who edit texts or comment on them diverge in their interpretations.² We have been reproached for allowing inconsistencies such as e.g. the transcriptions /-ēus/ and /-eus/ (Myc. -e-u) or /hek^hei/ and /ek^hei/, 'has' (Myc. e-ke), but in fact we did this deliberately because we felt that there were too many points of uncertainty and that to impose our views – particularly when even the two of us did not always agree on everything – would have been misleading and would give the impression that there is an established doctrine, though this is most emphatically not the case. Hence our decision to allow individual contributors to adopt their own conventions (provided they were consistent), in the awareness that students might find it confusing but in this way would also acquire their first understanding of the real world of Mycenaean studies. We all know that even in the same periodical one and the same word may be quoted in different phonological renderings. Our form of eclecticism is also supported by another consideration. We must remember that currently Mycenaean studies have turned away from purely linguistic problems and moved towards questions of interpretation, in the first instance of the actual texts and then of the society which produced those texts. From that point of view whether we read e-ke as /hek^hei/, 'has' or /ek^hei/, 'has' does not make much difference.

If so, why should we persist in these discussions? The answer is double. On the one hand, they are crucial for the linguist. Mycenaean is the only direct evidence that we have for Second Millennium Greek and, if we could get its linguistic interpretation straight, we would acquire crucial and unique information about the development of the language from Indo-European onwards and about the history and prehistory of dialect divisions in Greece. In its turn this type of information also illustrates questions of people contacts and movements which are important for the historian. On the other hand, linguistic questions may be crucial for the interpretation. I quote only one example. In **PY**

¹ In what follows I do not provide exact references to the Linear B words or tablets quoted if they are easily traceable through Aura Jorro's *DMic*. I use angular brackets <> to refer to spelling, slashes / / for (very rough) phonological transcriptions and square brackets [] for phonetic transcriptions. \$ indicates syllabic division. To indicate consonantal *i*, I use /j/ but for Indo-European or Proto-Greek reconstructions I use **i*.

²Y. DUHOUX, A. MORPURGO DAVIES (ed.), *A Companion to Linear B. Mycenaean Greek Texts and their World*, I-II, Louvain-la-Neuve 2008-11.

about the dialect position of Mycenaean with the risk of a vicious circle since in their turn all assumptions about the dialect position of Mycenaean depend on exactly that type of decisions. Consider the case of *to-so* about which we ought to know everything: meaning, usage, etymology, alphabetic forms. But should we read it $\tau\acute{o}\sigma\sigma\omicron\varsigma$ (as in Lesbian or Thessalian) or $\tau\acute{o}\sigma\omicron\varsigma$ (as in Arcadian and Ionic-Attic) or even /totsos/?⁵ In most instances the uncertainty will remain but there is a case for bringing to bear on the problem as many approaches as possible.

2. Phonology vs. morphology

To a large extent our understanding of morphology depends on our understanding of phonology, but there are instances when the process may be reversed: morphology may be less ambiguous than phonology. Occasionally this allows us to solve some phonological problems. Consider, for instance, the history of intervocalic *s in Greek. We know that an inherited /s/ between vowels has turned into /h/ before the time of the Linear B texts as shown by e.g. *pa-we-a₂* /p^harweha/, 'pieces of cloth' (< **pharwesa*) or *we-te-i* /wete(h)i/ 'year' (dat.-loc. sing.) (< **wetes-i*). Should we then assume that [s], as in e.g. *do-so-mo* /dosmos/, and [h], as above, were in complementary distribution? If so, [s] would have occurred word-finally and pre- and post-consonantly and [h] intervocalically and initially before vowel. Forms like *to-so* are not counterevidence because we cannot know whether this word has /s/ or /ss/ or even /ts/ or /tš/. Similar problems arise with words borrowed from non-Indo-European languages like *se-ri-no*, Gr. σέλινον 'celery' (a pre-Greek word) or *ku-ru-so*, Gr. χρυσός, 'gold' (a Semitic word). The odds are that these include initial and intervocalic /s/, but we cannot exclude that they had a different form of sibilant not entirely integrated into the Greek system and rendered as a *pis aller* with the *s*-signs. On the other hand, when we find forms of dat.-loc. plur. like *ti-ri-si*, Gr. τρισί, 'three' or *qa-si-re-u-si*, Gr. βασιλεῦσι, 'chiefs', we cannot help linking these postvocalic <-si> endings with the post-consonantal <-si> endings of e.g. *tu-ka-ta-si* /thugatarsi/, 'daughters' which clearly continue the sibilant of the IE ending of locative plural. We assume that after vowel /si/ changed into /hi/ as in the pronoun *pe-i* /spehi/, 'to them', or in the thematic dat.-loc. plur. in *-o-i* /-oihi/, but was later restored on

Fachtagung der Indogermanischen Gesellschaft, G.E.W. Dunkel et al. (ed.), Wiesbaden 1994, p.185-200. For a very balanced summary see E. RISCH and I. HAJNAL in the 2006 web publication of their *Grammatik des mykenischen Griechisch* (<http://www.uibk.ac.at/sprachen-literaturen/sprawi/mykgr.html>), p. 303-304.

⁵ /totsos/ is unlikely unless we believe that *to-so* has /ts/ while e.g. *-we-sa*, Gr. -εσσα has /tš/; in that case, however, the <z> sign of e.g. *ka-zo-e* (see below) would cause problems since it is unlikely to represent a third affricate; see in general C. BRIXHE, *Phonétique et phonologie du grec ancien I. Quelques grandes questions*, Louvain-la-neuve 1996, p. 60-63. Elsewhere C. BRIXHE, "Préhistoire et début de l'histoire des dialectes grecs", *Incontri linguistici* 29 (2006), p. 39-59 at p. 54-5 rightly objects to the assumption that Mycenaean had both /tossos/ and the like of /eressō/, since a merger of monomorphemic /[tj] and dimorphemic [tj] would not have been reversible, while Arcadian etc. have from the former $\tau\acute{o}\sigma\omicron\varsigma$ and from the latter /-ssō/ verbs. In fact Bartoňek, to whom Brixhe refers, would argue that the distinction did exist in Mycenaean and was realized as /-(s)s/ vs. /ts/, though the <s> spelling did not mark these distinctions (A. BARTOŇEK, *Handbuch des mykenischen Griechisch*, Heidelberg 2003, p. 142-143).

the analogy of post-consonantal /si/.⁶ At the time of the restoration, however, we must assume that [s] and [h] were not in complementary distribution because if there was an automatic alternation between the two we would not expect the analogical replacement of the one with the other. After the restoration both [s] and [h] occurred in the same intervocalic environment and there was no question of complementary distribution. Similar considerations can probably be made for restored sigmatic aorists like *e-re-u-te-ro-se*, Gr. ἤλευθέρωσε or sigmatic futures like *do-se*, Gr. δώσει.⁷ We may now try to explore in more general terms what we can learn from Mycenaean morphology about Mycenaean phonology.

3. The z-signs

I take as first example of what I am interested in the Mycenaean z-signs. The matter has been discussed again and again. To cut a long story short, from the first edition of *Documents* (1956) it was clear that the signs *17, *74, *20 could be read <za, ze, zo> because of words like *to-pe-za*, Gr. τράπεζα, *ze-u-ke-si*, Gr. ζεύγεσι, *me-zo*, Gr. με(ι)ζων. However Mycenaean <z> did not correspond to alphabetic Greek <ζ> in forms like the nom. sing.fem. KN *ka-za*, 'made of bronze' (cf. KN *ka-ke-ja-pi*, *ka-ki-jo*, Gr. χάλκειος) or the nom. plur. PY *ka-zo-e*, 'of bad/worse quality' (<**kak-īos-es*, cf. Gr. κακίων), or KN, PY *su-za*, 'fig tree' (cf. Gr. σῦκον 'fig' and συκίς, συκέα, συκία), etc. Palmer first argued in 1955 that the z- signs in fact indicated palatalized consonants and reiterated it in 1963.⁸ In 1966 the matter was discussed in full by G.R. Hart but the discussions continued and one might say that there is no great name of Mycenaean phonology who has not been involved.⁹

⁶That <-o-i> and <-a-i> are to be read as /-oi(h)i/ and /-ā(h)i/, and not as /-ois/ and /-ais/ is normally assumed but not by everyone. C.J. RUIGH repeatedly argued for the alternative view (e.g. in *Études*, p. 76-78) and more recently C. Brixhe made a strong case for it, though this has not found general acceptance (C. BRUXHE, "Du 'datif' mycénien aux protagonistes de la situation linguistique", in *Mykenaiika*, p. 129-157; *Phonétique*, cit. (n.5), p. 43-44, 65-67; "Préhistoire", cit. (n. 5), p. 51-53). C. BRUXHE, *Phonétique*, cit. (n.5), p. 43 also disputes the standard model of s-loss and restoration, wondering whether the morphological pattern did not have a preserving effect on a threatened initial and intervocalic /s/.

⁷More extensive discussion in A. MORPURGO DAVIES, "Phonetic laws, language diffusion and drift: the loss of sibilants in the Greek dialects of the first millennium BC", to appear in a collective volume, Oxford 2012.

⁸L.R. PALMER, "Observations on the Linear 'B' tablets from Mycenae", *BICS* 2 (1955), p. 36-45 at p. 41-42; *Interpretation*, p. 45-46. Cf. G. R. HART, "The effect of the palatalization of plosives in Mycenaean Greek", in *Cambridge Colloquium* (1966), p. 125-134.

⁹The bibliography is too large to be quoted in full. For a good summary of data and positions see M. DEL FREO, "*Pa-sa-ro*", cit. (n.3); for a recent and detailed account see E. RISCH and I. HAJNAL, *Grammatik*, cit. (n. 4), p. 243-256, §§ 84-86. Among the older articles one must refer at least to M. LEJEUNE, "Les sifflantes fortes du mycénien", *Minos* 6 (1960), p. 87-137 (= *Mémoires* II, p. 95-139); E. RISCH, "Les consonnes palatalisées dans le grec du II^e millénaire et des premiers siècles du I^{er} millénaire", *Colloquium mycenaeanum* (1979), p. 267-81; J. CHADWICK, "Traditional spelling or two dialects?", in *Res Mycenaeanae* (1983), p. 78-88, at p. 80-82; E. CRESPO, "Palatal stops in Greek: reconstruction or Mycenaean evidence", *Minos* 19 (1985), 91-104; C. J. RUIGH, "Problèmes de philologie mycénienne", *Minos* 19 (1985), p. 105-167 (reprinted in *Scripta minora* II, cit. (n. 3), p. 43-105);

It is now generally agreed that, like e.g. the <p-> signs, the <z-> signs indicate both voiceless and voiced sounds. As for the nature of the sounds various possibilities have been regularly considered: <z-> could indicate (i) sequences of stops (dorsal or dental) and prevocalic /i/ or /j/, e.g. /k^(h)jV/, /gjV/ or /t^(h)jV/, /djV/, (ii) simple or geminate palatalized stops (again dorsal or dental), e.g. /k^(h)k'/, /g^(h)g'/ or /t^(h)t'/, /d^(h)d'/, (iii) palatalized affricates, e.g. [t^s] / [d^s], (iv) non palatalized affricates, e.g. /t^s/, /d^s/. Forms like KN, PY *za-we-te*, 'this year', Ion. σῆτες, Att. τῆτες (< *k_iāwetes) or *su-za*, 'fig trees', could represent /k_iāwetes/ and /sūkja-/ or /k'āwetes/ and /sū(k')k'a-/ or forms with /t^(h)tj/ or /t^(h)t'/. Equally plausible are the more developed /t^sāwetes/, /sūt^sa-/ where the affricate could be /t^s/ or /t^s/; similarly *me-za* could be /megjōs/ or /me(g')g'ōs/ or alternatively /medjōs/ or /me(d')d'ōs/, but /med^sōs/ or /med^sōs/ are also possible. On the other hand, when we look at a form like *-pe-za*, Gr. -πεζα < *pedia (see *to-pe-za* above), we may consider /-pe(d)dja/ or /-pe(d')d'a/ or alternatively /-pe(d)d^sa/ or /-pe(d)d^sa/ but we are reluctant to read <-za> as /-(g) gja/ or /-(g')g'/ since the normal line of development is reconstructed as moving from velar to alveolar or affricate.

According to Chadwick an affricate interpretation would be easier in these cases, but alternations like *a-ke-ti-ri-ja* / *a-ze-ti-ri-ja*, probably for ἄσκητρια-, would be easier to understand if <ze> represented a palatalized consonant. However, Chadwick's own conclusion that the z- signs represented both affricates and palatalized consonants but in different dialects has found no acceptance and does not seem very plausible.¹⁰ Any discussion must now take into account two points made by J. Méndez Dosuna: (i) it is phonetically implausible to derive from /ts/ or /dz/ the /tt/ or /dd/ sequences of e.g. Boeotian and Attic which correspond to /ss/ of the other dialects and it is far easier to derive them directly from /t't'/, /d'd'/, (ii) there is good evidence for /s/ favouring the palatalization of a following /k/.¹¹ If (i) is right we cannot assume that in all Greek dialects the development of initial and intervocalic /kj/ and /tj/ went through an affricate stage and it could be that in the dialects where this happened we are dealing with a post-Mycenaean development; this could favour Crespo's view that Myc. <z> represents /t^(h)t'/ or /d^(h)d'/ but does not prove it. The second point could favour the same conclusion but again we cannot know how traditional some spellings are. The conclusion is that, as Risch and Hajnal clearly show in their account (see n. 4), certainty cannot be reached.

At this point it may be useful to look again at the etymology of *za-we-te*, 'this year'. Nobody ever doubted that this was formed by *-wetes* preceded by *k_iā-, but while the *ki- pronominal and deictic element was known, *k_iā- required explanation. It is normally stated that the starting point was the analogy with *k_iāmeron, 'today', formed from the pronominal *ki- 'this' and the Greek word for 'day'.¹² In other words *k_iā-

A. BARTONĚK, "On the sources of juxtavocalic s in Mycenaean", in *Tractata Mycenaea* (1987), p. 41-48 (with references to earlier work); C. BRIXHE, *Phonétique*, cit. (n.5), p. 53-60 and passim.

¹⁰ For a better formulation see M. DEL FREO, "*Pa-sa-ro*", cit. (n.3).

¹¹ J. MÉNDEZ DOSUNA, "On <Z> for <Δ> in Greek dialectal inscriptions", *Die Sprache* 35 (1991-93), p. 82-114; ID., "A note on Myc. *a-ze-ti-ri-ja*, Att. σβέννουμ and palatalization", *Die Sprache* 35 (1991-93), p. 208-20. Cf. also C. BRIXHE, *Phonétique*, cit. (n.5), p. 62-63 and for Cretan p. 93-110.

¹² See most recently R. BEEKES, *Etymological Dictionary of Greek*, Leiden-Boston 2010, p. 1481.

would have been resegmented from there and extended to form **kīāwetes* 'this year'. However, it is not normally explained why the new compound was not **ki-wetes*. In 1985 I argued that the analogical process, a pre-Mycenaean one, was only possible if **kīāmeron* had no longer this form (from which **ki-* would have been recoverable), but had already become /tsāmeron/ or /tšāmeron/; in this case the creation of a new **/tswetes/* or **/tšwetes/* could have created phonetic problems and a resegmentation and extension of /tsā-/ or /tšā-/ was more plausible. In its turn then *za-we-te* would show that the correct interpretation of the z-signs excluded a value /kj/ or even /tj/, even if etymologically justified.¹³ *Mutatis mutandis*, one could make the same point in order to support a form of the /k'āmeron/ or (more likely) /t'āmeron/ type. In that case, however, we ought to ask whether /k'āmeron/ or /t'āmeron/ would not have been perceived as compounds of /ki~/ /kj/ or /ti~/ /tj/, since presumably there was no contrast between e.g. /tjV/ and /t'V/.

If the premises are correct the point is still valid, I believe, and a /kjā/ reading of <za> ought to be excluded but very recent work by Brent Vine has thrown doubts on the whole formation creating a further level of uncertainty. For Vine Myc. *za-we-te*, Gr. τῆτες would result from the univerbation of an inherited phrase **kīeh₂-eh₁* **uet-es*, 'here/now, during the year', i.e. 'during this year', where **kīeh₂-eh₁* (> **kīā-*) is an original perlativ-instrumental from **kīo-* and is used adverbially. In other words for Vine the first element of *za-we-te* would be a **kīā-* form, which would remove the need to think in terms of analogy with τῆμερον.¹⁴ If that is so - and the problem is that we do not have parallels - my point, though sound, is no longer relevant. However, it is not clear as yet whether Vine's suggestions will be generally accepted. In general terms it is still important to note that some forms of analogy require specific phonological interpretations and consequently a study of analogical processes is relevant to our understanding of phonology.

4. The labialized signs

Another example may or may not be valid, but exploits a similar line of argument for different purposes. It is well known that Linear B in addition to the standard syllabary has a set of specialized signs, some of which at least can be classified into two categories: signs with originally palatalized consonants (*ra₂*, *ro₂*, etc.) and signs with originally labialized consonants (*twe*, *dwe*, *two*, *nwa*, etc.); the z-signs could belong here. The normal assumption, first stated by Palmer,¹⁵ is that the pre-Greek language of Crete had in fact sets of palatalized and labialized consonants marked in Linear A by special signs. Linear B would have then adopted these signs to indicate either palatalized and labialized consonants (e.g. [r'] , [k^w]) or clusters of consonant and [j] or consonant and [w]. The

¹³ A. MORPURGO DAVIES, "Mycenaean and the Greek language", in *Linear B: a 1984 survey*, A. Morpurgo Davies and Y. Duhoux (ed.), Louvain-la-neuve 1985, p. 75-125 at p.106 n. 14.

¹⁴ B. VINE, "A yearly problem", in *East and West. Papers in Indo-European Studies*, K. Yoshida and B. Vine (ed.), Bremen 2009, p. 205-224. I have not seen the (unpublished?) paper by Timothy Barnes quoted by Vine p. 206 which discusses related problems and makes some good points about the possible parallelism with compounds of numerals with 'day' and 'year' (see Myc. *di-wi-ja-me-ro* /*dwi-āmeron*/, TH Gp 109.1).

¹⁵ L. R. PALMER, "Observations", cit. (n.8); *Interpretation*, p. 39.

development must have occurred before our first texts if one is to judge from the *pte* sign which is normally taken to belong with the palatalized signs, since an earlier **pje* would have yielded in Greek *pte*, which is in fact the actual value in Linear B.¹⁶ In view of their vocalism it is not surprising that the signs for *ro*₂ or *dwo* and *two* are not found in Linear A, but it is significant that both *ra*₂ and, as we now know,¹⁷ *nwa* are attested in Linear A, though we do not have independent evidence for their value. In the case of *nwa* (which so far is not matched by *nwo* or *nwe*) we have alternations like *pe-ru-si-nu-wa* / *pe-ru-si-nwa* (see below) or the god's name *e-nu-wa-ri-jo* (KN) vs. the MN *e-nwa-ri-jo* (PY). The question then arises whether <*nwa*> is simply a short-hand spelling for <*nu-wa*> (or even **<na-wa>*), and represents the sequence /*nwa*/, with long or short /a/, or it indicates a labialized nasal [n^w], in agreement with what may have been its original value. If [n^w] actually existed, I assume that it could have also been written <*nu-wa*>.

The idea that <*nwa*> may indicate a labialized nasal may seem farfetched but if we look at the evidence for the sign it is striking that the majority of the examples, a) comes from KN, b) does not seem to have a Greek etymology. If we exclude the various spellings of *pe-ru-si-nu-wo* the complete list is as follows:

KN *a-di-nwa-ta* (MN), *a-mi-nwa*[(MN) (cf. PY MN *a-mi-nu-wa-ta* ?), *da-nwa* (?GN Gg 701), *]ḏa-nwa*[(? Dv 9587), *]ḏa-nwa-re* (MN), *]nwa* (MN), *]nwa-jo* (MN), *[.]nwa-jo* (MN), *]nwa-re*, *qa-nwaso* (?PN, DI 943, hand 118) vs. *qa-nu-wa-so* (MN, As 1516.17, hand 101), *qi-nwa-so* (MN);

PY *e-nwa-ri-jo* (MN vs. KN *e-nu-wa-ri-jo* GN), *ti-nwa-si-jo* / *ti-nwa-si-ja* / *ti-nwa-ti*[, *ti-nwa-ti-ja-o* (ethnic)

TH *]sa-nwa-ta* (MN Av 106.6).

The adjective *pe-ru-si-nu-wo* /*perusinwos*/, 'of last year', Gr. *περυσινός*, occurs six times at Pylos in this form, but its dossier also includes the nom. (?) sing. fem. *pe-ru-si-nwa* KN So 4442 (Hand 131), the nom. plur. fem. *pe-ru-si-nu-wa* PY Ub 1316 (hand 32) or *?pe-ru-si-]nwa* KN Dp 7742; the nom. plur. neut. *pe-ru-si-nu-wa* PY Ma 126.1; 225.2 (hand 2) or *pe-ru-si-nwa* MY Oe 111.1 (hand 51); Ue 652.2,4B; the gen. plur. fem. *pe-ru-si-nwa-o* PY Ub 1317 (hand 32).

Περυσινός is well known and is clearly of Indo-European origin, but before the decipherment it was normally interpreted as an *-(i)no-* derivative of the locative **peruti*, 'last year'. The appearance of *-w-* in the termination was a surprise, but as early as 1957 Lejeune suggested that it was due to analogy with *ne-wo* 'new', Gr. *νέος*.¹⁸ If so, we would be dealing with a relatively recent creation and it would seem likely that an analogically introduced and transparent */-wos/* would have kept its morphemic identity. In other words *pe-ru-si-nu-wa* and *pe-ru-si-nwa* would speak for a phonological [nw] cluster rather than for a labialized nasal. That such a cluster existed in Mycenaean is shown by the alternative spellings of the Mycenaean equivalent of Gr. *ξένιος*: PY *ke-se-ni-wi-jo*[, KN *ke-se-nu-wi-ja*, KN *ke-se-ne-wi-ja*. On the other hand Brent Vine has raised a number

¹⁶ Cf. the alternation between *pe-te-re-wa* (KN So 894.1) and *pte-re-wa* (KN Se-, So- passim).

¹⁷ Cf. SY Za 4 in P. MUHLY and J.P. OLIVIER, "Linear A inscriptions from the Syme Sanctuary, Crete", *AEphem* 2008, p. 197-203 at p. 207-08 and p. 216.

¹⁸ M. LEJEUNE, *Mémoires* I, p. 260, n. 16.

of objections against Lejeune's suggestion and in his turn has proposed that /perusinwos/ is a /wos/ derivative of an adverbial *perutim, '(from) last year', characterized by an ablatival *-m marker.¹⁹ We can conceive of the new formation as derived from *perusimwos (with *-mw- > -nw-) or as built directly on the later *perusin. In either case, and more so in the second, we are dealing with a recent creation built with an identifiable suffix, so that for both *pe-ru-si-nu-wo* and *pe-ru-si-nwa* a [nw] cluster is plausible. This does not of course exclude that <nwa> may be used to indicate a labialized nasal in words of pre-Greek origin. It is possible that some of the KN words did indeed continue older Cretan names with labialized nasals and preserved that pronunciation, but even in KN we find sufficient evidence (*pe-ru-si-nwa*, *ke-se-ne-wi-ja*) for a /nw/ cluster for which the <nwa> sign could be used. The conclusion then is that, though originally <nwa> may have indicated a labialized nasal, by the time of our documents it is likely that it was used (exclusively or otherwise) for a cluster of nasal + /w/. This may have interesting consequences for the other so called labialized signs such as <dwo> which were added to the inherited ones. Why were they created? Of course it may have been merely a matter of expedience: it is faster to write one sign than two. But there may have been more. First, the existence of a model. Secondly the fact that, as the spellings of /ksenwios/ indicate, there were hesitations in knowing how to indicate these clusters - whether these had to do with problems of perceived syllabification is a matter which would be too long to discuss here. Whatever the answer, the fact remains that, if Linear B inherited signs for a labialized series, it certainly added to the inherited forms. The same may have happened with some signs of the palatalized series to which we now turn.

5. The ra_2 and ro_2 signs.

Mycenaean has two signs, ra_2 (KN, PY, MY, TH) and ro_2 (KN, PY, TH), which are regularly quoted as instances of palatalized signs. Alternations of the type PY *a-ke-ti-ri-ja* / *a-ke-ti-ra_2* which are frequent in feminine *nomina agentis* were initially taken to demonstrate that $ra_2 = rja$ since this allowed a reading /tria/ or /trja/ for both spellings.²⁰ In 1965 Lejeune firmly treated ra_2 and ro_2 as 'complexes' and not as 'doublets', once again assuming that the two spellings <ri-ja> and <ra_2> were as equivalent as e.g. <pe-

¹⁹ B. VINE, "Att. ΠΕΡΥΣΙΝΩΣ (Myc. *pe-ru-si-nu-wo*) 'last year's'", *Alessandria* 3 (2009), p. 3-12.

²⁰ For the evidence see the Lejeune articles quoted in n.21 as well as A. HEUBECK, "Remarks on the sign-doublets ro_2 , ra_2 , ta_2 ", in *Colloquium mycenaicum*, p. 239-257; A. LEUKART, "Les signes *76 (ra_2 , <<rja>>) et *68 (ro_2 , <<rjo>>) et le nom du grand prêtre de Poséidon (sinon du roi) à Pylos", in *Mykenaiika*, p. 388-405; C. BRIXHE, *Phonétique*, cit. (n.5), p. 72-83; I. HAJNAL, *Sprachschichten*, cit. (n.3), p. 180-197. For the fem. *nomina agentis* it is important to consider their distribution: both *-ti-ra_2* and *-ti-ri-ja* are frequent in Pylos; Knossos has only one obscure example of *-ti-ra_2* vs. various examples of *-ti-ri-ja*; Thebes has twice the dat. sing. *a-ke-ti-ra_2* and Mycenae has two examples of *-ti-ri-ja(-)*. At Pylos two scribal hands write *-ti-ra_2* (nom. plur.) and *-ti-ra_2o* (gen. plur.); the others write *-ti-ri-ja* (nom. plur.) and *-ti-ri-ja-i* (dat. plur.). Only one hand (hand 1), which normally writes *-ti-ri-ja*, also writes *-ti-ra_2*, but this happens only once (nom. plur. *a-ke-ti-ra_2*). In what follows I refer to /r/ all through but obviously these signs also stand for /l/ values.

te> and <*pte*>.²¹ There are no instances of alternations *ri-jo* / *ro*₂, and the value of *ro*₂ was determined etymologically. A comparative like KN *a-ro*₂-*a* 'better' (nom.-acc. plur. neut.) derives from **ar-jos-a*; the form *po-pu-ro*₂ of KN L 758 is likely to be an adjective formed with the /-io-/ suffix and matched by the alternative formations *po-pu-re-jo* / *po-pu-re-ja* (KN X 976.1a, L 474). At the same time the alternations *ku-pa-ro* (KN) / *ku-pa-ro*₂ (PY), 'cyperus', *ku-pa-ro-we* (PY), 'flavoured with cyperus' guarantee that the sign has a value close to that of <*ro*>, i.e. it includes a liquid consonant. The evidence is circumstantial, but sufficient to support the general assumption that *ra*₂ and *ro*₂ reflect the same consonant(s). As expected, *ro*₂ does not find a match in Linear A but *ra*₂ does and may support the theory that there was a palatalized series of consonants in the pre-Greek language. However, here too we are confronted with the question of the synchronic value of these signs at the time of our texts.

The etymologies are not in doubt: *ra*₂ and *ro*₂ may, though, as we shall see, need not, indicate etymological /*rja*/ and /*rjo*/. On the other hand it was clear from the start that /*rja*/ and /*rjo*/ may have been the initial values of the two signs but either they were not their only values or the values had changed in time. First, there are alternations between *ra*₂ and *ra*, *ro*₂ and *ro*. One and the same man is called *ta-ra-to* in PY Eo 247.6 (Hand 41) and *ta-ra*₂-*to* in PY En 74.15 (Hand 1)²² and we have seen above the alternation between *ku-pa-ro*₂ and *ku-pa-ro*. But the clearest and most quoted example of a non palatalized value (and etymology) is *a-ke-ra*₂-*te* in the heading of PY Vn 493.1 *a-ke-ro e-po a-ke-ra*₂-*te*. This is generally acknowledged to be an aorist participle, perhaps from the verb ἀγείρω. If so, we need to go back to a form **agersantes* with etymological *-*rs-* rendered by *ra*₂. The most likely hypothesis - not a new one - is that here *ra*₂ indicates an assimilated /*rra*/ < *-*rsa-*. If that is so, however, a /*rja*/ value of *ra*₂ is excluded, for this word at least. This was pointed out as early as 1958 by Carlo Gallavotti and in 1966 Lejeune (see n. 21) accepted a /*rra*/ or /*r'r'a*/ value, but only for some words. In 1972 Ruipérez supported this view in the context of his general theory of palatalization and gemination, but all these authors (with the exception of Gallavotti) did not call in doubt the phonetic equivalence of -*ti-ri-ja* and -*ti-ra*₂.²³ Later on Lejeune returned to the question in the context of an inquiry into the differences between the graphic and linguistic features of Mycenaean and those of what he called *proto-mycénien* (the period between the creation of Linear B and the compilation of our texts). In his view the original form of the feminine agent suffix was /-*tria*-/ and the original values of *ra*₂ and *ro*₂ were /*rja*/ and /*rjo*/. Initially these signs were indeed 'complexes', but phonetic changes which had taken place after the creation of the script had led to /*rra*/ and /*rro*/

²¹ M. LEJEUNE, "Doublets et complexes", in *Cambridge Colloquium*, p.135-149, at p. 145-149 (= *Mémoires* III, p. 89-104, at p. 100-104); see also M. LEJEUNE, "Pré-mycénien et proto-mycénien", BSL 71 (1976), p. 193-206 (= *Mémoires* IV, p. 197-212).

²² *ta-ra-to* also occurs in PY An 192.10 (Hand 22) and *ta-ra*₂-*to* in PY En 659.6.10.13 (Hand 1) and Eo 351.2, [Eo 444.5], Eo 471.2 (all by Hand 41).

²³ C. GALLAVOTTI, "Il carattere eolico del greco miceneo", *RFIC* 36.(1958), p. 113-133 at p. 122-30 for *ro*₂ = *rro*/*llo* and *ra*₂ = *rra*/*lla*; p. 128 for -*ti-ra*₂ = /-*tirra*/ and -*ti-ri-ja* = /-*trija*/. M. S. RUIPEREZ, "Le dialecte mycénien", in *Acta mycenaea* I, p. 136-169, at p. 160-163. Cf. A. HEUBECK, "Remarks", cit. (n. 20) and I. HAJNAL, *Sprachschichten*, cit. (n. 20).

values and to a /-tirra-/ form of the suffix; the traditional spelling of the <ti-ri-ja> type had survived without corresponding any longer to an actual /-tria-/ or /-trja-/ pronunciation.²⁴ However, a turning point was marked by Heubeck (see n. 20) who in his 1975 paper at the Neuchâtel Colloquium tackled the question head on. For him the plural nominatives of the *a-ke-ti-ra*₂ type had to be read /-tirrai/ or /-tir'r'ai/ and were linked to a nom. sing. *-tirra or *-tir'r'a from an earlier *-ter-ih₂. On the other hand in his view the indirect cases like the dat. plur. PY *a-ke-ti-ri-ja-i* /askētriā(h)i/ originally had /-trijā-/ forms from *-tr-ijeh₂. Heubeck also pointed out that the contrast /tirra/ ~ /trijā-/ matched the Greek contrast between Aeolic -τερρα (possibly reflected in Homeric -τεῖρα) and -τριῶ or -τρις found in the majority of the other dialects. Admittedly Mycenaean had both nominatives in -ti-ra₂ and nominatives in -ti-ri-ja but this could be due to scribal confusion or to paradigmatic levelling. Some points may be challenged,²⁵ but the basic ideas that -ti-ra₂ and -ti-ri-ja represent different phonetic realities, that they may go back to different forms of the same paradigm,²⁶ and that they both correspond to forms attested in alphabetic Greek, are still valid. They reappear in Leukart's paper at the Athens Colloquium (cf. n. 20) and in the detailed account published by Hajnal in 1997 (cf. n. 20). Heubeck could not account for the *-e- > -i- change in the shift from his reconstructed *-ter-ia to Mycenaean /-tirra/; Leukart ("Les signes", cit. (n. 20), p. 401) and then Hajnal (*Sprachschichten*, cit. (n.3), p. 189) argued that for the relevant proto-stage we must reconstruct not *-ter-ih₂, but *-tr-ih₂, which in its turn yielded a proto-Greek *-tr-ja; the change from *-tr-ja to /-tirra/ (rather than e.g. */-taira/) is due to the development of an anaptyctic vowel. For this phonetic development Hajnal found a parallel in the denominative verb οἰκτίρω from *oiktr-īō (cf. Gr. οἰκτός, 'pitiable'). The comparison is phonologically perfect and the only problems are, a) whether this development was limited to * -tr-ja- sequences which followed a consonant or also occurred when they followed a vowel, b) what the modalities of the change were. The first question does not need to detain us much; if the change of *-tr-ja to /-tirra/ or /-tir'r'a-/ happened after consonant only, forms like PY *pe-ki-ti-ra*₂ /pektirra-/, 'wool carders', *ra-pi-ti-ra*₂ /raptirra-/, 'sewing women', etc., could have provided a model for e.g. *me-re-ti-ra*₂ /meletirra-/, 'corn grinders'. Unfortunately it is unlikely that we shall ever have the evidence to decide. The second question is more difficult. Hajnal, as we have seen, accounts for the /i/ as due to the insertion of an anaptyctic vowel in *-tr-ja and links it to other sporadic occurrences of /i/ ("schwa graecum"), where we expect a zero grade, while rejecting the possibility of a syllabification [-tr̥sja] with a secondary vocalic /r̥/. It is

²⁴ Following from Lejeune, C. BRIXHE, *Phonétique*, cit. (n. 5), p. 80 suggests that the feminine nomina agentis can be read as /-tra-/ with a further change similar to that found in e.g. in the Boeotian PN Δαματρα = Δαματρία. This is of course possible but, as Brixhe himself states, it would be easier to accept if we found an instance of a <-ta-ra-> spelling.

²⁵ See M. PETERS, *Untersuchungen zur Vertretung der indogermanischen Laryngale im Griechischen*, Wien 1980, p. 217; I. HAJNAL, *Sprachschichten*, cit. (n.3), p. 180-197. A difficulty which applies even to the later suggestions by Leukart and Hajnal (see note 27) is that the -ti-ra(-) spelling is found both in the nominative and in the genitive plural, i.e. we must allow for some form of levelling.

²⁶ This is not accepted by M. PETERS, *Untersuchungen*, cit. (n. 25), p. 215-217, but see I. HAJNAL, *Sprachschichten*, cit. (n.3), p. 188, 190.

difficult to pinpoint the difference between these two possibilities but an /ir/ treatment of a palatalized /r'/ in vocalic function would not be surprising. In any case one may tentatively generalize from this evidence and argue that in Greek a sequence *C₁C₂riV, where C₁ and C₂ are stops, regularly developed into /C₁C₂irjV/ or /C₁C₂ir'r'V/ and eventually into /C₁C₂irV/ or /C₁C₂irrV/.²⁷ Counterevidence could be provided by, a) the feminines in -αιρα, b) the adjectives or nouns in -αιρος and, c) the denominative verbs in -αιρω, if they go back to sequences of * -CCria, * -CCrios, and * -CCrie/o-. To judge from the reverse dictionaries, neither a) nor b) include forms where -αιρα and -αιρος are preceded by two stops; for the verbs we are referred to έχθαίρω (Hom.), 'hate', πταιίρω, 'sneeze' and φθαίρω, 'destroy'. Of these πταιίρω or rather πταιίρομαι is late and replaces an earlier πτάρνυμαι, φθαίρω is a late Doric gloss for φθείρω. The έχθαίρω present looks like an old denominative from έχθρός but the whole formation is far from clear and could be post-Mycenaean.²⁸ In other words none of these forms contradicts the original suggestion. It would be possible to go even further and argue that the outcome of *-riV after consonant (not necessarily two consonants or two stops) is regularly *-iria. Here the counterevidence would mainly consist of the numerous feminines in -αιρα and a discussion of all of them would take us too far. However, it is notable that in practically all instances M. Peters (*Untersuchungen*, cit. [n.25], p. 154 ff.), who has studied them, has shown that the -a- vocalism is based on the vocalism of related forms and I. Hajnal (*Sprachschichten*, cit. [n.3], p. 189-92) has reached similar conclusions. In other words it may be possible to conclude that the regular treatment of /-CrjV/ is /-Cir'r'V/ or /-CirrV/. This could be formulated somewhat differently, stating that /r/ in this position is palatalized and acquires a secondary vocalization which then yielded /ir/. If so, we would not expect to find /CarjV/ from these sequences.

Let us now return to our starting point. The Linear B writing rules would allow us to argue that <ra₂> and presumably <ro₂> are indeed 'complexes', i.e. they ought to be transliterated <rja> and <rjo>, thus allowing us to have a unitary /-tria-/ or /-trja-/ reading of the -ti-ra₂ / -ti-ri-ja suffix. The compelling evidence against this is the aorist participle a-ke-ra₂-te which speaks for /rra/ and presumably /rro/.²⁹ This and the supporting data may lead us back to Lejeune's suggestion that the tablets make use of a traditional <-ti-ri-ja> and a modernizing <-ti-ra₂> spelling, but probably at the time when they were written only the 'modern' pronunciation /-tirra-/ was in use. On the other hand on this scenario both the extent of the alternation and the lack of a -ti-ra- spelling are surprising. The alternative is to accept that <-ti-ri-ja-> and <-ti-ra₂-> refer to different

²⁷ A. LEUKART, "Les signes", cit. (n. 20), p. 403 and I. HAJNAL, *Sprachschichten*, cit. (n.3), p. 193 suggest that the epic -τερα is an indirect continuation of the Mycenaean /-tirra/, which seems highly plausible. Cf. also I. HAJNAL, "Die Vorgeschichte der griechischen Dialekte", in *Die Altgriechischen Dialekte. Wesen und Werden*, I. Hajnal (ed.), Innsbruck 2007, p. 131-156, at p. 151-152.

²⁸ Cf. T. MEISSNER, *S-stem nouns and adjectives in Greek and Proto-Indo-European*, Oxford 2006, p. 48-49.

²⁹ There is no reason to see any form of palatalization in this participle but this does not prove that ra₂ = /(r)ra/. It is conceivable (though unprovable) that one and the same sign indicates e.g. /rra/ and /r'r'a/. On the other hand the spelling by itself does not exclude the possibility that we are dealing with /agērantēs/, in which case ra₂ would represent /ra/.

forms: *-trija/* and */-tirra-/*. We have to choose between the two interpretations and the final decision is left to morphology: is it likely, possible or plausible that the Mycenaeans used two different forms of the same suffix? We owe to Heubeck, Leukart and Hajnal the reconstruction of a series of developments which allows us to give a positive answer to this question. Certainty is not possible, but their account is very attractive.

6. Mycenaean between PIE and alphabetic Greek

Our knowledge of Mycenaean phonology is inadequate, partly because the script does not tell us what we want to know, partly because we do not have sufficient texts. I have tried to argue that sometimes morphological data may supplement the information which we obtain from the written text. When we actually succeed in establishing a plausible phonological interpretation of the data we normally see that the tablets point to a linguistic stage which is remote from reconstructed PIE but is also different from the alphabetic Greek of the First Millennium. Unfortunately that is exactly the stage for which, if we have no concrete evidence, we do not know what to expect and may resort to unreliable forms of guessing. Opinions will differ and we naturally percolate towards what we know or think we know: the Greek of the First Millennium at one extreme, reconstructed Indo-European at the other. This means that we tend to adopt interpretations which are more 'Greek-like' or more 'PIE-like', depending on our own knowledge, inclination and intuitions. No harm is done and obviously we cannot write pages and pages of explanation every time that we need to refer to the nominative singular of the *ēu*-stems (*/-ēus/* or */-eus/*) or to the verb 'to have' (*/hek^hei/* or */ek^hei/*). A form of short-hand is necessary, but we must make sure that when we adopt a conventional transcription we make this explicit and we do not start to build further conclusions on it ourselves or allow others to do so. From this point of view a healthy anarchy may be confusing but at least it is not deceptive.