

ON DUAL NUMBER OF *a*-STEMS IN THE MYCENAEAN TABLETS *

A detailed examination of the tablets and of the direct and inverse indexes, carried out with the intention —now discarded— of making up an exhaustive grammatical index, led me to six tablets showing eleven possible examples of nominative-accusative dual with final *-a* for the so-called first declension, apart from those forms in *-o* and *a-e* (e.g. *to-pe-zo*, *e-qe-ta-e*) which are already known and accepted.

I must begin by declaring myself to be even more sceptical than any reader of this note could possibly be: I recognize the highly hypothetical nature of my tentative affirmation, for whose defense I will invoke no general considerations other than the following: firstly, the number of possible examples strikes me as high and quite safe from the reproach that *una rondine non fa primavera*; secondly, although the tablets show errors that are purely mechanical or anacolutha, as well as examples of syntactical laxity which may easily be explained by the type of text we are concerned with and by its composition and *style*, I believe that a generalized, a priori recourse to mechanical error, anacolutha and laxity may be mistaken and may explain away so much that it may in fact not be explicative of anything at all.

Thirdly, although there may be evidence to support it, the generalized, a priori argument that the entry of lexical and grammatical forms takes place before and even without previous knowledge of the numerical entries does not seem to me to be applicable either. On the contrary, the general norm in the tablets seems to be a conscious and regular wish to mark the grammatical categories with their corresponding accidence and, therefore, the

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concordance of the grammatical forms among themselves and the coherence between them and the numerical entries.

However, I am the first to admit that the pondered and singularized application of these arguments —error, anacoluthon, syntactical laxity, lack of coherence between grammatical form and numerical entry— may rebut, one by one, the eleven examples we are about to discuss.

I should like to add that the inadequacy of the script presents a double front against our hypothesis: in the first place, the deficiencies of the writing system favour the interpretation of these forms in *-a* as singulars or plurals which will in either case act —as opposed to the dual— as *zero*, unmarked terms which are *generic* both in the case of anacolutha and lack of coherence. Secondly, if we, or anyone else, had discovered a *possible* dual form ending in *-ra₃* (e.g. **di-pte-ra₃ 2*), the most orthodox and handy thing to do would be to see in it, despite the entry 2, a plural form ending in *-ai*.

In any case, if all the examples presented here are not to be discarded, our hypothesis, even falling short of the target, could be rescued and used as proof that the gradual elimination of the dual number and its forms —with the inevitable sequels which are detectable in the Homeric language— had already begun in Mycenaean times, at least in the dialect and/or level documented by the tablets. Needless to say, twenty-seven years of Mycenology with authorities in the subject of morphology —Chadwick, Lejeune, Ruijgh, Szemerényi, etc.— who have paid no attention to the examples that I am now offering, and the fact that they have expressly rejected them in certain cases, do nothing to dampen my optimism regarding the favourable echo that this note may arouse.

The examples detected are as follow:

1) and 2) PY Ab 745.B and 746.B (*hand* 21 in both tablets)

745.B pa-ke-te-ja , ri-ne-ja MUL 2 ko-wo 1 NI T 5

(PTT I, p. 29: *-ja* of *pa-ke-te-ja* over [jol])

746.B pa-ke-te-ja , ri-ne-ja MUL 2 ko-wa 1 NI T 5

(PTT I, p. 29: MUL 2 corrected from MUL 3)

Arguing over the corrections made on either tablets may merely lead us to the dead end of conjectures, all probable and none

undisputed, regarding the process of writing down or writing out, and in particular regarding the order in which the forms and quantities were known about and entered; perhaps the corrections in Ab 745 and in Ab 746 may have a different, opposite meaning.

In 745 it would seem difficult to prove that *pa-ke-te-jo*, now corrected, has already been noted down as a masculine caused by the inclusion of *ko-wo*; if what was important to the scribe was the consignment of rations in different quantities for adults and children, it is unlikely that he would lump the two women and the boy together under a masculine gender.

It would perhaps seem less likely that, after having written a feminine dual *pa-ke-te-jo*, the scribe should then go on to correct it by making it a dual in *-a*. The *lectio facilior* should be to see in *-jo* a simple mechanical error, and its corrected form *-ja* (and *ri-ne-ja*) as a plural not consistent with the entry MUL 2.

In Ab 746 the correction works against our hypothesis: the scribe has written down the plural, which he leaves uncorrected, limiting himself to the correction of the numerical entry. He has perhaps noted down MUL 3 as it included the *ko-wa*.

The idea that *pa-ke-te-ja* and *ri-ne-ja* are singulars should be rejected with regard to both tablets, as well as the idea that MUL 2 is the result of adding $1 \text{ } pa-ke-te-ja + 1 \text{ } ri-ne-ja$. An analysis of the Ab series excludes double occupational or trade names and demands that the working women be qualified by an occupational or trade name and with their ascription to a place or an owner: in both tablets it seems evident that *ri-ne-ja* is a trade name and excludes *pa-ke-te-ja* from so being: *pa-ke-te-ja* must be interpreted as an adjective describing an ethnic group, or, more probably, one referring to an owner (e.g. *pa-ke-ta*).

[Note.—In view of the interpretations given for *pa-ke-te-ja* as a trade name related to linen-work, I would like to ask —albeit with the greatest reserve— whether if instead of finding it attached to *ri-ne-ja*, we could not expect an adjectival compound, either **ri-no-pa-ke-te-ja* or something of the sort.

The etymology and the interpretation of *ri-ne-ja* seem as clear as the specification of the work undertaken is indefinite].

3) PY Ub 1318 (*hand* 32)

.1 ... di-pte-ra 4 [. . . .]di-pte-ra 2 ...

.5 ... e-ra-pe-ja , e-pi-u-ru-te-we , E 2

(PTT I, p. 240: .5 -pe-ja perhaps over erasure)

[Note. *hand* 32 —PY Ub 1316, 1317 and 1318— never writes the sign ra_3 in those cases where *di-pte-ra* should be plural].

In the Ub series we find issues of hides and skins (in the nominative) for the making of specific articles (in the dative of purpose; or is it in the appositional nominative in some cases?). An analysis of the Ub series and the place in which it was discovered (NE Workshop) lead one to suppose that the scribe had the hides or skins in front of him, knew the quantities of each issue when he noted down, but —of course?— this does not exclude the argument that the scribe's writing down of the word took place previously to his knowledge of the quantities.

The interpretation of *e-ra-pe-ja* and *di-pte-ra* seems to be easy and unanimously accepted; *e-pi-u-ru-te-we*, being an *eu*-stem, must be masculine: if it is considered as a plural (or dual!) nominative, it will be in apposition with respect to *e-ra-pe-ja*; it would perhaps be better to take it as dative singular, as a dative of purpose.

4) KN Ap 618 (*hand* 103).1 a-pe-a-sa / i-ta-mo , 'do-ti-ja' , MUL 1 ki-nu-qa
'*56-ko-we' MUL 1 [.2 ti-wa-ti-ja / a-*79 'a-no-qo-ta' MUL 3 ko-ma-we-to
MUL 2 we-ra-te-ja MUL 2 [

It is clear that *we-ra-te-ja* MUL 2 is a feminine adjective of the first declension. It is not so clear whether it describes an ethnic group, a trade, or very possibly, an owner (cf. *we-ra-to*, man's name in KN De 1136.B; in KN Ak 784.1 —*hand* 102— we have the variant spelling *we-ra-ti-ja*):

I must apologize for my ingenuity in interpreting the tablets in the following manner:

1. Absent(s): *i-ta-mo* (WN, nom. sing.) in? *do-ti-ja* (PN, loc. sing., or nom. sign.?) MUL 1; *ki-nu-qa* (WN, nom. sing.) in?

*56-*ko-we* (PN, in nom. sign.; the locative value is not morphologically explicit) MUL 1.

2. *ti-wa-ti-ja* (ethnic adjective, nom. plur.) of? *a-no-qo-ta* (MN: owner?, nom. for gen.: mechanical error or non-explicitation of the possessive value?) MUL 3; of *Komawens* (MN, owner's name in gen. sing.) MUL 2; *we-ra-te-ja* (a possible dual in *-a*) MUL 2.

5) and 6) KN Sd 4407 and 4415 (*band* 128 in both tablets)

4407.a CUR 2

.b *se-to-i-ja* , *mi-to-we-sa* , *a-ra-ro-mo-te-me-na*

4415.a CUR 2

.b *i-qi-jo* , *mi-to-we-sa* , *a-ra-ro-mo-te-me-na* , *a-ja-me*
(*sic*)

There is little to be said regarding the interpretation of the tablets: together with Sd 4401 —where the dual forms in *-o* bear out the CUR 2 restitution— these are the only tablets from Sd (and Se) in which two chariots would seem to have been recorded. The *i-qi-jo* form in Sd 4415 and the duals of Sd 4401 appear to rule out the ideas that the words were written before the quantities were known. The contrast between *i-qi-jo* and the forms following it in Sd 4415 is of crucial interest.

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So much for the six tablets (PY Ab 745 and 746, Ub 1318; KN Ap 618, Sd 4407 and 4415) with the eleven possible instances of the dual nominative-accusative in *-a* for nouns of the first declension: *pa-ke-te-ja* (2), *ri-ne-ja* (2), *di-pte-ra*, *e-ra-pe-ja*, *we-ra-te-ja*, *mi-to-we-sa* (2) and *a-ra-ro-mo-te-me-na* (2).

The following observations could be added:

a) these examples are documented in Pylos and Knossos. There is no documentation for Mycenae, Thebes and Tiryns.

b) the three tablets from Pylos and those of the Sd series from Knossos show two examples each, and only in KN Sd 4415 does the *-a* form compete with the already accepted *-o* form; the scribe 128 of Knossos gives us: only *-o* (4401), only *-a* (4407), *-o* and *-a* (4415).

c) eight of the eleven examples are limited to two scribes: 21 at Pylos and 128 at Knossos.

d) ten of the examples are adjectives, whether used as substantives or not; there is only one substantive, *di-pte-ra*. Perhaps it is also remarkable that the variation between *-o* and *-a* is found in *i-qi-jo*, which is completely (?) substantivated, as compared with those that have not been substantivated, or at least not to the same degree.

The above observations have been made on the presumption that our hypothesis is correct; we do not aspire, naturally, to reinforce it, not to attempt anything further than to attract attention to the fact that the documentation presented, in its own right and as contrasted with that which offers duals in *-o* and *-a-e*, allows us to make observations similar to those made regarding other *differences* within the language of the tablets; differences which, whether ascribable or not to *scribal hands*, could be interpreted as a *dialectal* ones, or as differences in *level* (standard as opposed to sub-standard), or as a mixture of the *inherited* and the *innovation* that gradually substitutes and eliminates it. It is easy to observe that the three interpretations are not mutually exclusive and may even be complementary: for example, the duals in *-a* could be a sub-standard archaism tied to individual or dialectal practice or preference.

If our hypothesis has seemed at all worthy of credit, let us go on to make a few observations of a morphological nature:

1) Up to the present we know that in Mycenaean Greek the *a*-stems have abandoned the inherited dual —nom.-acc. in **-ai*, because the plural nominative-accusative of these stems has in turn abandoned the inherited **-ās* form and has substituted it with *-ai*, which is parallel to the *-oi* of the *-e/o-* stems and comes from the pronominal inflection; Mycenaean Greek presents new forms of the dual nominative-accusative for the *a*-stems: in the feminine adjectives, whether substantivated or not, we have *-o*, which may be analogical of the thematic inflection or influenced by the numeral δύω/δύω; for masculine and feminine substantives, Knossos gives us *-a-e*, as a result of analogy with the athematic inflection, and in Pylos we have both *-o* and *-a-e* for fe-

minine substantives. If we follow Lejeune's views, the language of Pylos has either evolved more or is more innovatory than that of Knossos.

We believe in the possibility that Knossos and Pylos were still acquainted with the inherited dual in **-ai* in adjectives and feminine substantives of the first declension; we lack examples for masculine substantives.

2) Once one has recognized the survival of **-ai* duals, it could be considered that in the feminine *ko-wa*, nom.-acc. dual in PY Ab 372, 379, etc., there could be something more than a resorting to *graphics* to avoid confusion with the masculine *ko-wo*. An enlightening parallel which Pisani has emphasized is to be found in Latin: the feminine substantives such as *filia*, *liberta*, *equa*, &c maintain (they do not innovate, as they are athematic in origin!) the *-bus* ending in the dative-ablative plural because allowing *-īs* < **-ais* would do away with the necessary distinctions of gender (and sex).

3) There remains the possibility of interpreting the examples collected as forms ending in *-ā* which are analogical with *-ω* of the thematic inflection, but, apart from the scant feasibility of this interpretation, as it postulates a homophony which does not permit the distinction between singular and dual nominatives, we would have to separate these *-ā* duals totally from those that are later to be found in Homer (masculine nominatives only!) and in Attic: the Homeric and Attic data must go back to **-āε*, with *ā* resulting from a contraction and which is no longer affected by the change **ā* > *η*. The Homeric and Attic data are in full accordance with the Mycenaean dual ending in *-a-e*. [Note.—We could add that the hypothetical *innovation* *-ā* for the dual nominative has no chance of being, precisely because of its non-distinctness; however, we could admit the temporary coexistence of innovation (dual in *-o* and *-a-e*) and inherited form (dual in **-ai*) which will be eliminated as homophonic with the new plural in *-ai*; we must also admit the coexistence of homophonic forms (nom. sing. and nom. dual in *-ā*) which are maintained by the inflectional system, when the formal coalescence and homophony result from sound changes].

4) The hypothesis presented here, which favours the interpretation of *-a* as *-ai*, may be complementary with the well-

known Pisani hypothesis regarding the survival of the dual *-ai* in Homeric «plurals» such as *δοιαί*, *ἀμφοτέραι*, and those of nouns designating «coppie di cose»; I do not mean to say that one hypothesis confirms the other, but they do lend coherence to each other.

5) If our hypothesis appears to be viable, that put forward by Szemerényi would no longer be quite so feasible: in the Mycenaean duals ending in *-o* we would not recognize an inherited form **-oi*. We would hold to be a surviving, inherited from the one ending in *-ai*, which, because of the fact that it was undistinguishable from the plural, was eliminated in a process initiated before the date of the tablets and which was completed —leaving traces of *-ai* in Homer— before the beginnings of alphabetical Greek.

6) Lastly, we should like to point out that to presume that the duals ending in *-a-e* are the product of a *re-characterization* of **-ai* through the addition of the *-e* of the athematic inflection still poses several difficulties. In fact, I very much fear that postulating **-ai + -e*, that is, **-aye*, obliges one to think of a very early date for the process in question, a date before the (total) loss of the intervocalic *yod*, as we never come across the spelling **-a-je* in the tablets. [Note.—It seems clear that in Mycenaean Greek we find the process of the loss of the intervocalic *yod* to be either fully completed or in the intermediate stage of intervocalic aspiration; it would take too long —as well as being out of place— to discuss at this point the losses and retentions of secondary intervocalic *yod*, the possible analogical restoration of the primary *yod*, etc.]. I am not as reluctant as Ruijgh (1979) to admit of a hiatus in the Mycenaean *-a-e*, and, therefore, I do not believe it necessary to have recourse to the specific influence of the *-s/h-* stems in explaining the analogical origin of dual *-a-e*.

[Note.—For the interpretation of PY Ab 745 and 746, Ub 1318, and of the words found in these tablets, cf. Ventris-Chadwick, pp. 155 ff., 418 ff., 489 ff., Ruijgh 1967, §§ 214, 216, 320, Palmer, pp. 113 ff., Killen 1972 and 1979, Morpurgo].

ADDENDA. The comparison of KN Ap 618.2 *we-ra-te-ja* MUL 2 with Ak 784[+]8019 (*band* 102) brings to light another possible example of dual in *-a*: Ak 784.1: *]we-ra-ti-ja* []2.

And further examination of the Ak series allows us to add Ak 780 (*hand* 102), line 1: *da-wi-ja*, *ne-ki-ri-de* MUL 2, with *ne-ki-ri-de* which, judging from its form, could be a dual. The number of possible examples rises, therefore, to thirteen in eight tablets, and ten of these thirteen are limited to three scribal hands.

We are bound to admit in all objectivity, and against our hypothesis, that in the same Ak series and by the same scribal hand (103) an *apparent and inexplicable* masculine *dual* has been written down: *me-wi-jo* in 610.3; feminine *me-u-jo* in 5884, feminine *me-wi-jo* in 5940: these forms are, of course, plural, as compared with more abundant examples of dual (619, 627, 636, 782, 5741: *me-wi-jo-e*, *me-u-jo-e*, *me-wo-e*, whether masculine or feminine) that present no difficulty as to form whatsoever. That is to say that the *hand* 103 seems to waver between entries in the *generic* singular (or plural) and entries in which dual number is explicitated by the lexical form, as well as by decimal notation.

Very probably, the analysis of other tablets and scribal hands would give similar results; results which would be unfavourable to what is presented in this paper, but in Mycenology it is never too much to leave a door open to new, heterodox views.

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