

ON SOME RECENT ATTEMPTS TO IDENTIFY LINEAR A MINOAN LANGUAGE

1. *Confusion and uncertainty*

Many attempts have been made to 'identify' the Minoan language of the Linear A documents, that is to connect it to other (better known) languages or linguistic families.

Not to speak of the oldest approaches, unsuccessfully arisen nearly after the Linear B decipherment (C.H. Gordon, L.R. Palmer, S. Davis and others), in recent times there has been a new¹ 'wave' of identifications, among which those of Owens and Aartun: their extreme weakness, from a methodological point of view, is highlighted in some papers² and the whole question is briefly reconsidered in *CM*, pp. 35-37.

The case of Best and Woudhuizen is even more indicative, because of the quite great diffusion (on reviews, proceedings, etc.) of these hypotheses: so we can find quoted (by non-specialists, though linguists) some absolutely groundless ideas in scientific books (an example in *CM*, p. 34).

Aartun and Best-Woudhuizen, particularly, extended their interest up to the Phaistos Disk and (the latter ones) to the Cretan Hieroglyphic. If anyone had the patience to read and examine the old Hrozný's 'dechipherments' of Linear B and Protoindic script, he would find a lot of methodological similarities with the proposals of the above-mentioned authors on Phaistos disk and Cretan Hieroglyphic. None of them seems to have ever read such an important book as *Entzifferung verschollener Schriften und Sprachen* (by Johannes Friedrich, Berlin 1966) and, especially, the simple but fundamental notions contained in the third chapter.

The consequences of all that on the 'interpretations' of the disk have been summarily pointed up by me in *CM*, pp. 173-175.³ Something similar may be said of the Cretan Hieroglyphic: another article of mine⁴ illustrates various

¹ That is subsequent to *GORILA 5* and, particularly, to the overcoming of the important question of the Linear A readability, correctly put forward by Godart and Olivier (see *CM*, pp. 37-44, 50-59), though many (less or more reliable) interpreters have simply ignored it.

² Substantially these authors found their assertions on mere formal coincidences (isolated words on cretulae, terms occurring in tablet headings not at all clarified by combinatory analysis, etc.). The case of Aartun is much more amazing. See below and Negri 1996, 2001.

³ The Appendix on the Phaistos disk, from *CM*, is also readable on line, at the address: www.scritturedimenticate.iulm.it

⁴ «Alcuni appunti sulla scrittura geroglifica cretese», forthcoming in *Do-so-mo*. This text can also be read on the website www.scritturedimenticate.iulm.it

aspects of application of the combinatory method on different levels of hermeneusis of texts written in unknown or scarcely known forms of writing. The so-called 'Cretan Hieroglyphic' is a clear case in which it is possible to show how the method operates on the levels of the form of writing, the signs decoding, the study of morphematic and lexical elements. The full and correct application of the method is preliminary to any scientific and reliable attempt of 'decipherment'. As to Phaistos disk and Cretan Hieroglyphic, these simple concepts are (nearly) completely forgotten by the above-mentioned 'interpreters', so their 'results' can be easily acknowledged as untenable.

Thus, even if very probably the truth is that, at the moment, a definition of the problem is impossible because of the scarcity of linguistic material, the *status quaestionis* of the identification of the Minoan language, can appear confused and uncertain, at least to non-specialists. Yet, at this point, if we ask how to distinguish reliable works (that is interesting, even if not definitive, suggestions), we may affirm that a main argument and *discrimen* is the consideration given to data preliminarily got from combinatory researches and analyses.

However some other hypotheses have been recently published, that seem to me worthy to be considered.

I refer to Monti 2002 and Witczak 2000 / Witczak-Zawiasa 2002-2003. On them I will give a commentary in the next two sections of this article.

At the end I will reserve a section to briefly comment also my personal suggestion of identification.

2. Hurrian-Urartean hypothesis

Monti 2002 (p. 120) affirms that some considerations would indicate «la possibilité que la langue du Linéaire A soit apparentée avec le hurro-urartéen».

Actually, this paper is (only) based on a group of morphematic elements of the Linear A (not every identification is sure). Unfortunately no precise detail on meaning and function of these elements can be deduced from a combinatory analysis of the contexts.

So, for instance, the comparison between Min. *i-* / *in(a)-* / *an(a)-* and some formally similar Hurrian-Urartean elements is weakest: indeed by the same kind of reasoning we should assume, and more strongly, that Min. *in(a)-* and *an(a)-* are 'clearly' identifiable as Indo-European prepositions (< **en(i)* 'in'; **an(a)* 'on'): in the case of Minoan alternation *ti-ti-ku* / *i-ti-ti-ku-ni* we could even recognize a term (a *n*-stem) regularly inflected in locative. Therefore, if Minoan were an Indo-European language with **e* > *i* and **o* > *u* (at least in certain contexts), we would have, approximately:

nominative	<i>ti-ti-ku</i> [titiku:(n)]
preposition + locative	<i>i-ti-ti-ku-ni</i> [in titiku(:)ni]

Let us compare Lat. *statiō* / in *statiōne*; Gr. *meizōn* / en *meizoni*. We should expect that the proclitic preposition would be correctly written (through the Cretan syllabary) together with the word which is referred to: *i-ti-ti-ku-ni* (*in titikun-i*).

Besides, the value of this supposed Min. / Indo-European *i(n)-* could be also applied to the case of *da-ma-te* / *i-da-ma-te*, words written as isolated on ritual artifacts (among which gold and silver axes). In analogy, e.g., with the *in* + accusative Latin construction, we could interpret them as:

dative	<i>da-ma-te</i> [damartei]
preposition + accusative	<i>i-da-ma-te</i> [in damartem]

or in similar ways, identifying **Damartis* (*vel similia*) as the Minoan name of the goddess re-interpreted by the Greeks (through a paronymasy) as *Dāmētēr*, i.e. Demeter.

We could go on with further examples, but the truth is that substantially for all the Minoan morphematic elements recalled by Monti (*a-*, *i-*, *a-n(a)*, *i-n(a)-*, *-a*, *-ja*, *-ne*, *-re*, *-na*, *-se*, *-ni-ta*, *-ni*, *-me*)⁵ it is positively impossible to outline the meaning or the function, neither *grosso modo*, combinatory data being absolutely insufficient. The only exception, that is the Minoan ‘suffix’ *-ja* (which «donne l’impression d’être un véritable suffixe d’appartenance»: Monti 2002, p. 119, yet this assumption is not well proved *in eodem loco*), could be compared with genitive or possession suffixes, not only in Hurrian-Urartean, but in other linguistic families too (e.g. Old Etruscan genitive *-ia*; Indo-European genitive *-ā*, etc.).

The problem with Monti’s paper is that the interpretation of the considered elements is not based on an analysis of combinatory data, that is the possibility to obtain, from the (con)texts of occurrence, some less or more precise details on the *significatum*: in fact Monti founds his assertions on mere formal coincidences. This method has not value on a scientific level, since, as we have just shown, other linguistic families (Indo-European is just an example) should be used for analyses even more convincing than that displayed by Monti.

3. *Indo-European hypothesis*

On the other hand, the attempts (of identification of the Minoan as an Indo-European language) developed in Witczak 2000 and Witczak-Zawiasa 2002-2003 are noticeable because are based on an analysis of the combinatory data: the study being above all focused on the so-called ‘libation formulae’.

Moreover the arguments are strengthened, on a cultural-linguistic level, by the search for parallels in Mycenaean texts with cultural and religious content or reference. Of particular interest is the suggestion of seeking Minoan formulae or phrases translated into the Mycenaean language and culture.

Even if, in several occasions, I have expressed some different hypotheses on the possible (combinatory) interpretation of the libation formulae⁶ (and on

⁵ Let us remember that these morphematic elements constitute the only data upon which Monti’s proposal is built.

⁶ See *CM*, pp. 125-132, 137-139.

important terms, like *ja-sa-sa-ra-me*), the analysis of Witczak and Zawiasa seems to be considered with accuracy.

Nevertheless, before any discussion in details, we must clearly underline that the basis of all these ideas is built on crucially new (in comparison with *GORILA*) readings of some important texts.

In particular, the fundamental SY Za 2 should be now read as:

a-ta-i-jo-wa-ja , ja-su-ma-tu-re
u-na-ka-na-si OLEum
a-ja

instead of (according to *GORILA*):

a-ta-i-jo-wa-ja , ja-su-ma-tu OLIVae
u-na-ka-na-si OLEum
a-ja

But, even if it is sure that the logogram for OLIVae is used, in one case (KO Za 1), as a more refined *ductus* for the similarly shaped syllabogram *re*, everyone can see that the undoubted presence of the logogram for OLEum (also transcribed, even though unexplained, in Witczak-Zawiasa 2002-2003, p. 46; instead Witczak 2000, p. 44 read this sign as ‘89’) makes substantially sure the reading OLIVae in the same text, especially considering the comparison with all the remaining libation formulae.⁷ In any case, the epigraphic problems arising about such a crucial new reading, necessary to obtain *ja-su-ma-tu-re*, a main pillar of all this theory, are nearly ignored or, at least, inadequately considered and discussed.

Moreover, the idea (indeed borrowed from Owens 1996) that PK Za 11 shows a sequence *i-da* , *pi-te-ri*⁸ ‘to the Father (of) Ida’ is obviously valueless, the correct reading being *a-di-ki-te-te-[-.]/[-.]da* , *pi-te-ri* (cfr. also PK Za 15), where the doubtful *-da* could even be read as *-re* and the preceding grapheme [-.], for the visible curved stroke, is either hardly or in no way readable as *-i-* (cf. the *-i-* of *a-ta-i-*, at the beginning of the same text).

Another clearly wrong reading is *i-da-ma-ta-ra*⁹ in SY Za 1: everyone can see, from the picture of *GORILA* 5, that, without any doubt, the correct transliteration is *i-da-mi* , *ja-*[-] as simply noted by Godart and Olivier.

These preliminary mistakes harm the whole hypothesis. However some other cogent objections could be drawn against the method used by Witczak and Zawiasa in order to identify place-names in the libation formulae, in comparison with the scheme of all the remaining texts, but I think that, at the moment, this commentary of the matter can be sufficient.

⁷ See *CM*, pp. 125-132, 137-139.

⁸ Witczak 2000, pp. 44, 47; Witczak-Zawiasa 2002-2003, pp. 46, 50.

⁹ Witczak 2000, pp. 44; Witczak-Zawiasa 2002-2003, pp. 46, 50.

4. *Etruscan hypothesis*

This hypothesis has been proposed by me in Facchetti 2001 and in the Appendix of *Appunti*. I consider it as a simple suggestion, which, even if based upon interesting elements, cannot be, at the moment, adequately verified or refused.

Here I just quote some sentences of mine to remind what is the exact meaning of this proposal.

«Quello che distingue il mio studio da altri tentativi di identificare le possibili parentele della lingua minoica è il fatto di fondarsi strettamente (con l'esclusione, come si è precisato, di buona parte del § 3.3) su dati ricavabili da un'analisi combinatoria dei documenti, senza contare che i vecchi tentativi si basano spesso su letture imprecise o sbagliate. Ogni indagine deve sempre partire dal contesto documentario, dalla sua struttura, e tener conto di tutte le ricorrenze della parola o dell'espressione che si intende analizzare. Però fino a che punto queste corrispondenze-coincidenze sono sufficienti e che cos'altro servirebbe per rendere plausibile l'ipotesi di una tale parentela? Io credo che i dati esposti in questo studio non valgano da soli come prove atte a dimostrare scientificamente una connessione genetica etrusco-minoica, ma, allo stesso tempo, ritengo che essi rappresentino un nucleo di forti indizi e principi di prova, che inducono a sospettare una simile affinità». ¹⁰

«Il problema principale, lo ribadisco, sta nella penosa carenza di materiale documentario minoico di natura non onomastica. Non è escluso, tuttavia, che futuri ritrovamenti (minoici, ma anche etruschi) possano apportare elementi nuovi e particolarmente significativi per avvalorare o respingere la mia proposta». ¹¹

«Per quanto concerne, poi, il possibile legame etrusco-minoico (...), va chiarito e sottolineato il carattere di 'proposta' e non di 'pretesa' identificazione. Del resto, come rimarca lo stesso autore, nemmeno lui riesce a recuperare certezze definitive dai dati che ha raccolto e ordinato. Il fulcro centrale dei 'sospetti' si appoggia sul fatto che la 'formula di libagione' minoica (per cui v. più diffusamente *infra*), alla luce dell'iscrizione SY Za 2, potrebbe presentare due donatari presumibilmente marcati con *-ja* e *-si*, che potrebbero richiamare fasi preistoriche delle due forme del genitivo etrusco, e che detti donatari sarebbero *a-ta-i-jo-wa-* e *u-na-(ru)-ka-na-*, avvicinati a parole etr. come *atiwu* '(dea) madre' e *Welχana-* < **Wenal(u)χana-*, in cui si prova ad identificare *Welkhanos*, il nome cretese di Zeus; il resto è costituito da particolari di contorno». ¹²

¹⁰ Facchetti 2001, pp. 30-31 = *Appunti*, p. 128.

¹¹ Facchetti 2001, p. 31.

¹¹² *CM*, p. 35.

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