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**The Interpretation of Etruscan Texts  
and its Limits**

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The problems of interpreting Etruscan texts rest on two issues: how to determine the "best current knowledge" and how to value the various different interpretations offered by different authors. Much of Etruscan grammar is debated (what I call *controversum*) or unknown (what I call *ignotum*), but not to the same extent in all areas. For instance we have wide information about the flexion of nouns and demonstrative pronouns. As to vocabulary, despite the sparse remains, it has been possible to reconstruct a more or less precise meaning for a respectable number of terms: but much remains unknown, or unconfirmed. This, mostly, is what makes short Etruscan texts translatable, while longer ones can remain quite obscure. Etruscan hermeneutics can be divided into three phases: a first phase of (pre)scientific approaches, a second phase of "pruning" and a third phase of refinement. A lack of etruscologists with the linguistic skills to contribute to such work, and even more the logistical difficulties of making new ideas widely known, the third phase has only been gradually noticed. Finally I examine some contributions to Etruscan by enthusiasts who speculate on the matter without the slightest knowledge of the basic concepts of linguistics. In such way I have given examples of how the circulation both inside the field and outside can fail and suffer in various unexpected ways.

**1. The problems faced**

A published linguist of Etruscan, in talking to colleagues, or other scholars, or laymen interested in studies of the Etruscans, will have come upon the problem of how far Etruscan texts can be reliably interpreted according to the best current knowledge. This crucial problem rests on two issues:

1. What is meant by the "best current knowledge"?
2. How valid are the various different interpretations of these texts offered by different authors?

These two questions come together to define the characteristics required for the study of the Etruscan language.

This field of study is often treated with suspicion or lack of interest by outsiders because it is seen as having too little, and too fragmented, material to be worth approaching. It is thought to be a fruitless waste of effort, because it lacks strong reference points. This impression sorely needs to be corrected.

So what are the essential characteristics required of a specialist in the Etruscan language?

Firstly and most importantly, he must be familiar with the methods required to study what is an archaic lingual code of which the only evidence left are fragmented writings, virtually all of religious, dedicatory, funereal or juridical nature, but in quite respectable quantity (this is exacerbated by the near-total lack of glossaries and bilingual writings of any length, and by the status of the language itself, genealogically isolated and without surviving descendents.) Such a specialist needs scientific linguistic skills, specifically of historical linguistics, of studying isolated and scarcely attested languages, and of the relationship between graphemes (or groups of graphemes) and phonemes.

The attention given to phonology (as it is reflected in written Etruscan, and can be elaborated by rigorous study) is one way to tell the sheep from the goats among those who work on Etruscan.

This is treated at length in Rix (1984, §§ 7-25), Agostiniani (1992, § 3), and these remain fundamental starting points: the disputed parts (such as the doctrine of Rix (1984) on the correlation of palatalization on which the system of Etruscan stops is supposed to have been built) are essential elements of the debate (for the specific criticisms see Agostiniani 1986, 36 s., and 1992, 49 s., and Boisson 1991).

For example, in Agostiniani (1983) the relations between aspirated and unaspirated stops in Etruscan are studied systematically, reviewing all the available material with detailed statistical methods. Among the more significant results, it has confirmed that the scribe of the *Liber Linteus* was unfamiliar with the script (so the long text is not fully reliable for these studies); it has shown Etruscan phonology, or at least important parts, can be reconstructed, not only on the phonematic level, but also in its effective pronunciation; it has also refuted the old theory (on which spirantisation was based) of "gain of aspiration" in Etruscan stops, and instead has shown the stops lost aspiration near to a sibilant (which I have

elsewhere called "regola Agostiniani della deaspirazione delle occlusive aspirate": cf. e.g., *Appunti*, 73). This ample study rests on statistical analysis and makes a role model for further research, showing how patience and special skills can reconstruct a system that was until then seen as casual and unexplained variations in orthography. We cannot consider any more that, for instance, <t, p, k> are interchangeable with <θ, φ/f, χ> in any position (*pulumχva* and *fulumχva*, for instance, are different words).

The specialist on Etruscan will also distinguish himself by the use of fundamental discoveries in morphology, particularly with flexion (of nouns or verbs), but also derivation. By "fundamental discoveries" is meant not only knowledge of those morphological features we are now certain of, but also familiarity with the main elements still the subject of debate and discussion (i.e., the *controversum*) as the lack of clear data or adequate analysis makes them obscure.

Clearly much of Etruscan morphology is debated (what I call *controversum*) or unknown (what I call *ignotum*), but not to the same extent in all areas. The type of texts we have has allowed us to reconstruct very accurately the flexion of nouns and demonstrative pronouns; on the other hand our knowledge of the morphology of verbs and personal pronouns is much more fragmentary, mostly due to their absence from the writings.

So, for instance, all specialists agree on the assertion that Late Etruscan has two endings in the genitive, *-s* and *-(ia)l*; as with *-i* in the locative (in cases becoming a monophthong with the final vowel: *-a-i>-e*). This is based on definite data (see e.g., Rix 1984, 224 s.).

Other discoveries have been added over time to the nominal morphology, such as the precise recognition of the ablative morphs (mostly from work by Rix and De Simone); for some time disputed, it is now confirmed, partly by the new document of the Tavola di Cortona, as well as the definition of the "pertinentive" case as being (diachronically) bimorphemic, as demonstrated in Rix (1984) and Agostiniani (1992) (in the latter case showing important proofs of morphophonology: in particular how a sibilant does not palatalize in juncture of morpheme, p. 56); also the crucial identification of the two morphemes for plurals for persons and objects, brilliantly done in Agostiniani (1993).

In my comprehensive re-examination of the Cippo di Perugia (in *Frammenti*), more than in earlier research (I refer to Pfiffig and Manthe), I have been able to base myself on better knowledge of morphology such as clear recognition of the ablative and pertinentive and Agostiniani's theory of plurals.

Research has now allowed the debate to move on from "whittling down" to highly technical levels (e.g. see Agostiniani 1993).

From morphology we move to vocabulary, where we find the largest obstacles to interpreting documents.

Little remains of Etruscan vocabulary, only a few hundred words. But linking words to concepts has been possible for a respectable number of terms, always using hermeneutic strategies of analysis. But much remains unknown, or unconfirmed. This, mostly, is what makes short Etruscan texts translatable, while longer ones remain obscure.

One can leave aside such opaque parts, or hypothesize about a word's meaning, or at least its semantic area. Such theories, clearly, are open to dispute, and should be made taking account of every occurrence of the term and its context, including the grammar data, other nearby terms, and such extralinguistic clues as the archaeological context, etc. The support of other concomitant hermeneutic strategies adds weight to the proposal.

A hypothetical meaning suggested for a word (or a root) has to be studied and verified not only in one specific text, but in all its recurrences. Then this accurate combinatory analysis (when possible: i.e., when we have a dossier of several recurrences of the same term, which is not a *hapax legomenon*) can be strengthened by means of bilingual comparisons, that is to say by comparing Etruscan texts with "parallel texts" (i.e., texts more or less hypothetically related, in content and typology, to the Etruscan one) of other ancient and proximate cultures, as Latin or Greek. A clear example of this method (with conclusive results) is the Adiego's analysis of Etr. *šparza* 'tablet' (see Facchetti 2005).

Another great support to specialists' work can be provided by the so-called "figurative texts", that is captions written beside all sorts of images; however, their analysis must be carried out with necessary caution. From the group of the "figurative texts", the inscriptions on mirrors stand out in

importance and length of text: these epigraphs, even if with a different degree of interpretability, give us some other "firm points" on Etruscan vocabulary (and grammar) as well as the possibility to discuss some particular features of Etruscan culture (see, e.g., the analysis of Etr. *ar-* 'to move' contained in *Frammenti*: p. 14, n. 37).

Unlike phonology and morphology, fields which require good linguistic skills, studies of vocabulary and the analysis of texts (taking account of all the grammatical data) can receive important contributions from scholars of other disciplines, in particular archaeologists, and from the use of other skills (such as those employed in the study of ritual or juridical texts), to try to identify the meaning. The precise "form of the content" of the sign (the effective semantic limits to each element) will remain, for Etruscan, not entirely known, given the fragmentary material; but this need not mean that for the vocabulary we will ever have but possible or probable theories: we are sure, with some approximation, that *apa* and *ati* mean "father" and "mother", that *zilaθ* indicates the supreme magistracy, or that *tur(u)-* and *mul(u)vani-* express "giving" / "donating" / "dedicating".

The same goes for morphology: there are areas where we have reached certainties. It is quite wrong to say that in the morphology "all is hypothetical". This is indeed true for some, large, sections (including nearly all the verbal morphology), and in this sense what was stated in Wylín (2000, 29) is valid.

Debate is constantly renewed over the meaning of words, with hermeneutic skills from different fields coming together.

Consider, for instance, the base *χi-*, which is taken to mean 'all', 'every' in *Frammenti*, p. 22, n. 83, resurrecting an old idea of Torp and giving new supporting arguments; very recently Colonna (REE 2002, n. 91, p. 399) thinks that finding *χi* isolated on vases suggests instead a meaning of 'just (fair)' or similar: anyone can give a value to such a new clue (assuming that isolated *χi* is not an abbreviation), and apply it to all occurrences of *χi-* (with results that are scarcely convincing).

Another telling example is the complex, well structured address given by Ignasi Adiego at the *II Convegno Internazionale sugli Antichi Umbri* (Gubbio, 25-27 September 2003: proceedings in press), titled *Etrusco marunuxva cepen*. The Spanish scholar has examined every occurrence of *cepen*, and

basing himself in particular on its distribution in the funereal *cursus honorum*, has sought to put in doubt the value it is usually given ("priest"), highlighting that it could conceivably not even be among the nominals, and theorizes that it could be a quantifier (meaning 'all'). Rigorous method is used throughout, and the suggestions for *marun-* are indeed plausible (and it allows us, for instance, to read in a new light *marunuc-i* 'in the *maronatus*'). Yet, as I said at the event, the suggestions for *cepen* are less plausible, since (starting with the aforesaid *χi-*, which is not taken into consideration) some of the main premises are not complete: whilst it is true, as Adiego said, that the meaning of 'priest' was initially based also on the word *cupencus* of the same value, according to Servius (who quotes it as a Sabine term), this has not been the only reason for which such a «significado, se haya mantenido como una evidencia durante tantos años». Very importantly, *cepen* (*cipen* in Capua), outside the *cursus honorum*, occurs frequently, but only in texts of a religious nature that concern sacred rituals (*Liber Linteus*, Piombo di Magliano, Tegola di Capua): Adiego notes this at the end of § 4, but ignores it. Why would a word like 'all' not appear in other texts?

So the hypothesis seems thin. The presumed difficulty of translating *cepen* as 'priest' in ritual texts comes from refuting translations, Wylin's in particular, that are not always satisfactory. But comparing the same passages as they are interpreted in *Appunti*, it can be seen that such difficulties can be overcome. I give two examples, the second one particularly complex (cf. *Appunti*, 70):<sup>1</sup>

*cepar* appears at the end of the second example, which seems to be an irregular human plural of *cepen* (instead of an expected *\*cepenVr*). It was probably formed from the original base *cepa-* (*cepen* would be an alternative form expanded in *-en*), of which a trace remains in the *cepta* (very probably from *\*cepta-ta*: 'the *cep(a)-*', with *-(i)ta*, well-known Etr. enclitic article) which appears in the *cursus honorum* of the epitaph AT 1.61 (from Tuscania) exactly where *cepen* would be expected, and

<sup>1</sup>The abbreviations are: abs(olutive), acc(usative), adj(ective), adv(erb), con(junction), con(junctive), dem(onstrative), enc(litic), gen(itive), hu(man), imp(erative), inj(unctive), loc(ative), nom(inative), non-hu(man), n(oun), nu(meral), pl(ural), postp(osition), pret(erite), pr(onoun), rel(ative), s(in)g(ular), subs(tantivized), v(erb).

LL VII.15-16<sup>2</sup>

<i>vacl</i>	<i>cepen</i>	<i>ϕaurϕ</i>	<i>cer-en-</i> e	<i>acil</i>	<i>etnam</i>	<i>ic</i>	<i>cleava-na</i>
N	N	Adj	V	N	Adv	Con	Subs Adj
Abs (Acc)	Abs (Nom)	Abs (Nom)	Inj	Abs (Nom)			
Sg Non-hu	Sg Hu			(it is of) need	equally	like	the <i>cleavana</i>
the praise'	the priest	funereal	do				

"the funereal priest must carry out the praise' the same way as the *cleavana*"

<sup>2</sup>In quoting Etruscan texts we will indicate the references in Rix 1991.

## LL VII.17-19

<i>vacl</i>	<i>ar-a</i>	<i>θun-i</i>	<i>sacni-clē-ri</i>	<i>cibbi</i>
N	V	Nu	Subs Adj + Encl Pr	N
Abs (Acc)	Conj	Loc	Loc + Postp	Gen
Sg Non-hu		(in one)		Sg Non-hu
the praise <sup>1</sup>	may (they) carry out	together	for the sacred (area)	of the temple <sup>2</sup>
<i>cepen</i>	<i>cibθva</i>	<i>cepen</i>	<i>cn-i-cn-θ</i>	<i>In</i>
N	N	N	Dem Pr	Rel Pr
Abs (Nom)	Abs (Acc)	Abs (Nom)	Acc + Postp	Acc
Sg Hu	Pl Non-hu	Sg Hu	(doubling)	Non-hu
a priest	(towards) temples <sup>3</sup>	a priest	towards the same	the same (thing)
<i>cer-en</i>	<i>cepa-r</i>	<i>nac</i>	<i>am-ce</i>	
V	N	Con	V	
Imp	Abs (Nom)		Pret	
(they must) do	Pl Hu			
	the priests	afterwards	have been	

"may (they) carry out together the praise<sup>1</sup> in favor of the sacred area of the temple: a priest must do that thing (towards) the temples<sup>2</sup> (and) a priest, towards the same (i.e., towards the other priest<sup>2</sup>); after the priests have been there etc."

which escaped the attention of Adiego. Obviously this human plural in *-r* (preceded by two occurrences of *cepen*) gives still more support to the value of 'priest' for *cepen*.

These are two examples of disputed cases, though I believe that the weight of evidence is on one side.

Another example of *controversum* would be the case of *mas(a)n*, for which I suggested a meaning of 'repetition' or similar in *Appunti*. Wylin, when we were preparing the joint article *Note preliminari sull'aequipondium di Cere* (which appears in *AIΩN*), noted that such a term, if it does not mean a ceremony, could have *masnur* (LL X.12) as a form of human plural and so mean a type of official or priest, a meaning which fits in with every known context (e.g. in LL XII.10: *vactnam θunem cialχus masn unialti ursmnal aθre acil* "equally the *masan* in the temple of Uni Urmna must do *aθr-* with regards to the praise<sup>2</sup> of the (day) twenty-nine"), and so would supersede my own hypothesis.

For a case of solved *controversum* (against a previous hypothesis of mine), cf. Facchetti (2005) on the meaning, which Adiego has finally clarified, of Etr. *šparza* ("tablet"). Another such revision would be the interpretation of the sentence at the start of the epitaph of Larthi Cilnei. In *Appunti* (p. 39), following but also revising Steinbauer (1998), I suggested as a translation: "who, as *Arretina*, with good fame<sup>2</sup> spent (her life)<sup>2</sup> and in this/these (condition/s) (or "and in this [place] here") died"; but after the intuition of Agostiniani-Giannecchini (2002) (which clearly re-established that the relative proposition is attributed to Luvchumes Cilnies and not to the epitaph's titular) it will have to be changed to: "who led the *Arretini* with glory (or "in victory") and in this/these (event/s) died" (all this is detailed in Facchetti 2003, § 7).<sup>3</sup>

Now we return to the two questions asked at the start. On the first, it should be clear that we cannot remain anchored to the grammatical sketches of Pallottino (1978 and 1992) or

<sup>3</sup>With respect to Facchetti (2003), I would like to note two errors: the last line of p. 210 reads "posposizione *-θi* (II)" instead of "posposizione *-θi* (I)"; the third line of p. 211 reads "citata posposizione (II)" instead of "citata posposizione (I)". Besides, on pages 218-219, the word *zusa* (Ve X.1) is simply analyzed as a nominal form, but, all things considered, it could be, even better, a verb (a conjunctive: hence a possible translation: *zusaua zusa* "i profumi<sup>2</sup> profumino<sup>2</sup>").

Cristofani (1991): they put together very well the work done until then, but are now well out of date; Rix (1984) and Agostiniani (1992) are now the essential reference works.

Two difficulties have to be noted at this point. Firstly it can be asked how a scholar with no preparations in linguistics can judge on the debate over grammar (the *controversum*), even in simply evaluating different opinions. The second is material: new thoughts and findings must be regularly gathered and brought up to date, but this is made difficult by how widespread the information is between various specialist publications and the effect is to slow down the thinking on new ideas. Particularly in Etruscan, there is plenty of unsound or outdated material that makes its way in and is difficult for non-specialists to distinguish from more valid work, even if they are competent linguists.

For instance, when Luciano Agostiniani showed me his article on plurals (Agostiniani 1993, which I have already mentioned) in August 1999 I was struck, and not only by the intelligence of the arguments: I had not found trace of such an important work on the morphology in other writings. It was not a case of me lacking information, since he told me he expected, sooner or later, that someone would take a position on the subject. I must say that afterwards, especially in the wake of the publication of the Tavola di Cortona (Agostiniani-Nicosia 2000) I noticed a remarkable convergence on the subject: at the Incontro on the Tavola in question (Roma, 22 June 2001: *Atti Tabula* 2002), the aforesaid theory on plurals was explicitly assumed to be known by the main speakers on the lingual aspect (in merit, I was able to read also the excellent Adiego 2001). Certainly in a study such as *Frammenti*, published in 2000, I made ample use of such findings.

Such an example shows the effects of circulating new discoveries, and the cost of not making them known.

In my review (Facchetti 2002, 589) of Koen Wylin's book on verbs (Wylin 2000) I underlined how some of his analyses of texts suffer from his failure to take account of Agostiniani's theory of plurals, by now an integral part of the study of Etruscan. The clear recognition of this point, recently made explicit in Wylin (2002, 102), is a good example of how the debate on Etruscan can proceed linearly, without aprioristic defence of earlier ideas. And in a brief correspondence (in September 2002) with Dieter Steinbauer, author of the *Neues*

*Handbuch des Etruskischen* (published in 1999) I became aware of how not all scholars of Etruscan know of the real importance of these recent findings. Separately Steinbauer, who seems not to accept the ideas of Agostiniani, objected that a suffix *-r* for human plurals was incongruous with the translation *naper* 'measures', suggested also in *Frammenti*. Actually one of the most important implications of the theory is that only humans, with the numerals, mark the plural, whilst non-humans are left in the zero declension (e.g., *ci avil* [not *\*ci avil-χva*] 'three years'; *ci clen-ar* 'three sons'). Looked at this way, *naper* is simply a non-human with a root in *-r*, like *caper*, a type of vase (of which the plural is directly attested: *caper-χva*).<sup>4</sup>

Brief texts are now uniformly translated by experts, except for some, usually marginal, details; among the longer texts that can be examined with current knowledge, differences reflect sides of the debate (or stages in development) in grammar and vocabulary.

But not all the production of these experts falls within the debate (i.e., the area of the *controversum*): there emerge those ideas of Steinbauer that do not take account of points now clarified, such as the issue of plurals; these are ideas with inaccurate or incomplete bases.

Something similar could be said of De Simone (2000), a hermeneutic work on the Tavola di Cortona written by one of the best known scholars of Etruscan.<sup>5</sup> His readings of the epigraphic writings are erratic, and so his analyses cannot be accepted or considered as part of the debate on the *Tabula*. A similar case nearly happened to me and Wylin while we were preparing our study of the *aequipondium* of Cere, mentioned above; after the new readings of this text given in Maggiani (2002, 167) (very different from what we had been working on until then) we had to revise the interpretation mechanism, (and corrected it just in time for publication).

I would like to underline the difference between works by competent authors excluded from the debate because they disagree with clarified points, and those that have to be

<sup>4</sup>The same large mistake has been made (though in relation to *tenthur*) by De Simone in his "review" of *Appunti* (see below, note 5).

<sup>5</sup>On the (un)reliability of the last Etruscological works of De Simone, see my article in *Ostraka* 13 (2004): 309-313.

excluded because the authors are utterly unreliable (I will go into this in depth in § 3).

## 2. Some proposals

Etruscan hermeneutics can be divided into three rough chronological phases.

Out of the depths of old efforts, based purely upon "etymology", the first phase started about the middle of the nineteenth century and extends at least until the Second World War. It saw the first (pre)scientific approaches and the elaboration of hermeneutic methods. The second was the phase of pruning the ideas through "classical" methods (etymological, combinatory, bilingualist: important hermeneutic instruments that have hardly been exhausted). Caffarelli (1975) tries to summarize these two phases; it is largely an uncritical (though arranged by argument) accumulation of different opinions, but most useful for bibliographical data; the results of the second phase are to be found in Pallottino (1978 and 1992) and Cristofani (1991).

The third phase (of refinement) started in the eighties; linguistic science has provided methods that have provided ever more significant points with good grounds, and offered more sophisticated approaches (take for instance the definition of the "typological method" given in Agostiniani 1993). Due to the lack of etruscologists with the linguistic skills to contribute to such work, and even more the logistical difficulties of making new ideas widely known, this phase has only been gradually noticed.

For the current knowledge of Etruscan and, generally, on the attempts –all fruitless– to compare it to other languages, I recommend the excellent Beekes (1993); I very much agree with him, except little details, like the citation of the obsolete "genitive of genitive" (p. 55), the analysis as "double case ending" of *-l-θi* (p. 57), in effect constructed from genitive+postposition, and the now old and wrong opinion that the link between Etruscan and Rhaetian is not yet clear (p. 59; see Rix 1997). The work makes clear the lack of validity of the many approaches tried over the years that used mainly "etymology" (to find links with other languages) by, for instance, Vladimir Georgiev or the archaeologist Alessandro Morandi (good on editing epigraphs but not very gifted on linguistic analyses). Another interesting case would be the

suggestions of the noted linguist Francisco Adrados, whose two articles (Adrados 1989 and 1994) tried to prove Etruscan to be of Anatolian origin, yet showing a considerable lack of knowledge on the acquisitions in the specific field.

Such approaches belong to the phases preceding true hermeneutics, yet still today research emerges that repeats these mistakes, and a striking example is the work of Fred Woudhuizen, whose main publication has received a much-deserved review by De Simone (1997), who concluded appropriately: «Man braucht keine schlaflosen Nächte zu verbringen, wenn man W.s Buch nicht gelesen hat»<sup>6</sup>. As it happens, the same Woudhuizen, in a review of Steinbauer's *Neues Handbuch* for *The Journal of Indo-European Studies*, repeatedly criticised it as giving too little credit to the Anatolian theory. Woudhuizen with Jan Best published other totally unreliable writings on Linear A and the Phaistos disc; worst of all is the pure fantasy to be found in serious texts, on which good linguists have worked who simply do not specialise in the matter, as I realized on reading Villar (1997, 87, 550). Villar talks of the ideas of Best and Woudhuizen as if they were remotely admissible: in their opinion the Phaistos disc and the Lemnos stele are in an Indo-European Anatolian language, the first evidence of «a Cretan branch of Luvian» and the latter of «a dialect related to Lycian and Lydian, which the author [Woudhuizen] is tempted to classify as Carian» (see CM, 34).

Still more recently Mario Alinei has published *Etrusco: una forma arcaica di ungherese* (Alinei 2003; see Gheno 2004), and all his skills as a linguist has not availed him from doing similar methodological blunders. I do not wish to go into details here; suffice it to say that Alinei clears away all the combinatory work done on Etruscan (for grammar especially) to try to make Uralic inflexions fit without ripping the seams. He completely ignores the aforesaid recent findings in phonology (and phoneme/grapheme relationships), returning to the obsolete but convenient theory that the handwriting changed and orthography was not consolidated (p. 263); this has been a catch-all approach for all who used etymology or groundless comparative studies. With a few prods in the right direction, comparing the five or six hundred Etruscan surviving lexemes

<sup>6</sup>I remain of this opinion even after De Simone's review of my *Appunti*, which ended in an anathema somewhat similar to the one above (see above, note 5).



with the tens of thousands offered by other languages (or more if you take whole families) can result in anything. Even on the vocabulary alone, Hungarian and Etruscan are irreconcilable: take Etr. *cel*, which from the Tavola di Cortona and other documents we are certain meant "earth" (but see p. 54); or the supposed dichotomy between the titles *zilaθ/camθi* (Alinei also equals them with *canθ*:- but see Torelli-Agostiniani 2001, 137), on which he constructs all his theory, but simply doesn't exist in Etruscan (cf., e.g., Maggiani 1998).

This is not to say that the search for languages related to Etruscan will always be inadmissible; rather it is to stress that such a theory would be well-founded only if it took account of all the results (if only to refute them) that have emerged from the debate (which is still developing and changing) of the "phase of refinement", that offers theoretical bases from the most advanced combinatory studies (*sensu lato*). By contrast, none of the etymological attempts have come near to working in this way (and from this comes their fundamental unreliability), being unable or unwilling to take account of the scientific data of the "phase of refinement".

This less-than-ideal situation makes it difficult to state what the consensus is on any issue among the specialists (between what is accepted, debated and unknown). Being able to do so would permit us to put all contributions quickly into perspective and offer the layman a reference point in the tortuous matter of Etruscan linguistics. This reference point would in turn kill off any further etymological research that starts off by ignoring the specialists' findings.

But currently such knowledge remains buried in various publications that the non-specialists will find obscure and confusing, and the debated remains indistinguishable from the unfounded.

For this the authority of a single author will evidently not be enough; what is needed is a specialised linguistic commission of international scope, supported by respected scientific institution, that would gather and publish any information that is debated or certain, reviewing it by objective criteria. Such a commission would have to edit a journal, in print and on the internet, dedicated to the Etruscan language. This would separate specialised scholars from enthusiasts who decide to have a shot at solving the "mystery" of the Etruscan language (and these wild ideas, as

will be seen in the next paragraph, are not always as harmless as the specialists assume).

I know many of the specialists would think this proposal impractical; but I must make clear that it would not mean forcing agreement on all points; it would simply be to establish what is accepted, and what is debated.

Another issue is what resources (of time and means) could be granted to such a project; this would depend on the enthusiasm of potential supporters and founders, but then, *ad impossibilia nemo tenetur*.

### 3. Etruscan guessology

I will now move from glottology to guessology: contributions to Etruscan by enthusiasts who speculate on the matter without the slightest knowledge of such basic concepts as double articulation, phoneme, language change, upon which glottology is built. It is like writing on astrophysics without being able to define a parsec, a red giant, the force of gravity, Maxwell's equations, etc.

With Etruscan, such works are ten a cent. I have read that during the twentieth century Etruscan "decipherments" appeared at an average of seven every four years. For just one interesting example, take Nino Boriosi, who was a journalist and wrote poetry, and has a website dedicated to his works. His cv presents him as "philologist and glottologist", who won various awards and in 1995 was named Commendatore al Merito della Repubblica Italiana for his "wonderful" work *Grammatica Etrusca*. To sample his method, take the translation of the basement of Corciano Pe 3.2: *peθns . calu/snal . aule . cu/rane . aules . ile/naxeis . tenixunce*: "sculpted to commemorate the building of the room (Municipal Palace?) in the year 105 (B.C.) and placed here on the day fallen from the Curcian holiday"<sup>7</sup> (it is really "to the otherworldly *Peθn*- [or "to the otherworldly *peθn*": cf. Vs 4.4]; Aule Curane [son] of Aule by his vote<sup>7</sup> brought to completion"). What is really striking here is not the translation of proper names as ordinary words, or the arbitrary segmenting and grammar (these are common to all such enthusiasts), but that *aule . cu* is given as meaning "in the year 105 (B.C.)": it would seem the Etruscans of Perugia used

<sup>7</sup> "Scolpito a ricordo dell'edificazione dell'aula (Palazzo Municipale?) nell'anno 105 (av. Cr.) e qui collocato nel giorno caduto delle festività curciane".

the Christian calendar a century ahead of its starting point. Before his death in 1999, Boriosi also wrote a book dedicated to "understanding" the *rongorongong* of Easter Island.

Such publications (often paid by the authors), as a rule, do not contribute to the scientific debate on Etruscan; so the specialists ignore them, as harmless pranks. Yet a few of these have not been as innocuous as might be thought.

First there is the tragic, well-known disappearance of Wilhelm Corssen in 1875, after the harsh criticisms of Wilhelm Deecke (cf. Beekes 1993, 52). An analogous case took place in 1934, when Alberto Pironti, who taught Latin and Greek at a school in Spoleto, being interested in Etruscan, published a book in which he used the familiar method of simple assonance and translated Etruscan as some Greek dialect. In 1935 a full-page article in the *Osservatore Romano* announced that Pironti had finally discovered the "key" to Etruscan. The international (non-specialist) press took up the news and Pironti was congratulated even by noted Etruscan archaeologists, among them Bartolomeo Nogara (curator of the Etruscan antiquities in the Vatican Museum). But soon Carlo Battisti and Massimo Pallottino showed the hopelessness of this translation, and the Ministry of Education felt obliged to form a commission of linguists who disapproved entirely of Pironti's ideas. Soon afterwards the man died at the age of 44, leaving a wife and six children. Larissa Bonfante interviewed the last of them, Franca Pironti Lally, and released an article on the matter in the third number of *Etruscan News* (p. 3).

It is easy to see that linguistic studies, *inter alia*, can be appropriated and deformed by political regimes for their own purposes (the concept of "Aryan race" was at first founded on linguistic arguments). Recently enough Etruscan was used to glorify the "Pelagic" origins of the Albanian people by the communist dictator Enver Hoxha; he impeded more sober research by the likes of Eqrem Cabej into early Albanian, while honouring a scholar who had proved Etruscan to be related to Albanian and Pelagic. A few sentences from an intelligent article<sup>8</sup> by Ardian Vehbiu will do at this point: «What did the regime do? Give scholarships and send students to distinguished universities of Western Europe, so that Proto-Albanian and Illyrian studies could continue? Create a special institute, or at least a department, for the study of Albanian

<sup>8</sup>Can be read at <http://members.aol.com/Plaku/origins.htm>

language proto-history, under the deserved leadership of Eqrem Cabej? Of course not. They invited to Albania the French scholar Zacharia Mayani, who had managed to explain the mysterious Etruscan inscriptions with the help of Albanian. Then the order was issued to publish, in Albania, the most recent of Mayani's books. And the leader Enver Hoxha started speaking about Pelasgians. Now, who has read Mayani's book, and has even the remotest idea about the method of historical linguistic research, will have immediately seen that what Mayani has ingeniously pursued can be anything but science. And who has spent some time with the problem of Balkan proto-history, knows that Pelasgians represent a mythic entity». Not long ago a French statistics scholar, having read one of my works on Etruscan, wrote to me asking why I did not mention the famous discovery of Mayani on the Etruscan-Albanian relationship.

These air-headed contributions can be damaging to scientific research and its divulging between fields of study, both by sidelining the work of genuine specialists, and because they can be picked up by scholars of other branches.

This happens when one of these authors manages, by dint of connections or any other reason, to gain prestige through the mass media or a well-known publisher, or to be endorsed by celebrities of the world of culture (who know nothing of glottology, that is the matter *de quo agitur*).

Not too long ago an archaeologist who taught Etruscology at a university and an ordinary professor of law asked me why the linguistic community was disdainful or indifferent towards a "scholar" of such "intelligence" as Giovanni Semerano, since various "authoritative" personalities had come to his defence. Many universities have bought his dictionaries and I have noticed that some students have been using his research to find the etymology of a certain word. Semerano, who worked as a bibliographical supervisor, now "a riposo", was awarded a gold medal for culture, a silver nameplate from the region of Tuscany and will probably be named Commendatore or at least Cavaliere. He has received overdone praise in *La Repubblica*, *L'espresso*, *Il Sole 24 Ore*, *Radio 3*, etc. His endorsements have come mostly from the philosopher Umberto Galimberti (who wrote «on the subject of the Etruscans (...) Pallottino said the writing was indecipherable»), and other famed philosophers such as Massimo Cacciari and Emanuele Severino (the latter

has reportedly called Semerano's books «a feast for the intelligence») to name only the most prominent. His books sell like hotcakes. All this can impress the non-specialist, but scratch the surface and you will break through it. The endorsements come from persons who may be geniuses in their own fields but they know nothing of glottology (Galimberti cannot distinguish between the two concepts of writing and language; again, like teaching astrophysics without knowing what a parsec is). To see what their endorsement is worth, take a few sentences of Galimberti's (in a presentation intitled *Le peripezie di un libro "scomodo"*): «In 1985, an international convention was held in Firenze during a celebration of the Etruscans, in which Giovanni Semerano was accorded three minutes, too short to demonstrate anything. Towards the end of the seventies Giovanni Spadolini, knowing Semerano, asked him to research the etymology of "Italy", which at the time was assumed to come from "land of bull-calves", from "vitulus". Semerano noted that the "i" of "vitulus" was short, whilst that in "Italia" was long, so it was more probably from the Akkadian "Atulu", meaning "land of the sunset", to which corresponded the Etruscan "Hinthial", that is "shadow". At that point the Italian correspondent for the *The Guardian* became interested, and through the offices of Neppi Modona, contacted him in Firenze and the interview got a full-page title *An Italian Professor Finds Akkadian Roots under the Linguistic Tree*.<sup>9</sup> This gives us an idea of how rigorous Semerano's reasoning is. A university student in glottology could tell the absurdity of this statement; but that is not for someone who, however learned, has not studied linguistics.

There is no point in discussing the value of Semerano's dictionaries here. His recent book on Etruscan contains the

<sup>9</sup>«Nel 1985, in occasione della celebrazione degli Etruschi si tenne un convegno internazionale a Firenze dove a Giovanni Semerano fu concesso un intervento di soli tre minuti dove non era possibile dimostrare nulla. Verso la fine degli anni Settanta Giovanni Spadolini, conosciuto Semerano, gli commissionò una ricerca sull'etimologia della parola "Italia" che allora veniva resa come "terra dei vitelli" da "vitulus" (vitello). Semerano segnalò che la "i" di "vitulus" era breve, mentre la "i" di "Italia" era lunga e perciò era presumibile che la parola venisse dall'accadico "Atalu" che significa "terra del tramonto", a cui corrispondeva la parola etrusca "Hinthial" che vuol dire "ombra". Fu allora che l'inviato in Italia del giornale inglese *The Guardian* si incuriosì del personaggio e, grazie ai buoni uffici di Neppi Modona, lo raggiunse a Firenze e lo intervistò uscendo poi con un titolo a tutta pagina *An Italian Professor Finds Accadian Roots under the Linguistic Tree*.

usual hashed use of assonance, built in his case on Sumerian and the Semitic languages taken as a unit (the Anatolian interpretations of Georgiev or Woudhuizen are preferable!). The context, as usual, is casually ignored in his analyses; an example (as good as any in the book): on the table of Cortona *pêtruis* is treated as no different from *pêtrus*, both being considered as genitive and translated as "of Petro". Yet the first is ablative, as is shown by the *šcêvês* (<*šcêva+is*), with which it is accorded, following it (besides whatever can be concluded from looking at every other occurrence of the morph). The small *-i-* of *pêtruis* can mark the distinction between science and extemporization. And yet beyond the image some of Semerano's supporters give of an "uncomfortable genius" silenced by the academic establishment, one gets the impression that his pitiful attempts to deconstruct the work of comparative studies with Indo-European (built upon the constant, now centenarian notion of systematic correspondence, going well beyond individual assonance in recognising how languages change) have an underlying political motive, to confront what is considered to be the origins of Aryan supremacist theories (Semerano's supporters are all of the same political leaning). This seems to be confirmed in certain sentences I read in a presentation of his entitled *La lingua originaria*. After a serving of his usual howlers (among them: «The same name Sargon is found in the Etruscan name *Tarcon*, meaning: high authority, just as we find the name of Caesar in Kaiser and Tsar. In the ancient Akkadian language Sargon becomes *Sharrukinu*, meaning: legitimate king, and that name we find in the Latin *Tarquinius*»<sup>10</sup>) he adds, in conclusion: «The demand among scientists for such new linguistics has been constantly expanding since, in a long article in the London paper *The Guardian* on the 15<sup>th</sup> December 1979, George Armstrong wrote (mentioning the testimony of another great scholar of antiquity, Aldo Neppi Modona) that our view of history was destined to go through a beneficial cultural revolution which, more than fifty years before, could have avoided a catastrophe

<sup>10</sup>«Quel nome Sargon lo ritroviamo nel nome etrusco *Tarcon*, Tarconte, col significato di: alta autorità, come ritroviamo il nome di Cesare in Kaiser e in Zar. Nella antica lingua accadica Sargon suona *Sharrukinu*, che significa: re legittimo, e quel nome lo ritroviamo nel latino *Tarquinius*».

born of ignorance, the racial laws».<sup>11</sup> The “new linguistics” he refers to is evidently Semerano’s science.

Neither mass media nor prestigious publishers nor famed but non-specialist scholars nor, especially, politicians can guarantee the scientific worthiness of authors on Etruscan, and the same goes for other matters of linguistics, peripheral or otherwise.

An example of how the mass media can promote absurdities, to give but one, provided by Mario Negri, who collects them: on 2<sup>nd</sup> August 1995 the *Corriere della Sera* and *La Stampa* (at least) published and praised the news, taken from the AdnKronos agency, that the Hellenist philologist Ferdinando Russo, of the University of Bari, had at last “deciphered” the Phaistos disc, discovering that it contained the hexameter, later used by Homer, and that the disk was made by African Pygmies, since the disk repeatedly shows what looks like a drawing of a fist; and the word “Pygmy” comes from the Greek *pygmé* “fist”. You have to read it to believe it.

As for the great publishers, Semerano, for instance, has had most of his ideas published by Leo S. Olschki, which deserves merit for specialist publications on human sciences, and specifically for studies on languages of Pre-Roman Italy (including Etruscan; *Etruscan Studies* was for long published by this firm). In the case of Semerano it simply followed the wrong advice, though one given by respected authorities (in other fields of study). Last year I was invited to a reunion of the Circolo Filologico Milanese (a renowned association, with broad funds and support), to witness the launch of a linguistic-toponymic book, published by Hoepli (the publisher of Pallottino’s *Etruscologia* and others), written by Claudio Beretta, an old professor who excelled in his field. There was the usual talk of an epoch-making discovery, and it was not the author celebrating himself; it was given by two female university lecturers of Pavia (one of genetics, the other of prehistoric studies). The sponsors complimented each other (with applause from the audience) for how the coherence of the

<sup>11</sup> «L’esigenza scientifica di tale nuova linguistica è andata sempre più dilatandosi da quando, in un lungo articolo sul londinese *The Guardian*, il 15 dicembre 1979, George Armstrong scrisse (riferendo la testimonianza di un grande studioso di antichità, Aldo Neppi Modona) che la nostra nuova visione storica era destinata a costituire una benefica rivoluzione culturale che, più di cinquant’anni prima, avrebbe potuto evitare il frutto di una fatale ignoranza, le leggi razziali».

genetic, archaeological and linguistic data (the latter “finally” reconstructed by Beretta), yet neither they nor the author had any linguistic skill evident (linguistics is indeed the main matter of this book), and the same conclusion is reached from a short read through the book in question.

These examples show how, especially with Etruscan studies, there is need for a credible reference point such as the international linguistic commission I suggested earlier. Because of the myth that it is difficult in Etruscology to find two opinions that agree on any point, the state of current knowledge is opaque to non-specialists, even linguistic scholars. This makes the field an easy target for any author who eschews combinatory analyses for assonances with which they can make any language fit in some way on to Etruscan.

This is ever more evident when you consider that such radically unreliable studies make their way not just into the wider press but even in scientific publications of linguistic studies.

With one glottological association (founded by one of the major scholars of Indo-European), I was saddened to see at several conferences, among professional linguists, speakers on Etruscan of an embarrassing incompetence saying incredible things and yet receiving applause.

Even *The Journal of Indo-European Studies* has lacked rigour towards Etruscan, hosting Woudhuizen as an expert in the field (I have already mentioned his review of the *Neues Handbuch* of Steinbauer). In *JIES* 25 (1997), 520 s., Adolfo Zavaroni’s book (just as lacking in method as those mentioned above) was given a brief and merciful review by Edgar Polomé; he was equally incompetent in the matter and yet gave a favourable review of the unacceptable work of Aartun (admittedly a linguist, a specialist of Semitic languages) to decipher Minoan (and the obligatory Phaistos disc) as a Semitic language (more serious and harsher criticisms of this work have appeared; e.g., Negri 1996). So Polomé, unwilling or unable to criticise when needed, has left on *The Journal of Indo-European Studies* neutral reviews of absurd books.

Zavaroni’s article, *Sulla presunta sibilante palatale in Etrusco*, in *Incontri Linguistici* 25 (2002), contains some strange ideas (such as liquidating *husiur* in n. 10 with an unjustified *ad hoc* morpheme *-i-* for the dual, mentioned in passing without a word for the theories of Rix, which explain well the puzzling

allomorphy of human plurals; or the emphasis given to the cases of *Hannossa* and *Pabassa*: Latin did not have /s/, so it would be normal for contrastive phonology to make it into -ss-; in some parts of Italy, by the influence of dialects, words like *fascista* are pronounced with -[ss]- instead of -[s]-. Just before concluding (p. 99 ss.), Zavaroni has to postulate, as pivotal to his reasoning, the existence of an Etruscan morpheme *\*\*alis* (which he finds reminiscent of the Latin adjectives ending in *-alis*). But it has never been found in absolute form (i.e., without other affixes or enclitics), because it simply does not exist (Latin *-alis* is made of two morphemes: *-ali+s* [relational adjective]+[male/female nominative singular]; this structure cannot have anything to do with Etruscan). Zavaroni replied to me that there are appellatives such as *truials* "Trojan", *papals* "grandson" or metronymics like *crucials* or *pacials* that he insists present the absolute (and syncopated) form of this suffix; yet as every specialist knows, in the metronymics *-(i)als* is the form taken by the ablative (this is well noted and defined), whilst in appellatives like *papals* (plural *papals-er*) the suffix *-is* is a loan from Italic forms, such as *-cs* in *papacs* "grandson" (< *\*papakos*) or *-ns* in *cleusins*- "*Chusinus*". Other articles of Zavaroni have been accepted for publication or already published by respected scientific journals, such as *Emerita* (edited by Adrados), *Indogermanische Forschungen*, *General Linguistics*, *Die Sprache*, *Revue Belge de Philologie et d'Histoire* etc. The absence of "referees" familiar with linguistic matters such as Etruscan is an unfortunate consequence of the dispersal of ideas and contributions from genuine experts, which I have repeatedly stressed (besides Etruscan, I could cite the incredible articles of Hans Glarner on the Linear A script, published in the latest editions of *Kadmos*).

Lastly I would like to look at Claudio De Palma's book, *Il Paese dei Tirreni: Serona toveronarom*, published in 2003 in the *Studi* series of the Accademia Toscana di Scienze e Lettere "La Colombaria".

A detailed analysis shows up all the main techniques used by authors like these: varying orthography, arbitrary segmentation of words, arbitrary morphology, and assonances (such luxuries as systematic correspondences, scientific accuracy on the reconstructed (morpho)phonology or taking into account diachronic mutations or influences from other languages being largely unknown).

The first chapter provides a resume of the Italian excavations at Lemnos, but starts, worryingly, with: «*Serona toveronarom*: "Country of Tyrrhenians", or "Kingdom of Tyrrhenians". This is the definition given by the stele of Kaminia to the island of Lemnos».

The second chapter is entirely about the text of the stele of Kaminia (or of Lemnos). The phrase recalled in the book's title (*\*serona toveronarom*) is actually reconstructed. On *serona*-De Palma adds nothing new. I suggested, as a theory among many, keeping to the interpretive model given in *Appunti* (p. 16 s.), that it might be a toponym. In the same work (p. 17, n. 30) I deemed interesting the possible connection with the Philistine *srn* "king" (suggested in Van Der Meer 1992); this would imply *serona-* (< *\*seron-na*) as "kingdom, reign" and translations along the lines of: *seronai morinai(a)l* "in the kingdom of Morina" (I suggest here, for the first time, the integration of *-i(a)l*, a regular genitive II, which would solve all the problems of this form: see *Appunti*, p. 16) and *holaiasi phokiasiale seronaiθ* "under the reign of Holoie Phokias". De Palma assumes the translation "kingdom, reign" or similar by comparison with *srn*, but without citing Van De Meer or others. His reading *\*toveronarom*, on the other hand, is obtained through incredible manipulation and pseudo-translating. The lateral text of the stele does indeed read *seronaiθ . evisθo . toverona[...] / [...]rom* ; leaving aside the morphology and syntax, which we will look at later, De Palma goes against strong archaeological-epigraphical evidence (and common or unanimous opinion) in taking *toverona* and *rom* as a single word. Such an important point is glossed over in three lines of apodictic statements on page 32, with no supporting arguments. If we now pass to morphology, De Palma (p. 51 s.) states that *toveronarom* «is the key word to the exegesis of our text ... The interpretation of *toveronarom* as "of the Tyrrhenians" is evidently of major historical importance ... The ending *-rom* can be divided into *r-o-m*. The *-r* indicates a plural, as in Etruscan *clenar*, the Lemnian *-o* (corresponding to the Etruscan *-u*) is probably part of the genitive plural desinence together with *-m*. So we have *toverona-r-om* meaning "of the Tyrrhenians" (genitive plural)».<sup>12</sup> In the subsequent note 88

<sup>12</sup> «è la parola chiave dell'esegesi del nostro testo ... L'interpretazione di *toveronarom* come "dei Tirreni" ha evidentemente una grande importanza sul piano storico ... La desinenza *-rom* va suddivisa in *r-o-m*. La *-r* è la desinenza del

he cannot resist recalling «Latin *-arum, -orum, -um*» and other terms of equally embarrassing absurdity. No-one has told De Palma that for decades it has been known that languages can be classified by their morphosyntactic features (there are chains of comparisons to prove it) and that Latin, for example, is among those mostly “fusional” (or “inflective”): their inflection morphemes are cumulative, expressing the values of various grammatical categories. Take for instance Latin *reg-is* “of (the/a) king” / *reg-es* “(the/a) kings” / *reg-um* “of (the/a) kings”; *-is* expresses genitive singular, *-es* expresses nominative plural, *-um* expresses genitive plural: there are no single forms that indicate genitive, singular or plural. Etruscan, on the other hand, is more an agglutinative language, and this happens: take *aiθ-l* “of the temple?”; *aiθ-cva* “the temples?”; *aiθ-cva-l* “of the temples?”; it can be seen that *-l* expresses genitive and *-cva* expresses plural (and  $\emptyset$  singular: *aiθ* is “the temple?”). This is part of the “linguistic typology”, a fundamental branch of the science De Palma would like to handle; it seems just as evident he does not know that the form of the Latin I declension *-arum* comes from an earlier *-asom* (this was the form at the time the stele of Lemnos was carved) and as for *-um* < *-om*, it results from an ancient blending of nominal and pronominal flections: but historical grammar, and more generally the distinction between diachronic and synchronic analyses (another pillar of linguistic study), are unknown to these improvised interpreters. It seems superfluous to add that despite the clumsy phonetic revisions he suggests, the base *toverona-* cannot be connected to *Τυρρηνοί*, which presumes *turs-* as a root (with a change *-rs-* > *-rr-*: *Τυρρηνοί* and *Τυρρανοί* are also to be found); Latin *Tusci* comes from this same root, with a different suffix (with *-rsc-* > *-sc-*: Umbrian has *turs-ko-*: on all this cf. De Palma's incredible assertions on page 102). To conclude: the genitive plural *-om* (or *-um*) in Lemnian (or in Etruscan) is an invention of De Palma: the respective, distinct forms for plural and genitive are actually well known to specialists, and are always agglutinated (since the language is agglutinative) when the two values are to be expressed together. This simple analysis shows De Palma's «key word of the exegesis» to be a mess.

plurale, come nell'etrusco *denar*, la *-o* lemnia (*-u* in etrusco) è probabilmente parte della desinenza del genitivo plurale insieme a *-m*. Abbiamo così *toverona-rom* col significato di “dei Tirreni” (genitivo plurale).

The remainder of the analysis of the Lemnos stele is totally unacceptable.

The form *avis* is the genitive of *avil* “year” with dissipated *-l-* in Lemnian (unlike in Etruscan: *avils*) due to the strong palatalization, at least finally, that has it absorbed by the preceding *-i-* [*awi*<sup>13</sup>s] (Agostiniani 1995, 36); on this point too, De Palma accumulates contradictions and absurdities: «*avis* corresponds to the Etruscan *avil* (nominative) or *avils* (genitive). The context, including that of the word's appearance on the stele, makes one think of a liquid root that in Lemnos evolved in a sigmatic theme that could not decline»<sup>13</sup> (p. 44). Another example of arbitrary morphology is *eviθo*, (and leaving aside the obsolete theory of equating it with the Lemnian place-name Hephaestia, as De Palma does without giving any phonological justification) which De Palma translates as a genitive, “of Hephaestia”, yet there is no affix to suggest such a function (but if the form is nominal, it would be in the zero case).

De Palma constructs *seronaiθ* in the lateral text to have a zero case *serona-* with the addition of the desinence *-iθ*, and adds that it is a variant of Etruscan *-θi* (p. 46). It is in fact a locative *serona-i* with the added postposition *-θ* (an apocopate variant of Etruscan *-θ(i)*: for the Etruscan postpositions see Rix 1984, 227 s.; *Appunti*, 75 ss.): *serona-i* appears without the postposition in the frontal text.

De Palma agrees with the most recent interpreters that *aker* is a personal name. He adds that this name could be connected to the Greek Achilles (which came into later Etruscan as a loanword). How to turn *-r-* into *-l-* is solved by this line of argument: «in one tablet of Linear B of Knossos we read *akireu* which corresponds to the Greek Ἀχιλλεύς, that is, Achilles. Besides Linear B, this lack of distinction between the liquids *l* and *r* can be seen comparing the Mycenaean *uraio* (*sic pro u-ra-jo*) and the Lemnian *holaie*, and in the passage from the Etruscan *velθune* (*sic pro veltune*) to the Latin *vertumnus*»<sup>14</sup>

<sup>13</sup> «*avis* corrisponde all'etrusco *avil* (nominativo) o *avils* (genitivo). Il contesto, anche nelle occorrenze di questo lemma sulla stele, fa pensare ad una radice originaria in liquida, evoluta a Lemno in un tema sigmatico indeclinabile».

<sup>14</sup> «In una tavoletta in lineare B di Knossos abbiamo infatti *akireu* che corrisponde appunto al greco Ἀχιλλεύς, e cioè Achille. La indistinzione delle liquide *l* ed *r*, oltre che nel lineare B, come abbiamo visto anche nel

(p. 48). If De Palma had any grasp of the orthography of the Linear B, he would know that the syllabograms the Mycenaeans used to write a liquid (/r/ or /l/) are by convention transcribed as *r*; so it is a matter of writing, not language, a casual decision to use *r* instead of *l* in our transliterations. The Mycenaeans read this syllabogram with /r/- or /l/-, according to the context; in this case, *a-ki-re-u* was read [ak'il(l)eus] (the final *-s* was usually added in pronunciation; the Linear B syllabary had signs only for open syllables, insufficient for writing Greek appropriately; cf. page 88: «*Phaistòs, pa-i-to* in Linear B, we do not know if this is due to the absence or simply the graphical omission of the final *s*»; De Palma certainly does not know!) The Lemnian alphabet has distinct signs for *l* and *r*. Once again De Palma is talking of matters without bothering to find out the facts about them. The passage of Etruscan *Veltune* to Latin *Vertumnus* has very little supporting evidence, and in any case would be due to phonological rules applying to loanwords (there is nothing to suggest that in Etruscan *l* and *r* were interchangeable).

On page 55 De Palma talks of an «articulated postposition *-tis*», and soon after: «The Lemnian *tis* must be postposition, due to its brevity and position in the text» (a footnote recalls an irrelevant passage from Pittau, who knew nothing of identifying postpositions in Etruscan); further down: «in this context we could read ... with *tis* a postposition, but this would be contrary to Etruscan usage». <sup>15</sup> He must be clumsily paraphrasing, without citing the source, passages on pages 82-84 of *Appunti*, where *tis* has been first analysed as an adpositional element (but note that De Palma's text is far more contorted than these quotations might suggest).

I will not go into much detail concerning other comparisons decided by arbitrary assonances (*arai* "earth", *tavarsio* "king", *haralio* "commander", *eptesio* "horseman") or incredible morphological analyses, such as *vanalasaial*, which he takes to be a "double genitive" (p. 49).

The third chapter is dedicated to minor inscriptions of Lemnos, and it spares us nothing: on page 66, *varthamesa* (yet

confronto tra il miceneo *uraio* (*sic*) e il lemnio *holaie*, si riscontra anche nel passaggio dall'etrusco *velthune* (*sic*) al latino *vertumnus*».

<sup>15</sup>«Il lemnia *tis* per la sua brevità e collocazione nel testo deve essere una posposizione» (...) «Nel nostro contesto potremmo leggere ... con *tis* posposizione, ma questo contrariamente all'uso etrusco»

it is more probably *zarthamesa*: cf. *Appunti*, 18, 55) is repeatedly compared to forms of the name Artemis (yet Mycenaean shows that it never had that initial *w*); other howlers can be found on page 67, with the comparison of *tniso* to Neo-Etruscan (!) *tn* "this" (accusative < (*e*)*tan* < arc. *itan(i)*) and on page 68, with *novaisna* translated as "belonging to the young god", thanks to an evident «incurSION (!) from Indo-European» that explains Lemnian *nov-* (and Etruscan *nuv-*) meaning "new, young".

I will leave aside all the other brilliant material and conclude with some notes on the sixth and last chapter (*Tirreni in Occidente*). Here the archaic Etruscan inscription on the jar of Barbarano (AT 0.1) is submitted to the same martyrdom of De Palma's hermeneutics (p. 112 ss.): besides the usual spurious assonances (such as linking *avaiθi* to the «Indo-European root *ap/av* "water"»), there is an array of samples of arbitrary phonetics and changing consonants based on imagined orthographical variations. Such as: «*atiuthnath* shows a *-nath* ending that could be a family name in *-na* and a genitive in *-th*, with the *-th/-s* variation common in Etruscan: *larth/lars*, *huth/husina*, etc. The family name *atiusnas...*» Making *θ* and *s* interchangeable (elsewhere expanded to *θ/s/z*), a method typical of these enthusiasts, allows him to turn *atiuθnaθ*, which most probably should be divided into *atiuθ naθ* (*atiuθ*, still with the *-θ*, appears also in Cr 0.4), into a family name \*\**atiusnas* in the genitive.

What I have said above is evident for any specialist in Etruscan hermeneutics, perhaps less so for non-specialist linguists, and certainly not for non-linguists. This is why the administrators of the academy "La Colombaria", well acquainted with other human sciences, agreed to publish De Palma's book among their *Studi*.

My intention in this article is not to attack neutrals, but to show, with solid examples, how the work of serious scholars can be confused with ideas of no scientific validity. The progress that has been made in fields of study like Etruscan has to be matched by its divulgation, to other linguists and to scholars in more distantly related fields.

I might be accused of lacking tact, but I do not wish to attack individuals, only single points, to show how easy it is for enthusiasts to think themselves able to improvise a translation, and, succeeding in publishing senseless works even in

authoritative contexts, find their way into a situation that appears confused to outsiders.

It has been often suggested to me to simply ignore such proposals, and this "cure of silence" has merits (and I do not intend to spend my academic career refuting hot air), yet I have given here examples (among many more) of how the circulation both inside the field and outside can fail and suffer in various unexpected ways.

The international linguistic commission I have suggested, or some other mechanism, might provide a buffer against such pseudoscience.

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## Book Reviews

## Archaeology

Mary Settegast, *When Zarathustra Spoke: The Reformation of Neolithic Culture and Religion*. Costa Mesa, CA.: Mazda Publishers, Inc., 2005. 161 pages, \$35 (cloth). ISBN: 1-56859-184-5.

This slender volume takes on a contentious and extremely complex subject the dating of Zarathustra. While there are differing opinions as to the precise dates, the general consensus is that the *Avesta* was not written down until the 4th to 6th centuries AD; but the composition of the *Avesta*, based on similarities with the *Rig Veda*, is thought to date to the 11th century BC. Because the *Gāthās* are the earliest part of the *Avesta* and attributed to Zarathustra himself, their dates, and by extension the date of Zarathustra, ranges from ca. 1100-600 BC (Mallory and Adams 1997:307). Also to be noted are the classical authors who dated Zarathustra at 6500-6200 BC, but no modern scholar takes heed of them. However, it is because Avestan scholars disagree about the time Zarathustra lived, Settegast has stepped into the debate and come down on the side of the classical authors. For evidence of this earlier date, she claims that the spread of farming was the vehicle for carrying out Zarathustra's declaration that farming is the preferred way of life.

The preface lays out her hypothesis: "the possibility that the massive spread of farming after 6500 BC, which reformed and secured the Neolithic Revolution from Iran to the Aegean, might have been inspired by the teachings of Zarathustra" (xi). She is particularly influenced by the work of Colin Renfrew and his belief that Indo-European languages were spread by the spread of agriculture and Jacques Cauvin's *The Birth of the Gods and the Origins of Agriculture*. Because archaeologists have not answered all the questions relating to the spread of agriculture, she takes the view that the *Gāthās* do. Nevertheless, she rightly points out that advances in archaeology show Neolithic society to be much more sophisticated than the previous view of "primitive villagers" (2). Because of this, "[t]he call for reassessment is joined by a growing number of prehistorians who feel that a true understanding of the Neolithic will be achieved only when its