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REVIEWING THE NEW LINEAR B TABLETS FROM THEBES

Interested scholars should read my two reviews¹ of the edition with commentary of the new Thebes tablets² carefully and decide for themselves whether the account provided in the preceding article³ by V. L. Aravantinos, L. Godart and A. Sacconi (hereafter AGS) is accurate. The whole first page of my review in *Minos* (p. 475) praises the scholarly team involved in excavating, preserving, joining, drawing and editing the new Thebes tablets.⁴ I call aspects of this work ‘fine’, ‘superb’ and ‘expert’. I praise its speed. In my *AJA* review (p. 115), I stress that “[t]he transcriptions, photographs, drawings, indices and palaeographical tables in this volume are good and useful”. I conclude that the edition of the texts, i.e., the drawings and transcriptions and related indices and tables, in *TOP* is “good work” and “reasonably well done”. Why? Because it is good work that is reasonably well done.

Are there shortcomings that keep the volume from being ‘very good’ or ‘excellent’? Yes, there are. The editors omit two entire palaeographical sign charts and part of another. They do not provide physical descriptions of individual tablets, detailed information about tablet find-spots, or charts of scribal hands. They misread signs, in some cases systematically, and misalign signs within transcriptions. Some of their tablet assignments to series are questionable. Their ‘glossary’ is not a true glossary, but requires tracking down discussions of lexical items in the commentary on individual texts. All of

¹ My reviews appeared in *Minos* N.S. 35–36, 2000–2001, pp. 475–486, and *AJA* 107, 2003, pp. 113–115.

² V. L. Aravantinos, L. Godart and A. Sacconi, *Thèbes. Fouilles de la Cadmée I. Les tablettes en linéaire B de la Odos Pelopidou*, Pisa–Rome 2001 (hereafter referred to as *TOP*).

³ V. L. Aravantinos, L. Godart and A. Sacconi, *En marge des nouvelles tablettes en linéaire B de Thèbes*, *Kadmos* 42, 2003, pp. 15–30.

⁴ Contrast the impression given by AGS, *Kadmos* 42, 2003, p. 16, n. 2.

these shortcomings can be overcome by Mycenological experts with diligent work.

However, the interpretation of these new tablets presented in the commentary of TOP and in preliminary articles published by the editors is of interest to specialists in other fields of the study of ancient cultures, texts and languages. Non-Mycenologists do not understand the methods used in Linear B textual interpretation. My reviews, therefore, concentrate on the interpretation of the texts put forward in the commentary of TOP. I do so from the perspective of someone who has studied and written extensively about Mycenaean epigraphy, palaeography, scribal and sealing systems, textual interpretation, society, economy, religion, dialect and language for over twenty-five years.⁵

The preliminary articles by AGS mentioned above presented selections or excerpts of pertinent inscriptions. This made it impossible, before the official publication of the texts, for other Mycenologists, even those few who may have had limited access to the tablets or their photographs, to study all the relevant primary data. They could not then offer any serious critique, pro or con, of the theories of AGS that the new Thebes tablets contained references to a heretofore unattested Mycenaean cult of Demeter and Kore (Persephone), to theriomorphic deities, to an idiosyncratic measurement ratio, to a new sign value, and so on.

The preliminary articles of AGS and their extensive commentary in TOP do not make clear to non-Mycenological specialists how speculative some of their theories and identifications are. This was the primary reason for my concern about bibliography in TOP.⁶ Full monographs, like TOP, and articles that interpret new inscriptions in new ways should meet higher standards of bibliographical thoroughness than reviews or notes, which are often constrained, as here, by page limits.

⁵ See most recently: *Archaeology and Text: Decipherment, Translation and Interpretation*, in J. K. Papadopoulos and R. M. Leventhal eds., *Theory and Practice in Mediterranean Archaeology: Old World and New World Perspectives*, Los Angeles 2003, pp. 45–73; and ‘Archives’ and ‘Scribes’ and Information Hierarchy in Mycenaean Greek Linear B Records, in Maria Brosius ed., *Ancient Archives and Archival Traditions*, Oxford 2003, pp. 153–194. For further discussion of particular textual readings and/or interpretations advanced in TOP, please contact me at: tpalaima@mail.utexas.edu.

⁶ TOP, pp. 163–165. Besides standard handbooks and dictionaries, 22 out of 29 citations refer to the editors’ own publications between 1995 and 2000.

My reviews concentrate mainly on the commentary in TOP. I point out where interpretations of single words or entire texts are difficult to reconcile with the full body of Linear B data, with established rules of historical linguistics, with evidence from the Greek historical period, or with the consensus reached by fifty years of Mycenaean work on ca. 5,000 tablets from now seven sites. I also point out how improbable some of the religious interpretations appear to be. When a word like *to-pa-po-ro-i* can be explained by attested Mycenaean forms as ‘basket-carriers’ and instead is interpreted as ‘bearers of lightning bolts’, which in turn is explained as a unique metaphor for ‘bearers of torches’ and is then linked to the historical Eleusinian mysteries (TOP, p. 172), it is safe to conclude that some of the editors’ ideas are speculative.

In my AJA review (p. 115), I list six full categories of data, including data from previously discovered Thebes tablets, that have been used to identify texts or isolated terms within texts as ‘religious’ in purpose or meaning. Such data are almost completely absent from the new Thebes tablets. This should make us a priori skeptical of attempts to assign religious significance to these texts. In the few cases where such data do occur in the new Thebes tablets, I think they have been improperly generalized and applied to other texts within and across series, even by scholars other than the editors of TOP.⁷

I do not have space to answer point by point all the criticisms of my reviews raised here in *Kadmos* by AGS. Readers are advised to check carefully for themselves how AGS represent the opinions of other scholars, including myself.

For example, AGS claim that I “reproached them with virulence for not having cited the posthumous article by John Chadwick”⁸ in *Minos* 31–32, 1996–97, pp. 293–301. My exact words are: “One conspicuous omission from the bibliography is the masterful posthumous article by the late John Chadwick ...”⁹

⁷ This is most conspicuous in discussions of the Fq series where the occurrence of three *o-te* clauses on what I estimate to be between 15 and 18 full texts has been used as defining the norm, rather than the exception, for these documents. The Fq tablets strike me as records of routine daily issues of grain for a half-month period to individuals and groups. On three of the days, special events are marked. One of these is an ‘assembling of all elders’ (J. Chadwick, *Minos* 31–32, 1996–97, pp. 296–301).

⁸ *Kadmos* 42, 2003, p. 16.

⁹ *Minos* N.S. 35–36, 2000–2001, p. 477.

AGS allege that it was impossible to cite Chadwick's article because TOP was already in proofs.¹⁰ But Chadwick's article appeared in September 1999, over two years ahead of the publication of TOP; and TOP, p. 365, uses and cites another article from the same number of *Minos*. Its bibliography (pp. 163–165) lists articles published in 2000.

Moving on to less trivial matters, AGS claim that John Chadwick did not declare himself on the interpretation of the term *ma-ka*,¹¹ which they interpret as $\text{M}\check{\alpha}\ \Gamma\check{\alpha}$ or “Mother Earth”, effectively Greek Demeter. However, in the sentence right after those that AGS elliptically quote, Chadwick (p. 293) explicitly rejects their hypothesis: “I make no secret of the fact that I find the interpretation as $\text{M}\check{\alpha}\ \Gamma\check{\alpha}$ unacceptable.”¹²

As my reviews make clear, Chadwick had many good reasons for his scholarly opinion. We have no other examples in ca. 5,000 Mycenaean texts of two monosyllabic words being combined into a single lexical unit. There is no independent corroborating contextual information in the Thebes Fq tablets for the religious interpretations the editors propose. By this I mean that among the 28 or so entries besides *ma-ka* on a typical full Fq tablet, none is a religious term (theonym, religious official, religious transactional term, month name, designation of offering or cult locale) identifiable on any of the many ‘religious’ tablets previously found at Thebes or at other sites. This is true for all of the more than 55 personal or occupational names listed on the 120 Fq tablets and fragments.

Contrary to AGS,¹³ the sole textual parallel for *ma-ka* in the Mycenaean corpus (KN F 51) does not correlate *ma-ka* with *di-we* (dat. Zeus), but correlates *ma-ka* with *po-ro-de-qo-no*. This is consistent with the parallelism of the related un-prefixed word *de-qo-no* and *ma-ka* on Thebes tablet Fq 254[+]255. AGS conclude¹⁴ that *de-qo-no* is not a theonym, although they interpret it improbably as an official title. Their own reasoning then should lead to the conclusion that *ma-ka* is not a theonym.

¹⁰ Kadmos 42, 2003, p. 16.

¹¹ Kadmos 42, 2003, p. 16–17. Oddly enough later on p. 19 of their article, AGS do admit that Chadwick rejects their interpretation.

¹² Chadwick concentrates in *Minos* on the *o-te* clauses because the full texts for evaluating *ma-ka* were unavailable to him.

¹³ Kadmos 42, 2003, p. 20.

¹⁴ TOP, pp. 225, 390.

Among other possibilities, I think the strongest argument can be made for interpreting *ma-ka* as *magā*, a nomen actionis of the kind frequently used in these accounting texts (cf. *o-pa*, *wo-ka*, *po-qa* etc.). It means ‘kneading’ (of barley cakes, I assume eventually for distribution). *ma-ka* then would be parallel in function, on TH Fq 254[+]255 and on KN F 51 respectively, to *de-qo-no* = *deipnon* ‘meal’ and *po-ro-de-qo-no* = *prodeipnon* ‘preliminary meal’ as a reference to the use of grain for collective processing and eventual consumption. This would explain why the allocations of ‘barley’ in the *de-qo-no* and *ma-ka* entries at the head of the Fq texts are much larger than those for the other twenty-eight or so recipients on the full tablets. These initial large allocations are for aggregate use as opposed to the much smaller allocations for individual use by the other recipients listed on the tablets. If better alternative interpretations can be found for these terms, consistent with Mycenaean word-representation and their textual contexts,¹⁵ I shall be happy to accept them. Until then, I agree with John Chadwick that *ma-ka* = Mṛ Γṛ is unacceptable.

According to AGS, I distort the translation that Chadwick offers of the key phrase *tu-wo-te-to*.¹⁶ They write that Chadwick interpreted this phrase as “established a rite of burnt offering”, but I translate it as relating to “ceremonies, according to Chadwick, that have to do with aromatic incensing”. Scholars should read Chadwick, *Minos* 31–32, 1996–97, pp. 294–295. What AGS quote as Chadwick’s interpretation is in fact part of Chadwick’s rejection of their interpretation of this phrase as ‘when sacrifice was offered’. Chadwick points out that AGS erroneously take Mycenaean *thuos* as equivalent to classical *thusia*. He then proposes his own interpretation, that *tu-wo-te-to* is an aorist passive from the verb meaning ‘make fragrant’. Chadwick explains that *tu-wo-te-to* “could perhaps describe perfuming or incensing as a religious rite”. It is Chadwick’s interpretation that I correctly paraphrase and use.

These are enough examples of problems with the use of cited work. Let us turn to representative problems of interpretation. For other examples, interested scholars may read my reviews and eventually, I hope, the published versions of my preliminary conference papers which AGS here use and cite.¹⁷

¹⁵ Cf. the full list of theonymic and anthroponymic possibilities I discuss in *Minos* 35–36, 2000–2001, pp. 481–482.

¹⁶ *Kadmos* 42, 2003, pp. 17.

¹⁷ *Kadmos* 42, 2003, pp. 15, n. 1, 23–25, nn. 24–26.

Scholars should read AGS¹⁸ and then read my discussion of the quantities on tablet **Ft 140**.¹⁹ AGS propose that here and only here in the Linear B corpus, the increment T stands in a 12:1 relationship with the whole dry unit of measure. Everywhere else, including in group ration allocations (Pylos tablet **Fn 7**), as I point out in my review, the ratio is invariably 10:1. In hundreds and hundreds of entries, we get no instance of T 10 or T 11, which we would expect to occur somewhere if the ratio were 12:1. In my review, I suggest that on **Ft 140** we either have a scribal error – and I cite parallels – or the scribe has rounded off a large total (much less likely), or the tablet has been misread by the editors. At the time of my review I had not seen the tablet, so I gave the editors the benefit of the doubt. After the publication of TOP, I was permitted to examine the tablet in the Thebes Museum. It is clear that the reading of T 7 in line .5 is wrong – tablet irregularities on the upper right and the lower left of the digit strokes have been mistaken for digit strokes. The correct reading in line .5 is T 5. This produces the normal total T 5 (line .4) + T 5 (line .5) = GRA 1.²⁰

In the handout for my still unpublished paper from the Vienna conference, I discuss, at greater length than my brief reviews allowed, what I think is the consistent misidentification in TOP of phonogram *65 = *ju* (standing for ‘son’) as ideogram FAR. One serious problem is that the totals in all cases on the **Fq** tablets are for HORD and do not take FAR into account at all. The sign in question is also run up against the preceding phonograms and not separated from them, even where there is space to do so (e.g., **Gp 236.5**, see below). These problems are eliminated if we read the sign as *65 = *ju*.²¹

AGS claim that it is ridiculous²² to propose that 9 to 11 individuals (i.e., fewer than 20%) of the 60 or so personal recipients on the Thebes **Gp** and **Fq** tablets are designated as ‘son’. Is this true? The designation of ‘son’ can be handled in two ways in Mycenaean: by patronymic and by the use of a Mycenaean word for ‘son’. On Pylos

¹⁸ Kadmos 42, 2003, pp. 21–22.

¹⁹ Minos 35–36, 2000–2001, pp. 476–477.

²⁰ One of the reasons for my originally positing scribal error as an alternative explanation is that I thought the scribe himself in making a quick calculation might have misread the entry in line .5 in the same way as the editors did.

²¹ AGS, TOP, p. 396, admit that the entry on **Gp 227.2** refers to an individual as *u-jo* ‘son’. That same individual occurs five other times followed directly, without spacing, by the sign that is either FAR or *ju*. This is the starting point for my query.

²² Kadmos 42, 2003, p. 25: “quelque chose de ridicule”.

tablet **Aq 218**, six of twelve entries (50%) are designated as ‘son’, five by patronymic and one by the word ‘son’ (*i-ju*).²³ On Thebes tablet **Fq 254[+]**255, a mere three recipients out of ca. twenty-seven (11%) would be designated as ‘son’.

AGS agree that one version of the word for ‘son’ in Mycenaean would start with sign *65 = *ju* used to represent /hu/.²⁴ José L. Melena and I propose this for tablets by the principal scribes of the **Fq** and **Gp** series. Our interpretation eliminates problematical readings such as FAR , VIN on **Gp 124**, for which even now AGS have not come up with any convincing parallels.

AGS reason that the parallel phrase *ra-]ke-da-mo-ni-jo-u-jo* on tablet **Gp 227.2** by scribe 306? disproves my hypothesis, because scribe 306? here does not use sign *65, but sign *10 = *u*. However, this kind of fluctuation in phonetic renderings between standard signs and rarer signs in the syllabary, especially between one scribe and another, is well-attested in the Mycenaean texts. Among other things, it has been used as a tool in confirming palaeographical identifications of scribes. Here it would corroborate the editors’ doubts that **Gp 227** was written by hand 306.

Concerning sign *56, which AGS propose to read as *ko*, interested scholars should read the two succinct paragraphs in my review that treat past scholarship on the dossier for sign *56 and its place in the structure of the Linear B syllabary.²⁵ They may judge for themselves whether AGS’s reasoning concerning the Thebes tablets²⁶ is sufficient to refute thirty-five years of close scholarly work on a systematic dossier of word and sign occurrences throughout the Linear B corpus. They may also judge whether, as AGS allege, the standard interpretation is based solely on the contextually certain equation *56-*ra-ku-ja* = *pa-ra-ku-ja*.

Contrary to what AGS claim,²⁷ *potnia* is not used as an epithet in Linear B texts, even if it becomes one in some uses in historical Greek, as Chantraine points out. In my Vienna paper, I explain that

²³ See F. Aura Jorro, *Diccionario griego-español*, vol. 1, Madrid 1985, pp. 292–293, s.v. *i-**65.

²⁴ Kadmos 42, 2003, p. 24. Given the numerous hypotheses advanced to explain the complex history of the various Mycenaean and historical Greek forms for the word ‘son’, it is not unusual to propose two alternative possibilities to explain the usage in the new Thebes tablets.

²⁵ *Minos*, 35–36, 2000–2001, pp. 484–485.

²⁶ Kadmos 42, 2003, p. 30.

²⁷ Kadmos 42, 2003, p. 20 and n. 13.

even the sole historical parallel for *o-po-re-i* (in an inscription from Akraiphia) has a simple interpretation as ‘upon the mountain’, a fitting epithet there for Zeus. There is no reason to interpret the lexical item in the Fq tablets as anything other than an anthroponym (cf. classical Greek *Orestes*). If there is no Zeus and no Mother Earth on the Fq tablets, there is also no need to interpret the word *ko-wa* ‘girl’ as Kore. We should let the clear preponderance of the well over 50 names of human beings and mundane human professions dictate our interpretation of the few remaining items in the Fq texts. ‘Basket-carriers’ are ‘basket-carriers’, not carriers of ‘lightning bolts’ or ‘mystical torches’.

On Fq 207.3, *pace* AGS,²⁸ the reading *e-qa-do* MA 1 ‘PO 7’ is not ‘absurd’. I agree with them that readers should consult the facsimile drawing and judge for themselves. Scholars should also consult the photographs and tablets. If they consult the fine tablet drawing in TOP, p. 94, for tablet Fq 236, they will see that on line .5 sign *65, *pace* AGS,²⁹ is not separated from the *-no* of *ku-no*. The editors may have been misled by the misalignment of the sign they call FAR in their faulty transcription (TOP, p. 95).

I expect that some of the many interpretations I have advanced here and in my reviews and unpublished papers on the new Thebes tablets will inevitably be proved wrong as different scholars examine the full data now available and see things that I have not seen. I hope, however, that my discussion of the overall Mycenaean evidence will help other scholars weigh with due skepticism the validity of many of the ‘religious’ interpretations of the new Thebes tablets proposed in TOP.

²⁸ Kadmos 42, 2003, p. 30.

²⁹ Kadmos 42, 2003, p. 25.