

## How dysfunctional can an "archive" be? The case of Linear B.

### Introduction

The decipherment and translation of Linear signs on a clay tablet from Pylos (PY Ta 641) as the Greek word for "tripod", followed by a picture or "ideogram" of a tripod, together with some other similar pictorial correspondences on the same and some other tablets, has been taken as a strong indication that the identification of Linear signs with Greek language sounds, as proposed by Michael Ventris and John Chadwick in the 1950s, is correct. Another is that the identification regularly, though far from invariably, produces Greek sound patterns, words of various kinds.

Initially, Ventris and Chadwick did not expect their analysis to be accepted as wholly right, judging by the caveats in *Documents in Mycenaean Greek (Documents)*<sup>1</sup>. But in the years since its publication there has been little modification of their original proposals and translations. What I call for convenience an "Administrative Model" (AM) has become entrenched. One possible reason why emerges from *The Decipherment of Linear B (Decipherment)*<sup>2</sup>. Commenting on the sound values of the "syllabic grid" used as its basis, John Chadwick wrote: "The vast majority of these values have never been questioned, except by those who reject the decipherment entirely. It is a case of all or nothing (page 74)." To which I can only say "No it isn't, neither in principle nor in detail, and they should have been."

Certainly, the AM might claim at least two substantial achievements, to which many individuals, in addition to Ventris and Chadwick, contributed in many different ways:

(a) establishing that there are standard forms that underlie the Linear syllabograms (signs representing syllables), that there is an ideal or "standard form" of the Linear sign "A" or "WA" (shown at right). This reductionist approach was an essential prerequisite to understanding the sounds of those signs;



(b) deducing the sound values of those signs with the help of a rigorous analytic framework, plus some inspired and reasoned conjecture.

But the AM also has three fundamental, inter-related and recurrent flaws:

(a) it routinely fails to examine its evidence in the round for *all* its visual clues before proceeding to inference. Instead, the theory has been based on and contextualised by prior assumptions about the purpose and meaning of objects on which Linear signs are found. In addition, much of the analysis that produced the AM was based not so much on scrutiny of the primary evidence, but photographs and, especially, drawings of what a very small number of individual scholars thought they saw by way of signs, and later scholarship has been based even more extensively on "normalized" transcription and transliteration of those signs (that is, text printed as "ti ri po" etc)<sup>3</sup>;

(b) the AM assumed that its physical identification and interpretation of the role of individual Linear signs (syllabograms, ideograms and numerals) was largely correct, and that its understanding of the sound values of individual syllabograms (the signs representing sounds) was also complete. Yet the AM was almost immediately forced to make additional assumptions about the variability of those values, and it was unable from the outset and it has remained unable ever since to translate or interpret large parts of the Linear corpus;

(c) the AM does not tackle such problems or difficulties by re-examining the primary evidence or its own approach to it, but instead invokes generally unverifiable or unfalsifiable assumptions, often compounded and circular. Effectively it elaborates its original assumptions at (a) by refinements to or variations of those assumptions, with any translated or interpreted text permitted to serve only in a kind of blushing ancillary role.

I have addressed (a) and (b) in *The Problem with Linear B*<sup>4</sup>. Here, I want to focus on the third of those failings, to show that - even only on its own ground and in its own terms - the AM is untenable. To do so, I will examine three broad areas of interest - sounds and language, accounts and archives, and translation.

1 For fuller details of most sources, see Annex 2 of *The Problem with Linear B*, referred to in note 4 below.

2 2nd edition, Cambridge University Press, 1967 (Canto edition 1990)

3 What "transcription" and "transliteration" mean varies from author to author. I see no point in giving yet more definitions, and happily leave it to the intelligence of the reader to work out the nuances.

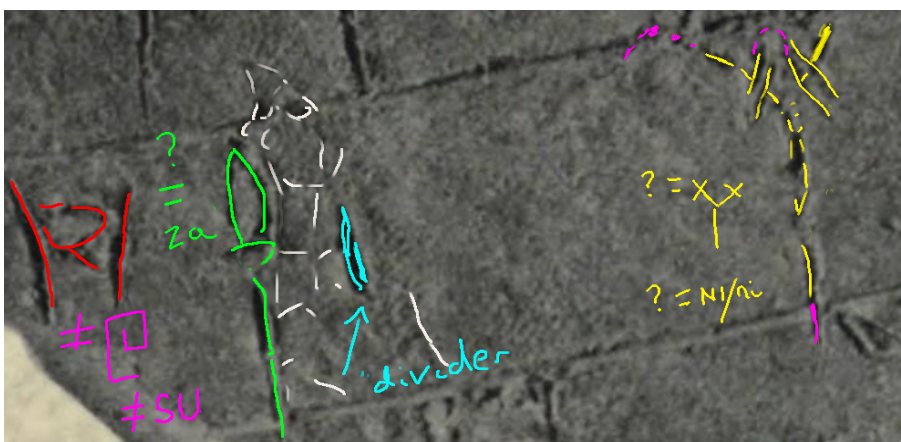
4 Referred to below as *The Problem*. Available at <https://hcommons.org/deposits/item/hc:20833/>

However, one of the AM's key assumptions is that Linear B texts or lists (in its eyes) normally comprise an "ideogram" and number, prefaced by syllabograms, mainly for names. The ideogram is envisaged as a sign, more pictorial by nature or origin, that conveys an image or caricature (sometimes remote) of a "commodity". Ideograms and numbers (and also measures) are thus central to the AM's whole administrative approach, and whilst Section 3 of *"The Problem"* has already illustrated the AM's multiple weaknesses in reading signs generally, it may serve as a necessary reminder of the conclusions there reached and also useful preliminary to what follows further below to re-examine the basis for perceived ideograms first.

I must emphasise that where I use adjectives like "flawed" or "imprecise", I refer wholly to the demonstrable effect, not to intention or competence of individuals. For some of the difficulties of the evidence base, see Annex 1 of *The Problem*. To which one might add the intervention of two World Wars and many other difficulties and tragedies, personal and otherwise.

### "Ideograms", numbers and measures

The AM's ideogrammatic evidence base is flawed by its misreading of signage, a process abetted or compounded by cumulative associated assumptions, with almost immediate implications for historical understanding. For example, the perceived prevalence of figs in the Mycenaean diet appears to stem from the AM's identification of the common Linear syllabic sign "ni" as also denoting an ideogram "figs" (conventionally written NI). The identification flows mainly from KN F 841 where the perceived syllables "su-za" appear before the sign "ni" or NI and a numeral (not shown below). The AM assumes that su-za equates to Greek suka or sukia, figs; that syllabic "ni" represents only the sound "ni"; and that, since, as an apparently isolated sound, it *appears* to have no syllabic value in KN F 841, its presence can only be explained if the scribe felt a perceived obligation to write an ideogram NI for figs as well as the word's syllables (su-za)<sup>5</sup>.



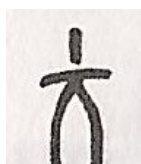
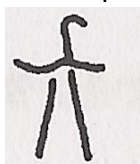
But the sign "su", maybe "za" too has been misread. No matter how allegedly imperfect the photograph (my enlarged extract from *Scripta Minoa II* plate LXII) the formation of putative "su" (red) cannot match the standard form (purple). The perceived "word divider" (turquoise) is redundant, or the enormous gap between the "divider" and the perceived "ideogram" NI, as, for no organisational reason, it occupies more than the entire space of the three preceding signs. The "divider" appears to contribute to the suggestion of a right-profiled standing or striding figure (white). The formation of the perceived NI (yellow) takes no account of closely adjoining incisions (purple), which may suggest

<sup>5</sup> One might well have expected a fig ideogram to be based on the sign su/ SU (su-ka). The AM proposes a NI ideogram may have been derived acrophonically from a pre-Greek or Minoan fig word that begins with "ni" (nikuleon). The AM no longer reads the NI ideogram in two other purported "su-za" tablets, KN Gv 862 and 864, but even if it did, the readings would be open to the same objections as pertain to KN F 841.

that it is not or not only NI. Multiple other lighter interventions and formations (such as the moulding of the surface) are still visible in what is arguably no bad photo, but they are unlikely to be the product of erasure, for there is no real trace of obliterated incision. They may contribute to more faintly delineated (not necessarily incised) signage or other features between the perceived word divider and perceived NI.

Secondly, and quite simply, taking "su-za" as equivalent to su-ka or su-ki-a is a considerable phonetic licence. As a result, the basis for identifying NI as an ideogram for "figs" is unsound in terms of both signage and language. As it is also in practice. For example, the Pylos Ab series purportedly features commodities and personnel in the form of wheat and "figs", plus "boys" and "girls" and the repeated signs TA and DA, which the AM now assumes are acronyms for female and male "supervisors", not primarily on the basis of any translation of the signs, but rather, apparently, calculations about the relative quantities of food such (assumed) supervisors might consume. The inference is then made that "TA" might be an abbreviation for *tamias*, *ταμίας* or similar, steward. Yet the "supervisors" (with one exception, 388, for a solitary TA) only appear on the line recording "wheat". No less remarkable, the grain and fig numbers on each tablet are always the same. All of which calls for yet more assumptions, the accumulation of which is another indicator that the premises on which the reading or readings are based are likely to be wrong.

Experts often cannot agree on the form a purported ideogram sign takes or what it stands for. Thus (from left to right below) in *Introduction* (page 42) and *Companion* (Volume I page 64<sup>6</sup>) the ideograms for "woman" are drawn quite differently. In the case of a real tablet, such as KN Ap 639<sup>7</sup>, it is far from evident that the ideogram concerned (if such it be, for each occurrence has a distinct pictorial quality) is a woman and not a man. The AM reads the alleged "ideogram" as MUL



(woman). Yet pictorially its manifestation often seems closer to the ideogram that *Companion* posits for VIR (man, at right in the row). As tablets go, KN Ap 639 does not look as though it is designed or laid out only for the personal reference of the scribe. Yet I cannot see how anybody could tell from the tablet incisions either which

ideogram was intended or what it might in any case represent. How could scribes, or a scribal system, be so opaque over such a basic, and one might have surmised important difference?

On KN Ra 1540<sup>8</sup>, signs read by the AM, to-sa pa-ka-na, are taken as meaning *τοσα φασγανα*, *tosa p(h)asgana*, "so many swords", followed by a drawing of a sword. "In Linear A, as in many other scripts of the Mediterranean Bronze Age, the ideogram did not die out but continued, in co-existence with the phonetic sign (*Introduction* §169)." But the statement is a generalised assumption, deployed to explain a specific sign and conform with a preconception about the nature and purpose of the tablet. The sword ideogram is, according to *Introduction*, carefully drawn, but why in that case has the shape been misinterpreted as "sword" rather than "dagger", as *Introduction* proceeds to advise? It does not necessarily resemble either. Note in particular the details in and around the alleged point and the curved incision beneath the "hilt". Similarly with an "ideogram" on the so-called "sealing" KN Ws1704. Arguing from the as then known find context, *Documents* asserts "the meaning of the word pa-ta-ja...is thereby assured", by which it seems to mean "arrow" (page 264). But according to *Introduction* (§ 320) "It is evident that pa-ta-ja and pa-ta-jo are alternative forms of a word which appears later as <palton> *παλτον*, 'light missile'."

For long there was uncertainty over the true identity of putative animal ideograms, and it remains for some others still. So much for "easily recognisable" (*Scripta Minoa I* page 47). But if modern experts cannot agree on what an "ideogram" represents, why should contemporary readers have fared any better? Ultimately, the AM's response is to say "It doesn't matter. The meaning would have been known to the scribe." I do not see why it would have remained clear, even to him or her, after the passage of days or months. Besides, if the temporary note was transcribed to a more permanent record (as the AM sometimes posits) would the "ideogram" suddenly have become any

6 A Companion to Linear B, see note 1 above.

7 Viewable by clicking on An1910\_218\_o at <https://web.archive.org/web/20171223213335/http://sirthurevans.ashmus.ox.ac.uk:80/collection/linearb/imag.es.php>

8 Viewable as above by clicking on An1938\_706\_o

clearer? If so, what's the problem with making it so in the first place? The point of an ideogram is that it should be a recognisably distinct abstraction, and also consistently represented. If it isn't, its identity becomes problematic.

Nor does the AM explain inconsistency of practice. On its reading, some tablets, such as the Knossos Ld series (cloth), contain a syllabic description (a word, possibly meaning cloth or similar) plus a *cognate* "ideogram" for the cloth commodity, but many others, such as some of the Lc series or Pylos Cn series (animals) or Pylos E tablets ("land tenure" etc), do not. The inconsistency has implications for clarity of meaning (forcing the AM into numerous unverifiable hypotheses), but it is the inconsistency itself which is puzzling, as scribal practice.

Similarly, the AM never explains the widespread inconsistency the tablets display in their use of perceived "ideograms" as, effectively, abbreviations for whole words. Thus according to the AM many tablet series are about "personnel" with ideograms for man and woman. Yet when it comes to writing "boy" and "girl" the Linear B scribes appear to have been incapable of resorting to or inventing such device, and instead nearly always write out "ko-wo" or "ko-wa" in full, often with apparently futile and laborious repetition.

The AM assumes that several perceived ideograms are exactly the same design as syllabograms (for example the signs "MA" (purportedly WOOL), "NI" (ditto FIGS) and perhaps "ZE" (ditto PAIR). It further assumes that they are acrophonic, MA for mallos, μᾶλλος, wool fleece, and so forth. The identification of NI as "figs" is probably based on a misreading of the Linear signs (see above). But more generally, the AM never explains why this acrophonic principle should apply to the formation of so few ideograms. There was no shortage of syllabograms, so why wouldn't scribes have selected many, if not all other syllabograms to stand acrophonically for "commodities"?

The AM makes frequent resort to acrophones or acronyms to explain perceived and isolated signs which it cannot otherwise explain (such as "o" for o-phelos, ὄφελος, debt). Now in already well understood and repetitive texts (such as later papyri or more modern records), it is reasonable and necessary to accept the presence of acronyms. But in texts whose meaning is largely unknown it is an inherently insecure procedure. It is possible to "explain" absolutely anything, by way of signs, acrophonically, but it is merely a way of reading our own assumed meanings into a text. For example, "the dog" is plainly not the definite article plus "dog" (because "we know" the "old" English we are looking at didn't yet have the definite article), but, as it occurs in what "we know" are accounts, it must stand for "t.h.e." or "transferable hedged entry". Similarly "a" and "an" (that is a.n.) stand not for an indefinite article "a" and "an", but "amortized" and "amortized negatively".

In the case of the Linear signs, it is possible that some or all were originally based on the acrophonic principle. But if so, would scribes have then have used those same signs as different acrophones? For example, whatever word may have been the basis for the acrophonic derivation of the Linear sign "o", it is unlikely to have been ophelos, debt.

"But ideograms are always followed by a number and generally preceded by a space or word-divider. They cannot just stand for a single syllable, like "MA" or "NI" as those mean nothing as words. So they can only be self-standing."

Yes, the *perceived* ideograms are often followed by *perceived* numbers, but otherwise:

- they are not always preceded by a so-called word-divider or a space, and spaces in Linear tablets are inherently problematic for many reasons (your perceived "space" may not be mine, and is it really empty, and if it isn't, does it contain trace syllables?);
- the mark which the AM reads as a word-divider may not be that at all, either physically or in import (so in KN F 841 above it appears rather to be helping to inform a standing figure);
- the AM's understanding of the syllabic value of syllabograms may be incomplete, and its understanding of number signs incomplete too. See below and Sections 3 and 6 of *The Problem*.

Ideograms are reportedly not found in the later Cypriot syllabary, widely believed to share common origin with the Linear scripts. Conventional thinking is that the syllabary's ideograms were lost over time, but if the *raison d'être* for all such scripts was administrative, and if they were also scribal notes, ideographically based, as the AM also proposes, the loss of such an essential component as ideograms is somewhere between hard and impossible to understand. At the very least, their absence from the Cypriot syllabary should give pause for thought as to the strength of the evidence for their existence in the Linear scripts.

The root problem with the AM's conception of "ideograms" is that it has simply assumed from the outset that they are ideograms and not, for example, pictorial elaborations of one or more syllabograms, just as many other elaborated signs are elaborations of syllabograms.

Numbers

Indisputably some Linear signs are number signs, but:

(a) many alleged numerical incisions are *appallingly* badly made and positioned if they are meant to be a real numerical record. That is not a consequence of wear and tear or photographic inadequacy, but original design. See the discussion in Section 3 of *The Problem*;

(b) I have not conducted a statistical analysis, but my strong impression is that numbers between 13 and 20 are under-represented, whilst the tablets contain a large number of records relating to what appear in some cases to be almost risible ones, twos and threes, for example KN K 1810<sup>9</sup>. The distribution appears to be distinctly odd for a genuinely administrative archive;

(c) number signs may still have syllabic value, as in some modern "texting" practices and use of things like "2 4 T" ("Two for tea"). It is the inverse of the later Greek practice of representing numbers alphabetically;

(d) evidence of numbers does not entail - at all - the administrative *record system* envisaged by the AM.

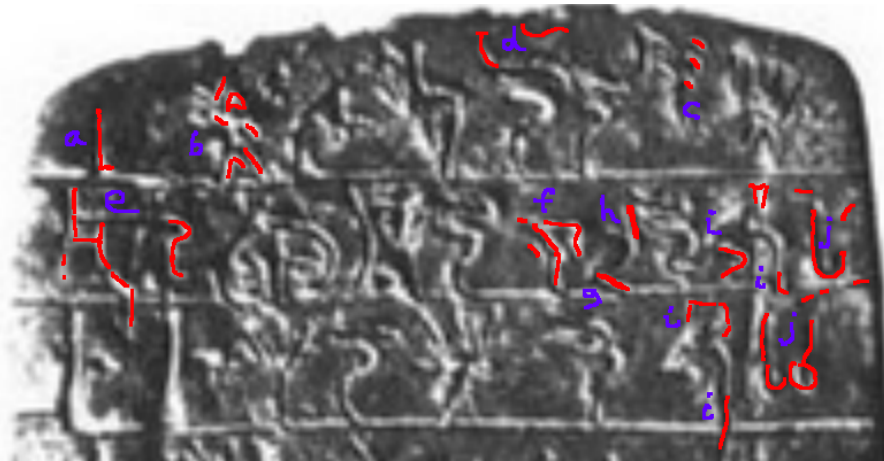
Measures

The AM believes that the Linear texts also contain signs for weight, dry and wet volume measures. I do not intend addressing that assumption fully because many of the points made about ideograms and numbers apply to the AM's purported identification of volume signs too. But I will illustrate the difficulties in its reading by brief analysis of the first few lines of KN Fp 1 (now possibly completed by KN Fp 31), a key text for the AM interpretation of liquid measures. I give below for the first three lines the photograph and drawing from *COMiK* (see note 1, but *Scripta Minoa II* has an equally serviceable photo), and transliteration from the Damos database.



- .1        de-u-ki-jo-jo    'me-no'
- .2        di-ka-ta-jo / di-we    OLE    S    1
- .3        da-da-re-jo-de    OLE    S    2

9 Viewable by clicking on An1938\_855\_o at the same address as note 7.



In line 1, top left, the AM (a) ignores (though *Scripta Minoa II* did not) opening incisions (marked in red), nor does the second sign (b) conform to "de" nor (c) the last syllabogram to "me" as the incisions are dots, nor is the additional detail, for example above putative "jo" (d) explained. In line 2, the AM interprets (e) the first "di" only by ignoring incisions and identifying a central vertical stem that has to run mainly in the line below. It bears no similarity to the second also highly conjectural "di" further in the line (f), nor does "we" (g) conform to the standard type; it lacks curve. Thus the context in which any further signs are interpreted is wholly unreliable.

As regards, the putative OIL ideogram posited in lines 2 and 3, in the first case (h) its reading relies entirely on ignoring a distinct upright incision between "we" and the ideogram. Similarly, the AM has ignored incisions (i) in order to identify the putative volume measures (the incisions used to form them markedly cross the horizontal grid lines) and even numbers (j).

The tablet signs are difficult to read as Linear script, but this is not a case of "if only we had a better photograph the real signs would be clearer". The fact that it may arguably be a relatively poor photo makes it harder to maintain that what remains visible is *not* deliberate incision. The AM's interpretation is, in many instances, based on what it selectively thinks should be there at the expense of what actually and visibly is.

#### Conclusions about physical reading of ideograms and other signs

The AM's conceptualisation of ideograms, a corner-stone of its argument, is based on an assumption, namely that it is self-evidently right, and depends on still further assumptions to try and make it work in practice, both in tablet series and individual tablets.

The observations in these few examples (and in Section 3 of *The Problem*) could be repeated across any Linear object and in relation to any signage. The AM's physical readings are selective, inconsistent and imprecise. It assumes "tablets" contain only scribal signs, that related incisions are of more or less uniform depth, that others (and other interventions like shape, moulding, colour) can be ignored. Its generic antidotes to the evidence for those additional features, such as "damage", "dirt", "erasure", "Only the scribe knew", "There was more than one scribe" or "The photograph isn't good enough" are all and only palliative assumptions, often inherently implausible, contradictory, and immune to falsifiability or proof. Nor can they remedy all the defects, such as basic imprecision or failure to explain not just occasional but overwhelming variation of same sign formation on single tablets. Where baffling sign variations have to be recognised, the AM resorts to positing a proliferation of ideograms and syllabograms.

It has become a key premise of the AM that differences in sign formation reflect different scribal hands and that those can be traced across different tablets. But, again, such analysis does not adequately address differences of the same sign within a tablet. The AM's further assumption, that those then also reflect different scribal hands, is implausible where it is irrational to assume that different scribes could ever have been needed, or where the variation shows no other indication (than the variation itself) of being due to a different hand. For the assumption is self-evidently

based on the yet further assumption that a single author was incapable or undesirous of the variation. But just as a single author (unless human competence has changed) is perfectly capable of creating divergent forms of the same "sign" (whether large or small differences) on a single object, so apparently similar signs across different objects may not, on close scrutiny, be as similar as thought, or coincidental, and either way attributable to different creative authors, not the same one.

The AM has assumed that what are, in its eyes, pictographic representations of signs are primitive because they are pictorial and they must therefore predate the Linear scripts. But the reasoning is a *non sequitur*. The AM has - in my view - rightly identified certain standard sign forms. The Linear scripts contain numerous, divergent elaborations thereof, many pictorial, some highly pictorial in quality. It is unsafe to infer that what look like pictographs may not similarly be pictorial elaborations of the standard forms that underlie the Linear scripts. The same may be true of perceived "ideograms".

### Sounds and language

The AM's failure to identify signs fully and accurately - as demonstrated above and in Section 3 of *The Problem* - means that, in principle, it is difficult to illustrate problems arising from its understanding of the related sounds and how they work. If some or many of the signs have been misidentified, so have the sounds and so have the words. Nonetheless, it remains reasonable and desirable to analyse the AM in its own terms, on the basis of the signs and sounds it thinks it has identified, only at a higher level than might otherwise have been possible and without giving multiple examples.

*Documents* declares (page 90) "The development of prepositions [in Mycenaean Greek] is already complete." Yet Greek is very rich in prepositions (words like "to", "from" etc). The AM is very poor. For example, the Mycenaean Vocabulary and other indices in *Documents* show:

- no dia, δια ("through" etc), eis/ es, εἰς/ ἐς ("into"), ek/ eks, ἐκ/ ἐξ ("out from");
- en, ἐν ("in" "into") only in perhaps half a dozen compounds;
- apo, ἀπο (apu, ἄπυ) ("from"), meta, μετὰ ("with" "among"), (k)sun, ξυν ("with") in a total of perhaps a dozen instances and nearly always only in compounds;
- epi, ἐπι (only on the assumption that the Linear B equivalent is opi, ὀπι, "on" "upon"), paro, παρο (several different meanings), amphi, ἀμφι ("about"), all relatively though not absolutely frequent.

Against the rich evidential background of many thousand Linear B objects, including putative "fragments", the prepositional distribution that flows from the AM is impossible, a Greek in which several prepositions approach something like the later frequency, but other main players are evidenced not at all or mostly only in compounds. Δια, ἐκ and ἐν have plausible Indo-European origins. Administrative accounts, which the AM assumes the Linear B texts to be, are precisely a genre where one would expect intensive use of prepositions ("in" "out" etc).

The AM regards the Linear B "archives" as palace accounts or similar. But they contain no common word for "palace" except indirectly in wa-na-ka-te-ro, ἀνακτερος, "of the palace", attested only some half-dozen times and an arguably dubious adjectival form of ἀνακτορον, palace. Me-ka-ro-de, μεγαρονδε, to the hall, is read in MI Wv 6. Yet (w)oikos|oikoi, (φ)οικος|οικοι is already established as "home", "house", "palace" in not much later Greek and is of impeccable IE credentials. It is also an obvious accounting term. The AM identifies I think only three cases: TH Gp158, TH Of 36, KN As 1517. It takes most occurrences of the sign group "wo-ko" as -ourgos, -ουργος, "worker", in various compounds, which may or may not be right. Possible evidence of the root, for example wo-ki-to, οικιστος, PY Cn 131, is in what the AM regards as a (non-translated) name. Any which way, the near-total absence of οικος from the AM suggests that all is not well.

Similar remark applies to allos, ἄλλος, "other" or "another". Plausibly IE and a *sine qua non* accounting term, but not one single instance in Linear B, according to the AM. So too logos, λογος, "word", "account", "rationale", and mhlon or malon, μηλον or μαλον, later ubiquitous for ovine and bovine livestock. Michael Ventris was initially troubled by the apparent absence of the later Greek definite article ((h)ο, ὁ, "the") from Mycenaean Greek. I still think he was right.

The consonants ξ and ψ (consonantal clusters "ks" and "ps") are infrequent in Greek, except as secondary constructs (a syllable ending -p or -k plus -sV-), but probably still more under-

represented in the AM's reading of Linear B where they are mainly confined to proper names.

To correlate its vision of the Linear language with Greek, the AM frequently implies that one needs to "compare" terms in the respective languages when, in truth, they do not share much similarity morphologically and are often wholly conjectural in the former (hence the AM's numerous question marks and other uncertainties). Thus "pe-pi-te-me-no-jo : presumably the genitive of a man's name /Pepithmenoio /, formed from the otherwise unattested medio-passive perfect participle \*πεπιθμενος from πειθω (John Chadwick, *Linear B Tablets from Thebes*, TH Ug 1)<sup>10</sup>." Translating a-mo-ta as "wheels", *Introduction* writes "a-mo-ta is the nominative plural of a neuter noun a-mo = ἄρμo... It is equivalent to later Greek ἄρμa... In Homer the noun ἄρματα means 'an assemblage of wheels, a chariot'.... Mycenaean uses a different word for 'chariot' [[i]-qi-(j)a, ippia, ἰππια], but one which can be paralleled in later Greek (§ 299)." The explanation obscures the fact that neuter \*ἄρμo is not attested in Greek (masculine ἄρμoς with different meanings is), nor does ἄρμa mean wheel, nor is it obvious why it should when it plainly means "assemblage" only. *Introduction* § 294 contains a longer example in PY Tn 316, but notes "The principal objections to Palmer's theory are that it necessitates the construction of hypothetical forms." Quite so.

On many, almost countless other occasions, the AM just leaves transliteration unexplained. So in Kn E 777, "for the ki-ri-te-wi-ja women" (*Documents* page 214). The unexplained may be better than the alternative. The signs ko-re-te ,\*"ko-re-ter", are taken as the title of some kind of mayor (*Documents* page 122), but "The Greek form is obscure (*Documents* page 212)." No it isn't. It isn't Greek. It would be a completely unevicenced lexical item, as would po-ro-ko-re-te, "vice- or sub-, a sense not preserved in any classical compound (*Documents* page 122)." Yet by circular reasoning, the existence of "koreteres" is taken as "evidence" for the "Mycenaean" social hierarchy, which in turn reinforces the notion that \*koreteres exist and that what they appear in is a "list".

One might have expected these and other oddities about the Greek that emerges from the AM would give pause for thought as to whether its identification of sounds (and signs) is accurate and complete. Even more so when within, at most, 300 years of the last Pylos tablets (as currently dated), forms of Greek must have already been current and widespread that gave rise directly to the oral formulae of Homeric Greek. Yet Homer's Greek is rich in prepositions and the definite article. Basically, it is unquestionably Greek, whereas the syllabic clusters, as analysed by the AM, rarely or barely so.

None of that matters much for the AM. For in its eyes the main reason why the Linear B texts bear little relation to later Greek is that they are of an intrinsically specialised nature:

"Since the tablets are without exception administrative records of a summary and abbreviated kind.... we must resign ourselves to the complete absence of literary or historical material; to a very one-sided vocabulary deficient in verbs, adverbs, prepositions and pronouns; and to only the simplest patterns of syntax (*Documents* page 26)."

That is the mainstay of the AM's approach to the language of the tablets. But what it represents is not a full and objective analysis of the evidence leading to reasoned conclusions, but a set of prior assumptions arrayed to explain the AM's failure to understand the Linear B texts.

Plainly, the discussion needs to move on to examine, in its own terms, the AM's assumption that the Linear texts are "without exception administrative records etc." But before doing so it is worth briefly reviewing the cogency and coherence of some of the numerous subordinate assumptions that the the AM makes in order to ringfence its vision of an impoverished Linear language.

"The scribes seem to have tried to avoid any monosyllabic word (*Documents* page 207)."

What that means is "our reading of the Linear B texts circumvents the need to tackle monosyllabic words." Like most languages, Greek is rich in many common monosyllabic and disyllabic words. *Documents'* Mycenaean vocabulary lists few. But why would the scribes have distorted their language in a manner so inherently unlikely? If the texts are scribal notes, common monosyllables are *exactly* the kind of entry to expect (words or concepts like "in" "out" "yield" "gone" "spent" "gross" "net").

According to the AM, Linear B uses a sign (transliterated as "|") to mark division. Essentially, the AM simply assumes that the *perceived* sign divides words. In some cases it may. One cannot infer that it does so always. If it exists at all (see Section 3 of *The Problem*), an alternative

10 "Linear B Tablets from Thebes", John Chadwick, *Minos* 10 1970:115-37. See note 1.



assumption would be that it divides phrases or units of sense. So signs between "word dividers" might contain several short, not just one long word, nor does its absence necessarily imply a single word. *Documents* itself thus translates o-u-te-mi as οὐ θεμισ (KN V 280 page 311); and to-so-de-pe-mo, τσονδε σπερμο(ν) "so much seed" (in the PY Eb series); and also e-ke-de-mi, ἔχει δε μιν, "but he or she has it" (PY Na 926), and ko-to-na-ki-ti-me-na found in the PY Ea series as κτινας \*κτιμενα)<sup>11</sup>. Yet despite recognising the possibility that the "word divider" may be much more than that, the AM has failed to explore the implications with any rigour.

From the AM's perspective, the advantage of assuming that the word divider is only a word divider is that it removes the need to identify shorter words, reduces the scope for ambiguity, and instead leaves often long and impenetrable syllabic clusters that lend themselves to the assumption that they are only common or proper nouns, maybe in recognisable Greek, maybe in something allegedly akin to an old form of Greek, or maybe in a non-Greek language altogether. Again, it does not matter much for the AM because the opacity of the results serves to justify its premise.

Another important consequence is that the AM does not need to consider elision, crasis, word juncture. It assumes that the script records the sound of whole words completely separately. Given the nature of everyday speech and later Greek (spoken) poetry, that is a very, very large assumption. It is also an easy one, but should the Linear B script be interpreted on the basis of our own personal convenience and preferences?

In response to Michael Ventris's concerns about the missing definite article (see above), John Chadwick observed in *Decipherment* (page 70):

"there was no reason to be bothered [ ]; philologists had anticipated its absence in the early stages of the language."

Similarly in *Documents* (page 87), "It seems most unlikely that any forms are to be explained as a definite article since this is clearly absent from many contexts where its presence would be demanded by classical usage."

Thus an assumption is deployed to "resolve" a problem. But both assumption and argument are flawed. "We know, therefore we need not look; we do not find; therefore we are right" instead of "We have looked, still can't find, therefore it may be an earlier stage." Secondly, even if "classical usage" could be defined, it begs the question to read it back into Linear B.

"The vast majority of these [sound] values have never been questioned, except by those who reject the decipherment entirely. It is a case of all or nothing (*Decipherment* page 74)."

Certainly, it was reasonable to base the initial decipherment of Linear B on a grid of signs and sounds corresponding to, say, English vowels and consonants. But it was wrong to go on to infer that Linear B writers and readers thought or spoke in exactly or only those same terms, in other words conceptualised sounds in terms of our vowels and consonants, or later Greek alphabetic characters, or modern phonetic or philological critical apparatus. It was, in any event, soon plain that they did not. Ventris and Chadwick had to invoke "spelling rules" almost immediately in order to explain, for example, that the sign for "so" in Linear B also stands for "soi".

Which should be more accurately designated a "licence", not a "rule". In *Decipherment* (page 97) Chadwick acutely pointed out that "All systems of writing are only approximations to the sounds of words."

Unfortunately, he never developed the insight in relation to Linear B. On the contrary, and despite its own licences, the AM proceeded to ossify the original identifications and *Documents* contains such unguarded expressions as "The transcription of the *Mycenean phonetic* [my italics] signs (page 153)" and "If the script represents accurately the phonetics of Mycenaean Greek (page 69)." Obviously, it doesn't. In *Decipherment* Chadwick again wrote "mi or u are much more restricted" than syllabograms involving "a" or "o" (page 96). But how do we know? *A priori* it is unlikely. According to the AM, most Linear B signs stand for at least two related sounds.

Similarly, having established that Linear B did not distinguish between voiced and voiceless consonants so that a sign could serve for both (as well as the aspirated forms), the AM then decided that of the two signs available for "dentals" one equated to "d" and one to "t" and similarly with "s" and "z". But the decision was arbitrary, based on the perceived coincidences between some signs and later Greek words. Such coincidences may be true, but that might not mean that

<sup>11</sup> There is a further useful illustration of the "word divider" difficulties in the discussion by Yves Duhoux on pages 339 and 363 of *Companion* volume I.

the coincidence is true in all instances.

"Nearly all the Mycenaean signs indicate a simple vowel... or a consonant-plus-vowel open syllable... there is no evidence for syllables of the type common in cuneiform, at, ak, etc., and tar, kar, etc."

So *Documents* (page 46). The statement is confused, but leaving that aside, what does "no evidence" mean? It can hardly be true that the language emerging from the AM's transliteration was so transparent that it relieved the AM of any need to explore such possibilities. There is an additional option, namely the conceptualisation of a sign or syllabogram not only as consonant-plus-vowel (ka, say) but as vowel-plus-consonant-plus-vowel (a-ka).

Again, in later Greek, the spelling of "ks" and "ps" is subject to long and widespread variability of practice, and their pronunciation similarly so. But instead of exploring whether and how Linear B might represent such clusters, the AM has assumed that, as word initials, they are always and only represented by the syllabograms k(Vowel)-sV- or p(V)-sV-.

Whilst the presumption of "summary records" can be deployed to substantiate absence of words we might otherwise have expected, it does not explain language irregularity, or why the Linear tablet Greek is apparently, on the AM's analysis, so odd. For that a different assumption is needed, one I have already cited above in a different context, namely

"Philologists had anticipated its [the definite article's] absence in the early stages of the language."

The AM uses the "early stage" to posit unevicenced forms, presumed to exist between known Greek and Indo-European or, as the preference seems to be, Proto-Indo-European, itself a construct. From the AM's viewpoint, the advantage of the process is that it is entirely unverifiable and unfalsifiable (as is the presence of non-Greek words). It is impossible to prove that any putative "archaic form" (or non-Greek form), however speculative, did not, potentially, exist. On the other hand, the "early stage" is also used to argue that Linear B words that otherwise look like normal later Greek cannot be so as they would be "anachronistic"; the signs must therefore stand for something else. Thus a sign group such as e-pi-ko-wo (PY An 657), cannot, according to the AM, be the later Greek epikouros, ἐπικουρος even though that might make sense in context.

But does the evidence of the Linear tablets, as read by the AM, support the assumption at all? In the case of o-u-te-mi and o-u-ki-te-mi *Documents* acknowledges that "The expression οὐ θεμις, οὐχι θεμις has an exact classical counterpart (KN V 280, page 311)." *Documents* (page 368) draws attention to the "exceptional word" me-ta-ke-ku-me-na in KN Sf 4428 to explain the disassembled nature of the accompanying chariot frame. If the signs are identified correctly, the word certainly is "exceptional" because its form is exactly that of later "classical" Greek μετακεχυ(σ)μενα. One might ask why such very close correspondence did not cause pause for thought as to the perceived "antiquity" of Linear B. Elsewhere we find *Documents'* "surprise" at such perceivedly late forms as e-re-u-te-ro-se, ἐλευθερωσε (e.g. PY Na 568, page 299), or ko-to-ne-ta, κτινεται (PY Eb 36, page 264), as they "appear earlier than one might have expected." In yet other cases, such as e-re-pa-te-(j)ο, ἐλεφαντειος, of elephant or ivory, the AM glosses over the apparent relative "modernity" of the term and the form. The implications of the (possibly correct) deduction that Linear B and later Greek are remarkably similar in their variable treatment and representation of the word for "son", "υιος"<sup>12</sup>, remain unexplored.

Other evidence for the antiquity (and hence obscurity) of the Linear language is no more convincing. The AM sound grid includes signs for "w plus vowel" and "qa qe etc" (labio-velars). Except in the case of "glide sounds" (see on), the AM assumes that Linear B consistently used "w plus vowel" for an IE inherited (later "lost") digamma (ϝ) plus vowel, and labio-velars similarly inherited directly from IE (\*k<sup>w</sup>a etc). The AM infers that Linear B must therefore be an old or precursor Greek, seeing that later Greek (aside from some localised variants) has lost the digamma and has no unique sign for IE labio-velars, those having largely shifted in speech mostly to dental ("t") and labial sounds ("p) represented by equivalent alphabetical letters.

But the use of a distinct sign for labio-velars in Linear B does not prove anything about how signs were sounded or spoken by users. Until at least the 16th century, Scots English were using "qu" and similar lettering for modern English "wh", but it is extremely unlikely that they were still pronouncing the words as "kw". We still differentiate labio-velars in English spelling, but

12 Yves Duhoux page 355 of Companion Volume I or *Introduction* § 111.

pronounce them variously "hw", "wh" or "w" or "h" according to dialect and idiolect. Similarly, Linear B may write the sign "qe" for later Greek "τε" ("te" "and"), but users may actually have pronounced it "te". The Spelling Rules section of *Documents* (page 45) already observes that "qu" is probably already written "ku" in Linear B, and goes on to note (perhaps prematurely) "qa" may already have become "pa" (page 81). But if the sign for those labio-velars was already capable of fluctuation, why not others<sup>13</sup>? In despite of which, the AM has consistently failed to explore the implications of variant spellings of original labio-velars.

The AM believes that Linear B uses "w" as a "glide sound" between "u" and following vowels, thus "u" followed by "o" might be written with signs for "u" and "wo". Importantly, that is a licence, not a "rule", and one that reflects the use of signs to capture words as sounded, not as historically derived from IE. For which same reason, it is simply unsafe to assume that Linear B always writes original IE "w" (the "lost" digamma). Some writers may have done, others not, depending on the word and how they heard it or wanted it to be heard.

The AM also employs the concept of "derivative" to explain irregularities. In KN Ad 670, *Documents* translates ri-ne-ja-o as "probably lineiaon, a derivative of λινον (page 159)." A strangely formed derivative which does not survive in later Greek at all (which has λινεος or λιναιος). So too pe-ki-ti-ra2-o "pektriaon from πεκω (PY Ad 694, *Documents* page 158)" has no evidential basis in later Greek. In PY Aa 240 *Documents* translates a-ra-ka-te-ja as "a derivative of ἡλακατη" (page 159). But ἡλακατη means specifically "distaff", and is far from being an obviously standard occupational term for "spinners" (the verbs being κλωθω or νεω). *Documents* does not explain why the form is not a-ra-ka-ta-ja. In similar vein, "Chadwick and Bjorck simultaneously recognised that the classical ταλασια, talasia (= ταλασιουργια, 'wool-spinning') is here [Pylos Jn series, purportedly about metal] used in the more general sense 'an amount allocated by weight for processing' (*Documents* page 352)." But if the explanation were right, then ταλασια would have been a useful generic concept, with no reason for its use being later limited to wool. A more reasonable and better evidenced starting point would be that the word (if the signs for it are right) always referred only to wool, as it does in later Greek. In which case, there is something wrong with the AM's interpretation of the rest of the Pylos series.

John Chadwick initially proposed that the language of Linear B was akin to the Arcado-Cypriot Greek dialect. The AM has later modified that view to include a strong element of Aeolic. Doric forms are also found, though less acknowledged, for example ko-to-na, ktoina, κτοίνα. In principle, it would also be more plausible to look for Ionic forms as well rather than exclude them on unevidenced and arbitrary assumptions about Greek migrations and Greek language development (and indeed the age of some Linear tablets, for which see Section 10 of *The Problem*).

The AM already acknowledges a dialect mix in Linear B. I do not know whether or how far that is right. But what is clear is that the AM doesn't explore the concept rationally or consistently. Thus *Documents* proposes κορινθια for ko-ri-si-ja (see the Index of Personal Names). But, despite positing that variation, so far as I know the AM has never posited the existence in Linear B of another, equally plausible example found widely in later Greek, namely "eV" > "iV", as theos > thios, θεος > θιος. Nor the possibility that ne-wo, νε(φ)ος might also have been pronounced or written ne-o or ni-o, and in particular, ni-jo. But why not? There is no proof that the writing "ne-wo" always represents continued awareness of an original digamma rather than perception of a "glide" sound. There can be no proof that all speakers thought, heard, spoke the same.

In sum, just as in later Greek, in fact almost any "writing", one *might*, in theory, find archaic or obscure dialect forms in Linear B, but that would be no proof whatsoever that all Linear B is archaic, or for that matter dialectally obscure.

## Conclusions about sounds and language

Discussion has been impeded partly by the prior difficulty of correctly identifying signs, partly by the AM's main assumption that the Linear tablets are "purely administrative" and their language consequently of a severely limited if not downright peculiar nature. Instead of analysing transliterations (and difficulties arising therefrom) on the basis of alternative hypotheses, the AM has invoked a series of subsidiary assumptions to safeguard its main premise. To that I now turn.

<sup>13</sup> *Documents* discusses a further example in qe-re-po-ta-o (PY En 659).

## Accounts and archives

"Our Linear B records...are *entirely* [my italics] concentrated on economic matters (Palaima in *Companion* Volume II page 113)." "The glimpse we have suddenly been given of the account books of a long-forgotten people (*Decipherment* page 101)."

How rational and internally coherent is the AM's premise that the Linear B tablets are "purely administrative"? Discussion now needs to embrace not just the Linear B script, but the slightly wider, archaeological context of the tablets' discovery. But if one looks forward to different argumentation, one is disappointed. For the premise is immediately and primarily supported by yet another set of assumptions. I discuss those and their plausibility below.

## Lists and names

"Thus [based on reading of purported "ideograms"] in many cases it was possible to deduce the general subject matter of the tablets before a single syllable could be read; almost without exception it was clear that they were lists, inventories, or catalogues (*Decipherment* page 46)."

The assertion carries an obvious circularity. "Thanks to (what we think are) ideograms and numbers, a given tablet must be a list, therefore it contains (only) additional names, I identify (what I think is) a name, therefore it is a list." As such, the assumption is also reductionist. It alleviates the need to explore signs and sign groups because they are, in the final analysis, we are sure, just ideograms, numbers, or names. It is reinforced by wilful interpretation of the "word divider", already discussed. But is the presumption of names and lists necessarily right, in principle or practice, even in its own narrow terms?

Michael Ventris's decipherment of Linear B as Greek was based on the hypothesis that certain signs might represent Cretan place names, crucially Amnisos. But whilst it may be true that certain Linear B signs also equate, say, to the sounds ko-no-so, it is not necessarily or always true that they therefore stand solely or even at all for the name Knossos. In theory, so long as the underlying language was in fact Greek, Ventris's identification would work, even if it was sometimes fortuitously or mistakenly deduced. Applying the decipherment's own spelling "rules", the same signs could also stand, for example, for the Greek g(o)nosos, γνωσω, I shall know, or koinosos, κοινωσω, I shall impart, or gonos sou, γονος σου, your offspring (or sex), and the putative adjectival ko-no-si-jo, Κνωσιος, knosios, maybe for g(o)nosis iou or iwn, γνωσις iou or iwv, knowledge of poison, or flowers or arrows. Similarly pu-ro for, say, phu-lo, φυλον, tribe, or pa-i-to for pa-i-ko-to, παικτο(v), to be played, or possibly παις του, "child (or slave) of" or "his child", or tu-ri-so for τυλισσω, I wind.

"This method of deduction.....studying the same words in different combinations, is often called 'combinatory'. Its usefulness... [leads] to some valuable conclusions about the meaning or sort of meaning possessed by certain words (ibid)." But that would only be true if (a) we could be certain what the "word" is, that is be certain about all its signs and syllabic sounds, and (b) it really is in fact the same word. As the limited Linear B syllabary is highly ambiguous, the same sign being capable of representing several sounds, the assumption is no less hazardous. Additionally, even one and the same Greek word may bear several different meanings. Essentially, the transliteration and translation (or, as often in the AM, the non-translation) of one sign group in one text does not mean its occurrence in another carries the same meaning whether (as commonly assumed) a (non-translated) name or something else. Finally, a "list" may be anything from a shopping list to an auction catalogue to a church service sheet. What appear to be "names" may be common nouns acting as proper nouns, adjectival nouns, adjectives, other descriptors, even whole phrases. Identification of one proper name does not entail other entries in a "list" are similar.

Linear "naming practice", as conceived by the AM, is also notably incoherent:

- no proper name of putative king or court personnel. Also few titles and sub-titles even though they normally proliferate at court. The (w)anaks, ἀναξ, presumed king, basileus, βασιλευς, "a kind of feudal lord (*Documents* 121), lawagetas, "people leader", and the similarly generic e-qe-ta, ἐπεται, hepetai, "followers." Apart from possibly the wanax, it's not clear that any of these are truly court personnel. But then we don't have much evidence for the court either (see above);

- proper names very rarely have patronymics as well (although patronymic name forms, like Johnson, are seemingly common). Only in some Pylos series are some of them purportedly regularly accompanied by common nouns (descriptors like "John, the cow herd"), but even then the practice is sporadic. Thus PY Eb 32 features a "priestess", with no other indicator as to which;

- common nouns which the AM identifies or invents to support its "list view" are no less ill-defined and vacuous. This runs all the way from *anax*, *basileus* and *lawagetas* to *khalkeus* and *flax worker*. Is a *khalkeus* someone who makes bronze from ore or who produces (all and any?) finished items or works in any metal? What is a "flax worker" (*ri-ne-ja*, *Documents Vocabulary*)? Does she harvest it or process it or what?

Overall, the naming practice is often colourless and generic, atypical of and unhelpful in genuine administrative systems and palace hierarchies.

### Functionality

The Linear scripts bear no dates (apart from some half dozen with disputed months) and no authentication (another obvious yet always missing personal name). "They were just an aide-mémoire." I have worked as a ledger-clerk and in a large bureaucracy. Any system which thought to control stock or maintain meaningful records without such basic steps is a complete nonsense. Repeat or partial deliveries (whether of the same or different sizes and qualities) are common occurrences, but impossible to remember, sometimes confused even with the aid of written or computerised records. Recording clerks take ill or die or abscond.

"The name of the responsible scribe, which never seems to occur on the tablets themselves, may sometimes have been recorded here [on sealings for boxes, baskets and so forth] (*Documents* page 114)." Of the sealings that survive, not one as yet contains such details. "They were recycled." Yet another assumption to explain a problem. There is and probably can never be decisive evidence for or against. It is unverifiable and unfalsifiable. But by way of counter assumption:

- a palace (of all places) "recycling" model is inherently implausible given the presumed availability of clay and slaves;
- why make life so difficult with all those extra "dongles"?
- the quantity and distribution of "sealings", "roundels", "nodules" and the like, all the generally small pieces of clay which the AM believes were either attached to commodities themselves as some kind of record note or attached to files (baskets or boxes) of other, grouped Linear documentation just do not square with the bureaucratic functions that the AM assigns to them<sup>14</sup>. For example, the use of clay for tablet labels (file tags) would appear (I say "appear" because I cannot really understand the thesis) to have involved the breaking of an old and making of a new clay label every time a "file" (however one defines that) was amended. Given the AM's assumptions about the nature of the palace administration, that could not be anything but a hugely frequent occurrence.

The use of seal rings to make seal impressions on clay is superficially plausible. But clay sealings and impressions are prone to crack. All in all, a cumbersome process, when wax would have been easier and more secure, and one defeating the archival and security purposes for which any sealing or tag was allegedly devised. The thesis that some sealing devices were stamped with a number of seal impressions for the quantity of the commodity is unworkable above twenty (maximum), and, again, cumbersome for any bureaucracy to implement hourly or daily, especially when it could easily certify written numbers with one or two abutting seal impressions, rather like lines after cheque entries. And it is only another assumption that the presumed bureaucracy needed to secure its workings in this way. Fiddling the books by manually amending individual records is not something a bureaucrat undertakes lightly for all sorts of reasons, and it would have been particularly difficult to do, undetected, on drying clay.

Aside from implicitly questioning integrity, the AM often alleges scribal error or "unusual" forms, as

<sup>14</sup> The knots in which the AM ties itself on the whole topic of "sealings" is reflected, for example, by Judith Weingarten in *Three Upheavals in Minoan Sealing Administration* (in *Aegean seals, sealings and administration : proceedings of the NEH-Dickson Conference of the Program in Aegean Scripts and Prehistory of the Department of Classics, University of Texas at Austin, January 11-13, 1989*).

demonstrated by a scan of the Mycenaean Vocabulary section in *Documents* for "misspelling", "unusual spelling" and the like. An alternative view would be that this large mass of critical marks and queries is instead indicative of the AM's own failure to understand what it is dealing with. The AM's image of a Linear B "scribe" is inconsistent and unconvincing. On the one hand a highly trained, possibly secretive guild enjoying several hundred years of development, especially by the presumed date of later Pylos tablets. Yet at the same time

- still taking stumbling first steps with experimental technology;
- still feeling the need to give a pictographic sign for an item (sword, say) when also writing out the word syllabically in full. But apparently not always so, which itself begs many questions;
- requiring drafts to be corrected by a senior scribe. Though again apparently not always so.

Nor do the Linear texts show much if anything by way of account heads and sub-heads or (according to the AM) account language. There is no processing of inputs or read-across of items, for example "received X from Y and transferred to Z" or "shortfall of X in Y so transfer from Z".

According to the AM, the texts also contain remarkably little by way of real-life transactions. So no contracts (of any kind), at all, and very few dispatch notes or receipts, divisions or responsibilities, or authorisations or conditions for proof of execution. Instead, the archives seem to comprise "assessments" and "inventories", that is largely non-transactional accounts. "Seem", because the AM often has to hypothesize even that gloss, the tablet itself being purpose-free.

"You misunderstand the Mycenaean economy. The tablets show it was redistributive, not market. Hence no contracts etc."

Not so. Even (in fact especially) a "redistributive economy" makes extensive use of exchange, receipt and dispatch. So to conclude "Finally, a great many *products* were *doubtless* (my italics) redistributed within the domestic economy (John Killen *Companion* volume 1 page 179)" is again to beg at least one question for which the AM provides no answer: where are the records?

To illustrate aspects of non-functionality in relation to just one series, the Knossos "D" series is purportedly about sheep. Despite its brevity and a purportedly common pattern or template, features which one might have thought ideally suited to insertion in a longer or running list, each single return, if that is what it is, occupies a separate tablet. But the tablet does not say what it is for, whether census, contribution or something else, nor its date, nor information about condition or age of animals. The AM provides no explanation for the tablets' apparently complex lay-out. For example, the alleged "place name" does not come first, but instead the alleged name of the shepherd in larger characters. Nor is any rationale given for the apparent splitting of entries between two lines, a complexity of layout eliminated in the AM's translation and totally pointless given the alleged content. Otherwise inexplicable single signs, for example "o" are taken as acronymic abbreviations for, say, ὀφελος, ophelos, debt. For the problem with "acronyms" generally, see "Ideograms" above. Finally, the process mechanisms are utterly opaque. Some tablets have no "collector", on others *Introduction*, for example, prefers "owner" to "collector". It is a big difference. Did the "collector" of animals bring them to Knossos and report to the scribe? Unlikely given the apparent numbers involved. Or collect the data *in situ* and somehow convey it to Knossos? No more likely given the missing authentication. A palace is not obviously the prime location for such primary records.

## Commodities

For the purported bureaucratic records of a palace economy, the texts are limited not only in terms of function but also "commodity". They are heavily weighted, according to the AM, to grains, wool and textiles plus some domesticated animals (overwhelmingly sheep, but also oxen, pigs, goats). They say little or nothing about economic production of fish, game, honey, fruits (excepting "figs", if only the identification were right, see "Ideograms" above), vegetables, and relatively little, given their presumed importance, about wine, olives or oil. (Wine and honey purportedly feature in the Fp and Fs series of "offerings to divinities at Knossos.") Despite their palace credentials, with the possible and partial exception of baked bread, the tablets say nothing about cooked or processed foods, even though some purportedly refer to traded herbs and spices. There is little differentiation of metals, a preponderance of "bronze", some gold, but almost no silver, tin, lead, or copper (unless that is what "bronze" means). Similarly, swords, helmets and tunics, but apparently

no shields and grieves, and very few spears or bows and arrows; chariot frames and chariot wheels, but no sub-components; rowers, and, rarely, boat-builders but no other common nautical types, nor their "boats"; nor any sub-division of land troops, for example as archers, sling-shots, spearmen or, for that matter, even plain "soldiers". According to the AM, the tablets say almost nothing about hunting, even though other Mycenaean evidence and later Greek myth suggest it was a favourite pastime.

Whether the tablets provide convincing evidence for production is also questionable. See, for example, John Killen's list of items on page 191ff of *Companion* Volume I. Note the curiously limited range and the number of question marks.

Nor do the Linear texts, according to the AM's own "translation", contain one single word, written in syllables, equating to a named unit of measurement. "That's because ideograms implied units. So the sign for wheat, say, means a certain unit of wheat." That *might* be so. But it is, in truth, a very big assumption that has to be made in order to preserve the AM's entire interpretation.

#### Duration and scope of the records

"After writing, the tablets were dried (not baked) and then generally [sic] filed away in boxes of gypsum or wood, or in wicker baskets, and stacked on shelves in rooms set aside for the purpose." Thus *Documents* (page 114). The AM has rightly modified that view over time. For if the tablets were as permanent as *Documents* originally envisaged, far more should have survived, even allowing for the vagaries of destruction and attrition, giving us a far more complete record of Mycenaean Greece. They would also be written up more concisely and more consistently than the chaotic profusion of single and widely divergent entries that we appear to have.

Interpretation now vacillates between the records being "temporary" or "semi permanent", either view regarding clay as only a relatively ephemeral medium. Yet the quantity and range of the assumed bureaucratic content are equally inconsistent with only a "temporary" or "semi" permanent archive. A very few days of the "command economy" envisaged in *Documents* and adopted by the AM would generate a huge quantity of diverse record (and "sealings").

Nor is there one definite example, out of many thousands, of a continuous text running over from one side to another of a tablet, nor (more realistically) from one tablet to another. Whilst some tablets may appear to the AM to follow a similar format or layout (such as the Pylos "land registry" series), the observation, if true, does not mean they constitute a single document with multiple pages, as any viewing of the tablet shapes should help confirm.

#### Clay

#### Configuration and colouration

The Linear A and B tablets (and smaller clay items) are non-standard, individually made, puzzling if they are bureaucratic records. The presumed availability of "slaves" should have made production of standardised tablets an obvious step, no matter how small or large the record to be made. According to the AM, tablets also fall into two basic shapes, "page" or "palm leaf". "Palm leaf" is a misnomer as the tablets are relatively thin and long, particularly at Knossos, where page tablets (certainly demonstrably whole ones) are very rare. The palm type is patently ill-suited to any bureaucratic record, particularly an inventory, because items are not listed on their own individual lines. Yet Linear B writers were simultaneously apparently using page tablets with line entries as standard practice. So it is not that they were unaware of the option.

So far as I can judge from available photographs, the clay surface of tablets is never, ever a level plane. On the contrary, it appears to have been deliberately moulded into sometimes very marked undulations. The same applies to the sides. The photographic record again makes it impossible to comment on the back of most tablets. Linear B tablets also routinely bear rule or grid lines, rather like our pre-ruled note paper. They are drawn free hand. Very free.

It is worth emphasising all of that: I know of not one single tablet with straight sides, a flat surface or horizontal lines drawn with a ruler (any level edge) or grid. For an administrative archive it is an astonishing, frankly incredible omission.

Unlike our ruled paper, where the line is a support for the formation of some letters, there is no evidence the Linear "grid" lines were designed or conceived as such scribal supports. Many Linear incisions cross them wilfully, so they are not exactly and entirely separators either.

According to the AM, only a relatively few Linear tablets have signs on both sides. In most cases it is not clear that the "verso" (let us say the shorter side) has anything to do with the "recto". In some cases, the "recto" has plenty of room to accommodate the "verso". But in any event writing on the verso after writing on the recto would have been cumbersome and precarious for account keeping as it would smudge entries. Yet it is equally unlikely that the tablet would have been put aside and then turned over for later impromptu use as the clay might have dried. The problem is only compounded in the case of so-called "roll bars" where purported account records are made on four or several sides of Linear A. Whilst such simple account keeping might be easy on, say, wood (rather like tally sticks), it wouldn't on clay.



The colouration of the clay varies markedly between tablets and within tablets. In some cases the colouration is targeted, that is limited to specific areas of the tablet. Thus MY Oe 106 (left) and MY Au 102<sup>15</sup>. The AM argues that tablets have been affected by later fire. That

may or may not be true, but it cannot explain the specificity of colour or discolouration, and does not preclude the possibility that the tablets were already coloured in various ways before any accidental later firing. Often their surface has been abraded or stippled or otherwise marked. Like the colouration, the preparation is at odds with bureaucratic record keeping.



### Durability and purpose

The AM assumes that clay was a more or less impermanent medium, and so things written on it were more or less impermanent too. But clay has considerable disadvantages as an ephemeral writing resource: heavy, unwieldy, damp. Its (oft fingered) edges smudge. Storing in baskets and boxes (as the AM sometimes envisages) would make matters only worse. Individual Linear tablets may be small and light, but in accumulation obviously not. Besides, if the users of Linear A and B wanted to make ephemeral or semi-permanent records, an alternative assumption would be that they used wax, inks, wood, maybe papyrus or vellum.

The Mediterranean peoples of the third and second millennia were probably well used to earthquake and fire, phenomena that normally have the effect of making it perfectly clear that wood, papyrus and parchment do not survive. Stone can, but may not be as easy to work as one would like with stone or bronze tools. Clay is not only easier to work, but, whether it be pots or records, it also endures. So contrary to the AM's assumptions, their creators might have chosen clay as the medium for whatever the Linear texts are, not because it was impermanent, but on the contrary precisely because they knew it wasn't. It survived. Demonstrably, they were right. It has. On that assumption, the Linear records may be things to which their users attached considerable importance and value. They wanted them to endure.

"The clay wouldn't have survived if it hadn't been accidentally sealed by destruction. So the use of clay might still be interpreted as denoting things ephemeral." The prior and underlying assumption is that the tablets record administration, predominantly of impermanent kind, therefore they would not have been worth firing, therefore any firing can only be due to accidental conflagration (sack or similar). So the assumption provides the "proof". I know of no hard, external evidence (I doubt there can be any) that some or all of the tablets could not have been normally fired by their creators at whatever temperature and however often. That hypothesis would imply that the tablets were not intended for ephemeral administrative purposes, but something else.

But as the clay evidently can survive, unfired, in a form that preserves its working for many years (see note 4 in Section 10 of *The Problem*), the whole argument is misbased.

<sup>15</sup> MY Oe 106 (?by Marsyas) CC BY 2.5, <https://commons.wikimedia.org/w/index.php?curid=471642>; MY Au 102 by Zde - own work, CC BY-SA 3.0, <https://commons.wikimedia.org/w/index.php?curid=31764187>



## Location

To date, the perception is that the overwhelming majority of Linear A and B finds has been made, in the shape of clay tablets, within and around presumed palace sites, not entirely in them but rarely more than a few hundred metres distant. The same is true of the various "sealings". The distribution has generally been taken as endorsement of the supposition that the texts reflect the workings of a palace bureaucracy. Again, there is a circularity to the argument: since the find site is a palace, the Linear finds must be palace records; we read them as palace records; therefore it is a palace. A key issue remains unresolved, namely independent evidence for and assessment of whether the "palace" sites really are palaces.

Pylos appears to provide the best evidence for a centralised palace bureaucracy, as so many tablets were found in its putative "archive" room. But a cart has got before a horse somehow. For the prior question, at Pylos and elsewhere, is, again, whether a reading of the tablets as genuine bureaucratic records is right at all, and only secondarily whether the concentration is explicable *only* in terms of a central administration. Not all aggregated "documents" (if documents they are) are archives.

The nature and number of finds at the Linear sites is inconsistent with the AM's assumption of a palace bureaucracy. Firstly, the AM never explains why it is that so many tablets are allegedly fragmentary. If the palace sack somehow smashed them to pieces, why are there, in fact, so few fragments and so many apparently incomplete tablets? Why, in other words, have some "fragments" survived and others not, even though the conditions were the same for both? As for numbers, it is difficult to obtain an accurate quantification for the various sites. A rough but generous estimate at Knossos would be 8,000, including all fragments. Allowing time off for weekends, that equates to roughly five scribes producing six tablets a day per year. As a total, it is incompatible with the AM's administrative and archival concept, particularly when the AM proposes that so many of the tablets are the briefest aide-mémoire. An assumption of (what would need to be vast quantities of) undiscovered tablets would be no more rational than the reasoning that surrounds the surviving "fragments" thesis.

## Conclusions about accounts and archives

The proposition that the Linear tablets are purely administrative is not internally cogent or coherent. It might yet be redeemed in so far as it turned out to be based, after all, on the internal evidence of the tablet texts, that is on their translation.

## Translation

The normal test of whether an individual understands an alien written language is whether he or she (a) understands the signs in which it is written and (b) whether the person can then make sense of those signs. Often the first step is redundant, but it matters, for example, if an English speaker is learning Greek or Chinese. If he or she is really struggling with making any head or tail of a text, then ultimately the question arises whether the individual properly understands the signs in which the language is written in the first place. The understanding may be broken down into sub-divisions, namely recognition of the shape of the sign and awareness of its sound value.

The AM has demonstrably failed to translate Linear B. The easiest and quickest independent check may be made by counting the number of question marks, "probables" and the like in the Mycenaean Vocabulary section of *Documents*. The AM often translates only tablet snippets, not necessarily unreasonable except that such translations are often based on the assumptions of the "combinatory method" discussed above.

Proponents of the AM are disarmingly modest. "It cannot be claimed that we are in a position to 'read' the Linear B texts (*Introduction* page 33). "Even when we can transliterate our text mechanically into syllables, we are a long way from reading it (*Documents* page 26)."

The AM logic is, again, untroubled by the admission of failure. Essentially, thanks to perceived ideograms and numbers and the fact that the Linear tablets are, in the AM view, obviously and only

administrative, name-based and so forth, we more or less know what they are about: men and women, sheep, wool, goats etc. So, if we cannot translate terms exactly or fully, it is a pity, but it does not invalidate the model, frankly doesn't matter.

As a result, there is little point in the translations. For what we are actually being invited to evaluate is not a real translation, but a succession of mainly 20th century speculations applied to a presumed topic that may in any event have been misidentified by sign or sound. Nonetheless, I shall discuss a few of the Linear B tablet translations, not only to try and give some flavour of the AM's weaknesses, but because some more generalisable points emerge from the exercise.

PY Ae 303

John Chadwick's translation reads:

"At Pylos: slaves of the priestess on account of sacred gold: 14 women (page 71 of *Decipherment*)."

He proceeds "the mere fact of being able to translate the tablet does not answer all the questions." He then lists four and adds "All these are questions which we cannot answer; the facts were known to the writer of the tablet, and he did not expect it to be read by anyone who did not have the same knowledge." His interpretation pithily summarises the AM's entire approach.

There are major issues surrounding the reading of the signs on this tablet, but I leave those aside here. John Chadwick does not ask some of the more challenging questions about the translation. For example, why not mention the quantity of gold, if this is a transaction? Why should a scribal note use prolix (h)eneka, ἐνεκα, on account of, when a genitive of price would have sufficed? It is a surprising term for "on account of", particularly at this presumed early date. Is it even rightly translated? The signs could also stand for ene(g)ka(n), ἠνεγκα(v), they have brought, i-je-re-ja, ἱερεῖα for the neuter plural, sacrifices. And why the mention of the presumed Pylos when, by default, it should have been obvious? The AM never satisfactorily explains scribal practice in that regard. Again, is the word necessarily Pylos? Another option would be pu-ro, πυρ(ο)ω, I burn (as offerings). "But it is obvious that pu-ro stands for Pylos. It appears as a name on many other texts." Both assertions are only assumptions.

*Introduction* (§ 204) concludes "The picture which emerges...is that of a guild or group of priestesses (on the assumption that the i-je-re-ja was only *prima inter pares*), who are charged with the duty of attending the sacred treasure." But aside from the fact that the conclusion only follows a lengthy paragraph of contextualisation, the picture that emerges is, to me, totally unconvincing, the language strained, and the purpose of the record still utterly opaque.

The AM, both in this and many other instances, is partial, often pursuing only one of several available translations. Whilst one option may be marginally preferable to another, ultimately they all tend towards the same vanishing point. They are not coherent or cogent, in many cases, they mean almost nothing, for all the reasons discussed above, including the lack of author and date. The AM's answer is paradoxical and unanswerable, namely to assume that the signs meant something to the original writer but hidden from us. One may make the same assumption about any language text. On that basis almost any translation of almost anything would pass muster.

PY Ta 641

Translation of the famous tripod tablet contains multiple difficulties. In line 1, a3-ke-u, often translated as a putative Αἰγεύς, Aegeus, a singular, proper noun when some kind of plural is expected grammatically, leads to an explanation based, effectively, on scribal error. The picture of a two-eared jar appears to correspond to the word for a single-eared one. Scribal error is, again, postulated. Similarly and thirdly to explain me-zo-e, a dual form in line 2 when the corresponding numeral is "1" and the surrounding words also singular. "There is no doubt about the meaning of the phrase e-me po-de: it comprises the instrumental of the numeral 'one' and the instrumental of the word for 'foot' (*Introduction* § 238)." Yet the corresponding picture shows a three-legged tripod. "The only plausible reason is that this tripod, like that mentioned [later], has been damaged and has lost two of its feet." One can only take so much special pleading. The reasoning is implausible; after all, in the later case, the loss is (according to the AM) explained by the

syllabograms, in terms, in words. Here it is not. That “e-me” corresponds to the instrumental of “one” is based on tenuous analogy between other tablets and an assumption that it represents an “old” form of the numeral.

One might proceed to criticise further elements of the proposed translation, but in doing so miss the wood for the trees. For the AM fails to address, for example:

- why an inventory should be written on a long, thin “palm leaf” tablet;
- the unnecessary repetition of terms, for “tripod”, “cup”, “larger” “smaller”, and the use of the comparatives “larger” and “smaller” when “large” or “small” would suffice (and be more natural, just as we say “large or small plates?”);
- the oddity of the items listed and (especially) depicted with their curious numbers of “ears” and “feet”;
- why, as is the case with so many tablets, its signs should be so beautifully delineated.

#### PY Jn 829

According to the AM, PY Jn 829 concerns not allocations but assessment of bronze. *Introduction* (§ 207) explains that “the prefix jo- attached to the opening word of line 1 indicates that, within the postulated group of Jn tablets dealing with assessments, our text is the first.” But what monosyllabic “prefix” can possibly exercise such power and meaning? “Some twenty Linear B inscriptions begin with proclitic o- or jo-.....*Whatever the correct explanation* [my italics], the function of o- / jo- is plain enough: it serves as an introductory particle, not only of one inscription, but sometimes of a whole set of texts (§ 130).” The function cannot be “plain” when we cannot translate the term. The AM is assigning the function because it fits its preconceptions about the meaning of the (largely untranslated) texts.

For the AM is able to translate, perhaps, the third line of the tablet, but in the first two the only real approach to meaningful translation is ka-ra-wi-po-ro-qe as κλα(φ)ιφοροι τε, “and key bearers”, and even that is a form unattested in later Greek. The rest, ko-re-te-re, du-ma-te etc, is unknown, untranslated, and what the AM offers instead is speculation as to what it thinks the signs should mean even though there are no matching Greek forms, and the putative results, such as “supervisors of figs and hoeing”, are at best implausible or, as Professor Hooker fairly states, “only to admit our ignorance of the real meaning of these terms”, or perhaps more likely indicative of a failure to identify the underlying signs fully or correctly at all. Any analysis which is unable to translate the opening three lines of a text is unlikely to offer sound insight into the meaning of the whole. The novice language student would get poor, if any marks.

#### “Spice” tablets

A number of putative “spice” tablets have been identified at Mycenae and Knossos. The AM interprets many of the signs as abbreviations, thus “ku” for cumin, but if the rationale were sound, we would expect an abbreviation “ka” for ka-na-ko, saffron, especially as the noun is apparently qualified by adjectives (red or white saffron). But we don’t get it. If ko-ri-ja-do-no is the later Greek κοριανδρον, coriander, why should a mere scribal note additionally use the \*123 sign (thus in KN Ga 415 and, according to the AM, the ideogram AROM or “condiment”) when the word would have already been unmistakable? The same \*123 sign is absent in KN Ga 418 and 424 despite the presence of the far more ambiguous po-ni-ki-jo, “Phoenician [spice]” (the AM adding “spice”). In KN Ga 675, the AM translates pe-ma as σπερμα, seed, when, in the context of herbs and spices, the signs might stand more obviously for πεμμα, condiment. How can they possibly not? The AM interpretation of tablets as spice-related is inaccurate. Some appear to itemise herbs, seeds and spices in an unconvincing mix. Were fennel and mint in any sense “rare” or even “cultivated”? They spread like wildfire perfectly naturally.

#### Grain consignments and rations at Knossos: E and F series

“Although there is no indication whether these tablets record payments or receipts (*Documents* 213).” The imprecision is startling, even in an aide-mémoire, and whilst it may be true, it is only an

assumption that they record either. The designation “Men of Luktos” etc is never explained (it apparently occurs also on the Knossos “spice” tablets). Who are they? Does it mean the people of Luktos, the community? If so, why not say so? It would still be an odd descriptor. The wheat payments (or whatever) of Luktos are wildly discrepant in E 668 and E 749.

*Documents* takes E 777 as a ration, but it is complete guesswork. Each of the legible three places (if the reading is right) “receives” the same quantity of wheat, explained by the AM on the assumption of a ration for a notional 500 women at each site. As for the (untranslated) “ki-ri-te-wi-ja-i women”, why should only that word (if it is only one) take a feminine plural in “ai” when all the others, particularly the preceding and apparently in-agreement ko-no-si-ja (of Knossos), do not?

#### Mixed rations and consignments at Pylos: PY Un series

*Documents* comments “Unfortunately in no case is the heading really intelligible (page 219). It is a common complaint of the AM, and one can only ask, as not all the tablets are damaged, “Why not?” Again, it raises the question whether the AM is itself competent to tackle the Linear texts.

In PY Un 138 the AM treats me-za-wo-ni in line 5 (NB) as a proper name governed by the preposition pa-ro in line 1. The gap is impossibly large. In line 1 pu-ro is again assumed to mean “At Pylos” but given the apparent ideogrammatic list of agricultural products, it is far from obvious that the town assumption is correct. The products are an extraordinarily unseasonal mix, a no less extraordinary medley found in PY Un 2.

#### Land ownership at Pylos: PY E series

*Documents* redefines later Greek meanings in an attempt to produce a translation of the tablets. Thus ko-to-na is taken to mean, not sub-divided deme, the meaning of the later κτοίνα, κτοίνα, but field or plot, whilst ki-ti-me-na and ke-ke-me-na are given the meanings “private” and “communal” and o-na-to “lease”. *Documents* acknowledges that these are “effective rather than etymological meanings” (page 233), but isn't that tantamount to saying the words mean what we want them to mean?

The AM does not explain what the grain references in the series signify. The tablets are not about land ownership or registration as they contain no boundaries, inadequate nominal identifiers, and evidence no real “title”. Taxation might be more plausible, were it not that the actual tax, let alone its collection and so forth, is not mentioned. John Killen offers a useful discussion on page 163ff of *Companion* volume 1, see especially note 13. Nor does the AM explain the existence (in its eyes) of two separate “recensions”, or tablet versions, within the same series. *Documents* proposes that one of these is “more polished”. Yet why would scribes produce ever more polished clay versions of text that had yet to be transferred to some final vellum or papyrus or wood registry? The clay tablets cannot be the full or permanent archive as the series covers far too small a sample of land use. Again, for an aide-mémoire, the tablets are often beautifully written.

In PY En 609, reluctance on the part of some to read the signs da-ma-te in the first line as Δαματηρ, Damater, has led proponents of the AM to invent several “derivatives” or similar of a stem in, say, \*dam, with similarly contrived translation. But the obvious question is how could any Linear B reader, including the writer, on first encountering or re-encountering those signs not read them as Δαματηρ? What possible contextual signal could he have, as this is only the third word in the text, that Δαματηρ was out of court? That is not to say that it is the only possible translation of the signs. To exclude damartes, δαμαρτες, married women, on the grounds that there is no subsequent ideogram for a woman is to assume a rule “ex post” rather than interpret the evidence. The AM does not know what the sign DA following da-ma-te means. Not for the first time, the difficulty for the AM is that at least two alternative translations seem to be available. Again, it needs to fall back on “only the scribe could know.” But it is not obvious that even he or she could.

#### Proportional tribute and ritual offerings: PY Es series and others

In the case of PY Es 650 and Es 644, the structure does not chime with a scribal note. All a scribe needed to write was a heading “they have so much seed” followed by a list of names and

quantities, but instead “they have so much seed” is apparently written out in full on every single line. The needless repetition - a common feature of the AM's reading of many tablets - is equally blatant in Es 644 with “do-so-mo we-te-i-we-te-i” and the wheat ideogram again repeated on all twelve lines. The tablet is otherwise purportedly a list of *single* names, again prompting questions about who and where exactly.

PY Un 718 (*Documents* page 280ff) is regarded as an important tablet concerning religious offerings to Poseidon, but the failure to translate the first sign cluster, sa-ra-pe-da, is rather a show-stopper, even more so when followed by o-wi-de-ta-i in line 2. “The difficulty of interpreting this word leaves in doubt the punctuation and syntax of the first two lines..... Is this a scribal formula to emphasize the transaction *has not yet actually occurred* (my italics)?” If it is, it would be utterly bizarre in the context of offerings, and “as far as one can see” is an exceedingly curious translation of administrative *actualité*. But are the signs a single word at all? According to the AM, the tablet is full of surprises. “Note the surprising repetition of the spelling [tu-ro2] in the form of a ‘monogram’ [TU+RO2].”

### The Pylos N (linen) tablets

The AM interpretation rests on the assumption that ri-no in PY Na 228 equates to the later Greek λινον, linen, and that the word is in turn taken up by the ideogram “SA”. (Compare the fig ideogram discussed above.) Yet even if the equation is correct, the inference that the ideogram means “linen” can only ever be that, as it occurs on tablets where the AM does not understand many of the other signs. The translations are often implausible. So Na 248 “Tamitana [untranslated in *Documents*] free to the huntsmen: thirty of linen.” Other tablets, such as Nn 831, list a strange mix of linen “givers” (the mayor, the shepherds, the smith). “The simplest form of assessment is shown by the tablets Na01-Na44, which merely contain a place name followed directly by an amount of SA (*Documents* page 297).” But the AM does not generally know whether the tablets are about assessment or giving or receipt as it can find no relevant word. It would be scribally redundant to make exceedingly terse “assessments” for individual places or groups on individual tablets, yet that is what has happened according to the AM.

### Conclusions on translations

Given the paucity of substantive translation, one might reasonably conclude there is not much to conclude on. In which context, it would be timely and apposite to mention the posited “Linear A”, as its fate is an even more graphic illustration of the failings of the AM.

Until Sir Arthur Evans's spadework, historians and the public, in so far as they thought about it at all, thought of pre-historic Crete as being as much Greek (or for that matter not Greek) as any other part of Greece. Mythology and poetry indicated that the “Mycenaeans” (Agamemnon et al) were Greek. With the arguable exception of Herodotus, nothing much prejudiced Minos's putative Greek credentials either. On the contrary, the island of Crete was rich in Greek myth and legend.

But Sir Arthur became convinced he had found a wholly new, non-Greek civilisation. Michael Ventris and John Chadwick were swimming against the tide in propounding the case that the Linear B tablets (discovered first at Knossos and later on mainland Greece) really were Greek after all. Their case was rapidly (perhaps in some respects too rapidly) accepted. But at the very moment that it was, a new orthodoxy arose, namely that it was the *perceived* “Linear A” tablets that now could not possibly be Greek. The new orthodoxy was based largely on the observation that if you applied the decipherment of the Linear B signs to the same Linear A signs, it resulted in a great number of sign groups with “i” and “u” vowels, and it was impossible (as it was thought) to reconcile the pattern with Greek.

The conclusion, such as it was, was ironic, as until then a natural assumption had been that, since Linear A and Linear B objects shared many of the same signs and other features, it was reasonable to infer that they were related languages. Ruling out perceived Linear A as Greek should have sent scholars back to the decipherment of Linear B time after time to see if it was as right and complete as they thought it was, and for that matter whether the reading of perceived Linear A and Linear B signs right and complete too. For the plain fact is that in dismissing the

Greek credentials of (still perceived) Linear A they were ruling out by far and away the strongest candidate language. Nothing else comes remotely close.

The new orthodoxy maintained a polite pretense, expressed to this day in various degrees and forms of "scepticism" or "agnosticism" not matched by any serious re-evaluations. The reason for its strength or obstinacy was obvious and natural. If the AM was entirely right in its decipherment and transliteration, then Linear A could not possibly be Greek, and on the other hand if Linear A was Greek after all, then, again, the decipherment and the AM were highly likely to be significantly wrong. It was even, no doubt subconsciously, convenient to posit a "non-Greek" Linear A construct, as it helped explain perceived "non-Greek" names or other elements of Linear B.

And so the orthodoxy took hold and has continued ever since. Thanks to the prior block created by the decipherment of Linear B, or the Administrative Model, the Linear A texts remain to this day untranslated. That is to say they are and are likely to remain untranslated in their entirety, just so long as the AM endures. None of which has stopped researchers from assuming that the Linear A texts follow the same pattern as Linear B. They have assumed that all those on clay tablets must be economic or administrative in nature. They have even altered the tablets' own line breaks to produce versions that better reflect the assumed reality. But I repeat. As they cannot properly understand one single word of what is written, the analysis and the treatment are without basis.

"The tablets contain signs for commodities and signs for numbers, so obviously they are about the things we say they are."

There is no point going round the roundabout again. I have just demonstrated in relation to Linear B that such observations and arguments are fundamentally and consistently flawed. The same is true in relation to Linear A and for that matter the so-called Cretan hieroglyphics or pictographic script as well.

The effects of the AM have thus been totally perverse. Premature defence of its limited success in identifying its transliterations with ancient Greek has totally obstructed translation in the case of Linear A and obfuscated it, almost irreparably, in the case of Linear B. Yet for all that, two important facts do emerge from the translations of Linear B, however inadequate they may be. For

- even if the AM's reading of the Linear texts were corrected (signs fully and correctly identified, sound equivalences and syntax likewise), the texts would largely still remain administratively vacuous (because they do not incorporate the requisite data to be anything else) and probably ambiguous (because ambiguity, despite the artificial efforts of the AM to curtail it, will always be an inescapable consequence of the way the Linear script works); but

- the texts, *as currently interpreted*, are not all qualitatively the same. Some may be entirely speculative, non-provable, non-falsifiable, but the tripod tablet in which deciphered signs ti-ri-po correspond to a tripod *picture* provides strong visual evidence, proof in any normal sense of the word, that the decipherment of Linear B and the translation itself must, in that case, be partly right.

#### Overall conclusions on the Administrative Model

Some general conclusions on the AM's approach to Linear B might be in order. For example that:

- its physical identification of ideograms and other signs is visibly and consistently flawed;
- the lines of its "sound and spelling rules" are drawn arbitrarily, and the "rules" are accompanied by a host of exceptions, scribal errors, unknowns, and incongruities;
- its "administrative presumption" is only that, and almost completely non-functional in practice;
- resulting translation, such as it is, is impoverished, again arbitrarily pursuing one line, whilst failing to explore the implications of others;
- overall, the Linear B texts emerge from the AM as an unconvincing corpus of "administrative documentation" that, inevitably given its premises, tells us nothing we could not already *assume*, namely that these people harvested grain, kept sheep, and were organised.

The harshest critics might go further, to say that the AM is totally flawed, imploding under its own vacuity, Linear B not even deciphered. I disagree. Much isn't right or is incomplete, but I think the AM's identification of *standard* syllabograms and sounds is largely correct. In addition, it may transpire that some of the Linear objects are texts involving numbers. But if they are, they are unlikely to coincide in all particulars with the signs misidentified by the AM, or form part of a real administrative archive. I discuss a possible alternative context in *The Problem with Linear B*.