

47 Highpoint,
North Hill,
Highgate,
LONDON, N6.

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Dear Bennett,

Thank you very much for your letter, for your notes on the Mycenaean tablets, and for your comments on Pylos 641. I'm glad several of the Mycenaean tablets have joined: I've only had photos of a few of them, and not the ones that do: so I look forward to seeing the book. I wonder if Wace will turn up any new ones?

Congratulations on your Fulbright move to Greece, which should be very pleasant for you and useful to all of us. (This is one of those cases where our kinds of English don't quite agree - does "next fall" mean "this autumn (1953)" or next year?) I'm thinking of going to Greece next summer to do some architect work on a dig, possibly with Sinclair Hood at Knossos. John Chadwick is also thinking of going out then, so there will be a chance of meeting up. I'm struggling with a bit of Modern Greek homework.

Chadwick and I have been giving a few lectures between us in Oxford and Cambridge, and I am due to give one tomorrow to the Hellenic Society / BSA here in London. 641 does at least make the work more plausible, because for one thing its last phrases have such an obvious context that they can't possibly mean anything else - which you can't really say about any other Linear B sign-group. But I'm only too conscious that Linear B is only $\frac{1}{4}$ - deciphered, at best; or that, if technically 'deciphered', we still can't read the tablets extensively - which from the layman's point of view isn't much better.

I get some encouragement from a few analogous situations:

1) Pedersen, Lykisch und Hittitisch. p4:

"... Über die sprachliche Zugehörigkeit hat man sich aber am frühesten beim Phrygischen geeinigt. Auf Grund der klaren Schlussformel der neuphrygischen Grabinschriften musste diese Sprache als indoeuropäisch anerkannt werden. Dass wir von den altphrygischen Inschriften verzweifelt wenig verstehen, bildet keinen Einwand dagegen; es ist töricht anzunehmen, ein Text in indoeuropäischer Sprache müsse schon allein durch Sprachvergleichung uns verständlich sein; entfernte Sprachvergleichung ist für die Entzifferung in Wirklichkeit gut wie nutzlos..."

2) Erman, die Hieroglyphen, p 47:

Als Champollion die Hieroglyphen entziffert hatte, nahmen die Aussenstehenden an, dass nun nicht mehr das volle Verständnis der ägyptischen Inschriften hinderte. Man verstand die Schrift zu lesen und was die Sprache anlangte, so hatte man ja im Koptischen, der Sprache der christlichen Ägypter, ein Hilfsmittel, mit dem es ein leichtes sein musste, den Wortschatz und den grammatischen Bau zu ergründen. So dachte man, aber nur zu bald zeigte sich, dass diese Siegesgewissheit voreilig war und dass gerade auf der sprachlichen Seite noch unendliche Schwierigkeiten einem wirklichen Verständnis der Inschriften entgegenstanden"

3) Gelb, The contribution of the new Cilician bilinguals ... p 139:

... The progress achieved in the decipherment of hieroglyphic Hittite was strongest in the field of writing and grammar, and weakest in the field of vocabulary. This is the situation which affects not only relatively little-known languages such as H-H, but also such well-known areas as Mesopotamian cuneiform or Semitic languages in general. In Akkadian, e.g., while the writing is well-known and the grammar offers few obstacles, our greatest difficulties remain in the field of vocabulary."

It will be very valuable if someone like yourself can start from whatever things do look plausible (641, perhaps) and work back, treating the precarious phonetic values with the greatest brutality, and being ready to revise our ideas on the exact spelling rules and on the precise relationships of the language.

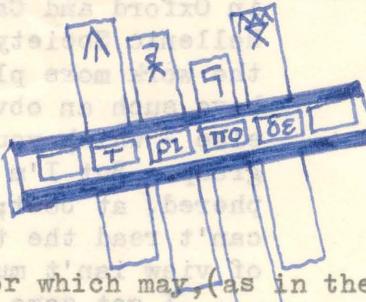
The theory that "non-syllabic" consonants can be omitted from the spelling does make interpretation difficult (and often unconvincing); but ti-ri-po(S) / ti-ri-po-de seems a reasonable demonstration at least as regards final -s. In Cypriote po, for example, can theoretically represent at least 15 different Greek syllables:-

πτο	βο	φο
πτον-	βον-	φον-
πτομ-	βομ-	φομ-
πτω	βω	φω
πτ-	β-	φ-

In Linear B we might be faced with at least 42 alternatives:

πτο	πτω	βο	βω	φο	φω
πτοι	πτων	βοι	βων	φοι	φων
πτολ	πτων	βολ	βων	φολ	φων
πτομ	πτωρ	βομ	βωρ	φομ	φωρ
πτον	πτως	βον	βως	φον	φως
πτορ	πτω	βορ	βω	φορ	φω
πτοσ	πτ-	βοσ	β-	φοσ	φ-

to each of which an initial s- might be prefixed. I have made a toy out of card-board (which is perhaps most useful in explaining the problem to the outsider) consisting of a box with little windows through which you can slide strips containing all the possible spelling permutations of a given syllable.



But one is loth to spend much time at this stage in worrying recalcitrant sign-groups to death, when one knows that the new unpublished Pylos tablets must contain so much which will cause one to revise one's ideas radically again, or which may (as in the case of "#") suddenly provide the necessary context for a word on which no further progress had seemed possible.

Your going to Greece will not, I suppose, hold up the editing of the new (or brand-new) Pylos tablets : perhaps the reverse ? I imagine, too, that however much the new ones help decipherment, it will be best to continue the tradition that the editio princeps presents the bare texts, without any attempts at interpretation which may soon become out-of-date.

But this brings me to the question of further work and its publication. If, say in about a year's time, the study of the new tablets has enabled a decipherment to be put on a firmer basis and agreed amongst us (and perhaps if there is a lull in digging up new ones) we shall have to think of writing a general book on the "The Mycenaean Tablets". It would not, I hope, be as massive as Scripta Minoa, and needn't contain facsimiles of the tablets published elsewhere. The use of special type might be confined to unidentifiable ideograms and syllabic signs. But it would contain, in addition to the necessary archaeological commentary :

1) Discussion of the signary, hands and variants. Textual criticism.

2) Discussion of the ideograms, including formulae and quotation of typical tablets in each class.

3) Social and historical evidence to be drawn from the contexts and content of the tablets.

4) Glossary of such vocabulary sign-groups as can be interpreted.

5) Proper names, with discussion of such extraneous elements as may appear.

6) The language.

7) Relationship to Cypriot, and to Linear A (doubtful section ?). On this last question, Furumark is about to publish a mimeographed report. I can't imagine quite how he will treat it.

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In addition to you and me, the following are at present actively engaged in juggling with the Linear B phonetic values or in looking at the language from the viewpoint of comparative philology:-

John Chadwick at Cambridge,

Prof. L.R. Palmer, 1 Canterbury Square, Oxford
(with undergraduate George Huxley, Magdalen College)

Arne Furumark, S:t Johannesgatan 24, Uppsala
(with Gudmund Björck)

Ernst Sittig at Tübingen.

Myres seems to look at our work encouragingly, but would probably be the last person to suggest an active collaboration with us for the future.

Having had the first draft of phonetic values from me, I don't think any of them are in a position, or likely (except perhaps Furumark) to rush into print with a wholesale book on the tablets. While there's no reason why any of us shouldn't write articles about whatever aspect of the problem we like, or on which we have made a contribution, I think there's a lot to be said for writing a joint book (or at least an agreed summary of joint results) (minority views included), provided that no irreducible differences of opinion have meanwhile arisen.

If you think that this is a possible idea, I would be very glad to have your comments on the way in which one might begin to prepare the ground in advance; and I will discuss it with the others.

Even if a team of more than 3 proves impracticable, I think that Chadwick and I would find it very difficult to write anything ourselves without your collaboration on problems of text, ideograms etc.

At the present moment the most useful form of preparation seems to be the circulation, for comment, of contributions to the up-to-date state of the

- 1) glossary, including personal names
- 2) syllabic grid
- 3) meaning of the ideograms

(on the signary and the text, the ball seems entirely in your court).

The most thorny question is to what extent everyone might want ~~their~~ their own identifications logged up to them, and how this invidious scoring, if required, should be filed, and presented in published form. To some extent a division of labour could be effected (and a charge of exploitation avoided) if sections of the book were put in final form by different people.

I'm getting ready to move into our new house (I'll send you a note of the new address when it happens), and this is taking a fair amount of time.

One thing I may do, while waiting for more tablets, is to look more closely at the ideograms (on which I am sure you have many ideas of your own), both from the point of view of the archaeological evidence, and also the analogy of contemporary tablets, particularly the Nuzi ones (general discussion in Starr, NUZI vol. I, pp 528-544, "Epigraphic evidences of material culture"). There's a close parallel to the upper lines of the Knossos CHARIOT tablets, apparently lists of obscure objects with the materials from which they are made, in the similar descriptions of charioteers' equipment at Nuzi (where a lot of the incomprehensible words are Hurrian terms); in the reference to parts of the equipment which are missing (cf ?); and in the counting of articles of chariot equipment in pairs, istennutu (if Furumark and I are right in suggesting that ἴ may mean "pair" (βεῦγος ?) and σι "single" (μόνφος ?)).

I also enclose the drawing of one of our slides. The comparison of frequencies are really not properly comparable, but it seemed necessary to make some use of the statistical material.

With best wishes,

Michael Ventris