

47 Highpoint,  
North Hill,  
Highgate,  
LONDON, N.6.

27.ix.52

Dear Bennett,

I'm enclosing a copy (to keep) of the first draft of an article which John Chadwick and I are trying to get into JHS for next year. To some extent it's very experimental, and our convictions about the conclusions expressed will obviously be altered by the new Mycenae tablets, which I haven't seen yet, and by your own reactions to the suggested line of attack. But it's very difficult to keep waiting for one's own mind to change, especially with the delays of proper publication. So really my *as it stands?* first and main question is: do you think it's worth publishing? And if by chance our ideas meet here and there, do let me know where I've demonstrably taken the wrong turning.

I've sent a copy to Chadwick for comment and for a check on the Greek philology and for the closing section; one to Myres; and one to Furumark, who's lecturing on Aegean scripts at Uppsala in the spring and wants to get all the views. Incidentally, he has accepted the Vocabulary in a big way, and has, he tells me in a long letter, found a lot of "historical evidence" of his own in the tablets beyond what can be read into the Vocabulary. He wants to include them in ~~the~~ various archaeological papers he's writing (in the main stream of his "Mycenaean Pottery" and "Settlement at Ialysos"). I told him to go ahead, but to make it clear that we think the whole decipherment is very shaky, and to take responsibility for anything he reads into the texts that we don't.

Myres, I gather from Chadwick, who has seen him recently, thinks that we are "at least on the right lines", from which I gather he means that he too now feels the Pylos-Knossos language is Indo-European. But in letters to me during the last couple of weeks he has suggested that the time was ripe to bring out again some of the ideas he dealt with in "Who were the Greeks?", notably on the subject of the Pelasgians. He says: "If anyone puts up a claim for Pelasgian = Minoan, I should receive it with cautious concurrence", probably having Georgiev and Van Windeken's Pelasgian in mind. The trouble is that, though Pelasgian is a useful excuse for words which one can't read as Greek, the words that apparently do make sense conform to Greek and contradict the very features which Pelasgian is said to typify.

Myres says he is "at the moment beset with the proofs of SM 3": what on earth is it going to contain? Do you want me to suggest helping him with the proofs?

Yours,

Michael Ventris

Letter to MGFV 25 Oct 1952



47 Highpoint,  
North Hill,  
Highgate,  
LONDON, N.E.

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I've sent a copy to Chadwick for comment and for a check on the Greek philology and for the closing section; one to Myers; and one to Turamark, who's lecturing on Aegean scripts at Uppsala in the spring and wants to get all the views. Incidentally, he has accepted the Vocabulary in a big way, and has, he tells me in a long letter, found a lot of "historical evidence" of his own in the tablets beyond what can be read into the Vocabulary. He wants to include them in the various archaeological papers he's writing (in the main stream of his "Mycenaean Poetry" and "Settlement at Ialyssa"). I told him to go ahead, but to make it clear that we think the whole description is very shaky, and to take responsibility for anything he reads into the texts that we don't.

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Michael Ventris