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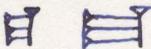
November 25, 1941

Dear Miss Kober,

Just a few words in connection with your welcome letter: I am busily engaged in Mycenaean chronology for my Hartford paper, and can't take time to run all the points to ground.

vowels Would the problem still exist if one assumed that the Minoan scripts possessed signs for closed syllables as well as the open ones which came down into the Cypriote syllabary? There are enough unidentified signs to account for a closed series of the vowels and consonants for which there is Cypriote evidence. And syllabic signs may have been used to indicate only one of their phonetic elements, as in Hittite, and I believe generally in Cuneiform. If there were signs for closed syllables, there would be no difficulty in expressing contiguous vowels.

se-ke I think the Cl.C. history of the ke sign would be one of the chief beneficiaries of a chronological analysis of Cl.C. I am of the impression (but it is no more than that) that the 4 form is usual in obviously early inscriptions, but I would like to know more of the antecedents of the Golgoi forms. I will be glad to hear what the phonologists think of the problem.

ku I am glad that we have reached at least partial agreement on this question. You are probably right about A25 and B50 being the same sign. I was not altogether happy about identifying A25 with S45. I was particularly interested in your saying that A25 sometimes has two extra strokes in the upper register. On the related question of the inclusion of the sign for ma in this group, I am fascinated in spite of myself by the fact that the cuneiform characters for ku and ma are almost identical. Of course it is almost certainly pure coincidence. 

CM 37 I suppose that only additional material from Cyprus will really solve this problem. The presence of B21 on false-necked jars in the Knossos tablets is certainly suggestive. I presume that it indicates the nature of the contents, *unless Evans is right, which I doubt on general principles.*

Another wild idea of mine (I have great numbers of them, but usually manage to keep them to myself): You speak of the fact that Linear B must have existed prior to its appearance at Knossos. Do you think that there is any possibility of its having existed on the Greek mainland, in spite of our present lack of evidence? The *Lin. B.* script is certainly close to the Helladic. I have long wanted to get to work on one of my pet ideas, that there were Helladic dynasts at Knossos in the L.M.II period. There are several scraps of evidence, but the most important seem to me the script and the "Palace style" of pottery, both of which appear only at Knossos in Crete, but (assuming for the moment a close relation between *Lin. B.* and the Helladic script), ~~both~~ are widespread on the mainland. Of course the lack of early examples of the Helladic script is a serious difficulty, but a comparison of our material before and after Pylos should warn us against excluding the possibility merely on the present lack of direct evidence.

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I look forward to seeing what progress you make with the ideograms, and will be glad to do whatever I can with the Cypriote aspects of the problem. I am also very anxious to see your paper on the suffixes. We must try to get Blegen and his co-workers into a discussion at Hartford, and see what common ground we can find. Blegen himself, as you probably know, is very reticent, and hates to suggest anything until he is absolutely certain of it. A young man, however, whose name I can't recall, who is working with Blegen on the tablets, is much more prone to the discussion of half-formulated ideas; we may be able to get something from him. Dinsmoor said that one of them is going to discuss the Pylos material at Hartford.

I must close my letter with this, and get back to the chronology of floral patterns on L.H.III pottery. Thank you again for writing, and do it whenever you have any questions or anything which you wish to pass on. It is very welcome.

Yours truly,

J. F. Daniel