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EMMETT L. BENNETT, JR., MICHAEL G. F. VENTRIS,
ALICE E. KOBER, CRYPTANALYSIS, DECIPHERMENT
AND THE PHAISTOS DISC

Thomas G. Palaima

The process of deciphering the Linear B script and working on decipherments of Linear A, Cretan Hieroglyphic and even the Phaistos Disc has never been taken up systematically in a paper at the official Mycenological colloquia sponsored by CIPEM. The main accounts of the decipherment of Linear B by John Chadwick, Elizabeth Barber, Maurice Pope, Andrew Robinson and most recently Margalit Fox do a good job, when aggregated, of examining the interactions of the personalities involved and of tracing the steps that led to the eventual decipherment.¹

Still, Chadwick's account published in 1958 was written without using Michael Ventris's own final words on the subject. Ventris gave his most thorough succinct account in a paper delivered August 27, 1954 at the Second International Congress of Classical Studies published posthumously in Copenhagen in 1958. In this paper Ventris honestly, rather than generously, gives credit to Kober for showing the way.² For example, Ventris notes:³

“The first systematic programme of analysis and research of the Linear B documents, purposely stopping short of the attempt to substitute actual sounds and words for the symbols, was undertaken by Alice Kober of Brooklyn in a series of fundamental articles published between 1943 and her premature death in 1950.”

Ventris proceeds to devote two full pages to Kober's meticulous demonstration of grammatical inflection in the Linear B texts and its implications. He even implicitly credits her with the first use of something that is widely

¹ Chadwick 1958; Barber 1974; Pope 1975, 146-179; Pope 1989; Robinson 2002; Fox 2013.

² Ventris 1958, especially 72-76.

³ Ventris 1958, 72

associated with Ventris's genius, the Grid.⁴ He particularly stresses:⁵

"This [Kober's] method of attack precludes random attempts to give a premature vocalization to isolated words (since every assumed value automatically leads to a 'chain reaction' among those which are grouped with it on the same vertical and horizontal columns of the Grid), and effectively disproves any such arbitrary attempts by others."

Chadwick's account, itself published in 1958 and undoubtedly written in the preceding years, appeared at a time when the decipherment was under attack by classically trained scholars who themselves had no training in, and therefore insufficient understanding of, how pre-alphabetic writing systems functioned. They criticized the workings of the Linear B script and some of what from our modern point of view look like ambiguities in the script's functioning as 'visible speech'. But they did not realize or acknowledge that many of the same criticisms, *mutatis mutandis*, could be directed at the historical Cypriote Syllabary or any open-syllabic script being used to represent a language with closed syllables.⁶ Cypriote Syllabic was used broadly, unproblematically and publicly in its culture for at least 500 years to write Greek and to represent the still unidentified non-Greek language indigenous to Cyprus known therefore conventionally as Eteocypriote.⁷ Some of these scholarly dilettantes took a perverse joy in demonstrations intended to 'disprove' Ventris's decipherment.⁸ But their demonstrations were only apparent and only satisfied the standards of proof of uninformed scholars who wrongly viewed the results reached by Ventris, especially the spelling rules, as so multivalent as to defy proof.

See, for example, Young's remarks in his fullest critique of the Ventris decipherment. Young launches off from Saul Levin's serious attempt to argue the thesis that the texts in Linear B "contain at least one non-Greek language [along] with non-Greek vocabulary and non-Greek structural features."⁹ Young explains the 'multivalency' of the Linear B script according to Ventris's decipherment not as an economical feature of the way it operates as a syllabary, but as a false construction—charitably put, a self-delusion by Ventris—that permitted an impossibly elaborate illusion of decipherment to be conjured up and sustained by Ventris and his acolytes.¹⁰

⁴ Ventris 1958, 73: "This result [evidence for inflections] can be tabulated in a diagrammatic form labelled by Kober 'the beginning of a tentative phonetic pattern', and known familiarly as 'the Grid'."

⁵ Ventris 1958, 73.

⁶ Palaima and Sikkenga 1999, esp. 599-602.

⁷ Palaima 1991.

⁸ Young 1961; 1965; 1967; 1969.

⁹ Levin 1964; Young 1965, 512.

¹⁰ Young 1965, 514.

“It has become a kind of parlour-game to exploit the spelling rules devised by Ventris. These Ventrisian rules enable bits of a curious sort of Greek to be got out of Lin B [*sic*] texts; but experiments have shown that bits of English or Latin or other tongues, when spelt out in syllables according to the Ventrisian system, are capable often of yielding bits of Greek just as plausible as anything in the Ventris-Chadwick Documents volume. One eminent Oxonian, dining at a high table, amused himself by taking the names of the Fellows of the College present and turning them into Ventrisian syllables, from which he made a new translation of them into Greek, in which they all turned out to be Greek gods. For some British Hellenists this game has replaced the crossword-puzzle as a pastime for journeys.”

Notice that Young does not say how many fellows there were at high table, what College it was, or what their names were. If there were even a half dozen present, what *were* their modern names that would convert into: **ze-u, di-wo, di-we, *di-wa, e-ra, po-se-da-o, po-se-da-o-no, po-se-da-o-ne, *po-se-da-o-na, *a-te-mi, a-te-mi-to, a-te-mi-te, e-ma-ha, a-re, *a-pa-i-to, a-ta-na* and so on? The story is clearly apocryphal. Young cannot resist heating up his anecdote, as one does when substituting satirical rhetoric for reasoned thought, with his universalizing claim that *all* the fellows’s names once converted into syllabic representation could be interpreted as Greek theonyms. Notice that Young’s own names converted into Linear B à la Ventris: **do-u-ka-ra *jo-u-ka* do not yield any Greek theonyms. Nor do the names of other decipherment critics or skeptics: Arthur James Beattie, Ernst Grumach, James Hooker, Sinclair Hood, Saul Levin.

One might as well question the use of the Roman alphabet to represent English because we can use phonetic values represented by letters, individually or in combinations, in certain specific environments to make the sequence of Roman letters /ghoti/ spell ‘fish’ [fiʃ] by assigning the values to those same letters in environments where they never otherwise appear. Indeed /gh/ is pronounced as [f] in ‘tough’. But ‘gh’ nowhere in English represents that value of [f] in initial position in a word. Likewise /ti/ does have the value of /sh/ or [ʃ] as in ‘malediction’, when preceding /on/ at the end of many, many English action nouns: e.g., the words ‘animation’, ‘inspiration’, and ‘action’ itself. But /ti/ never has the value of /sh/ or [ʃ] in isolation at the end of a word. Thus George Bernard Shaw’s parlor trick is revealed for what it is: a non-probative amusement. Yet it is out in the open and available to anyone and everyone conversant with how the English language is represented conventionally in English texts. Young, as a non-specialist, was dealing with a recondite subject that he did not attempt to master. His line of reasoning that Linear B spelling

rules were part of a parlor game now is seen to be as onanistic as it did to me when I first encountered his logic here in the decade after he published his ideas. But his ideas unfortunately are redolent of the scholarly atmosphere of the times.

Some of these attacks bordered on what we would call ‘vicious’, i.e., not in the spirit of Gif, and all the more so because the true object of them, a figure revered by scholars who knew him well, Michael Ventris, was dead and gone. *Nil nisi bonum de mortuis* was itself moribund. In such a climate, the account of Chadwick, Ventris’s closest and most necessary post-decipherment collaborator—remember that Chadwick had not been part of the group-working team to whom Ventris had sent his Work Notes¹¹—erred on the side of creating a ‘myth of inevitability’. According to Chadwick’s explanatory tale, Ventris’s instinctive genius devised methods that were so logical and systematic from the start that eventual sound results were assured. Little could be farther from the truth.

Among other accounts, Barber’s is the most analytical and scientific; Pope’s the easiest for generalists to place in the context of other scripts; Robinson’s masterfully biographical; and Fox’s is a corrective step, using the PASP archives, at acknowledging the magnitude of Kober’s role.

Here I focus on the importance of the Phaistos disc, personal name structures, and Emmett L. Bennett, Jr.’s understanding of Ventris’s ‘Pelagian solution’ to see the work that went into creating our field. The process by which the Linear B texts were deciphered can be used as a model for how work in our field should proceed: by cooperatively identifying problems, proposing hypotheses, collecting, systematizing and openly sharing data and then testing and if necessary correcting both theoretical proposals and available evidence in order to move collectively toward sound solutions.

As I have mentioned, Michael Ventris delivered a paper in Copenhagen on August 27, 1954. In this paper, Ventris was explaining not ‘the Ventris decipherment’, but the process of discovery of how the Linear B script worked. That process, as Ventris made clear, lasted over a half century and was conducted at first by a succession of scholars and eventually, and then rather suddenly, by a small group of scholars.¹² In his paper, Ventris honestly distributes credit for advances that led to the decipherment. The publication of Ventris’s paper took until 1958, so it is not cited in *Documents in Mycenaean Greek* volume 1 (1956), in Chadwick’s *The Decipherment of Linear B* (1958), or in later treatments of the decipherment. I made sure, however, that Margalit Fox used it during her

¹¹ Ventris 1988.

¹² On the importance of group working for Ventris, intellectually and personally, see Palaima 1993, 24–26, and Robinson 2002, 50–51.

work with Kober's papers, notes and notebooks and other materials in the PASP archives that form the basis for her account.¹³

Two years and ten days after he gave his paper, Michael Ventris departed from this world in the very early hours of September 6, 1956, in an auto-lorry accident.¹⁴ The postmark on this postcard (Fig. 1) that John Chadwick sent Emmett Bennett six years afterwards I have long kept above my office desk as an ironic *memento mori*.

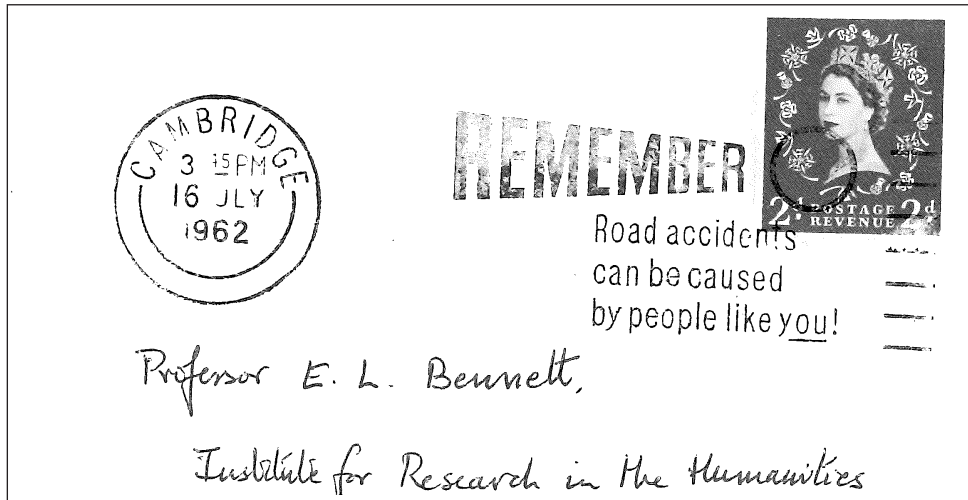


Fig.1. Postmark on postcard sent by John Chadwick to Emmett L. Bennett Jr. July 16, 1962. Palaima. PASP Archives

The Phaistos Disk has long been a taboo topic at Mycenological colloquia, despite the fact that the founding fathers and the founding mother of our field took the disk and the signs stamped on it into account when they were working toward understanding the writing systems of Minoan Crete and Mycenaean Greece. Alice E. Kober (b. December 23, 1906; d. May 16, 1950), Emmett L. Bennett, Jr. (b. July 12, 1918; d. December 15, 2011) and Michael Ventris (b. July 12, 1922; d. September 6, 1956) were the principals in bringing about the decipherment of Linear B in 1952. Here we will look at their work in the 1940's for insight into their methods.

From 1935 until her death at age 53 on May 16, 1950, Kober (Fig. 2) kept up a scientific attack upon Linear B and the language or languages that the inscriptions in Linear B might represent. She was, at the time of her death, the scholar with the fullest mastery of the data and the best practitioner of method.

¹³ Fox 2013, 263-264.

¹⁴ Fox 2013, 257-262; Robinson 2002, 147-152.

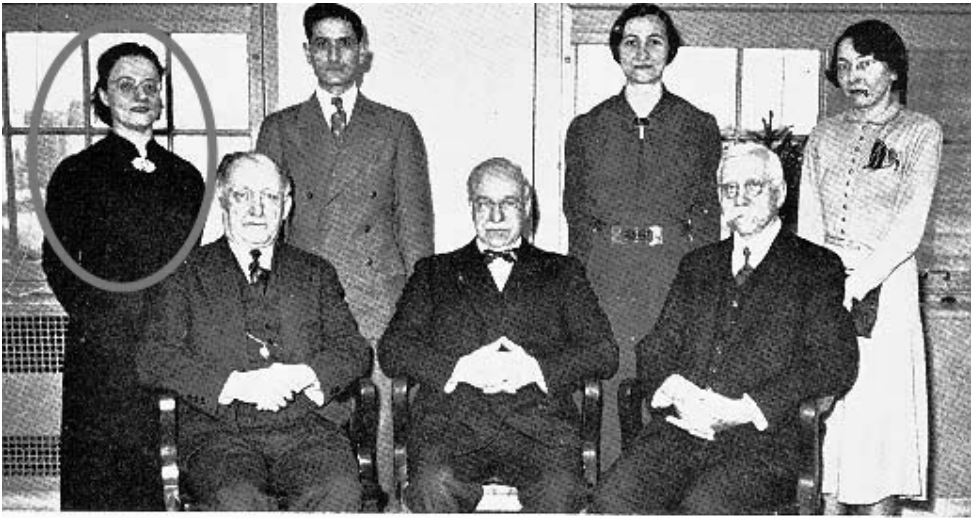


Fig. 2. Alice E. Kober in 1938 at far left (circled) with Brooklyn College faculty. Palaima. PASP Archives.

Take note of three of her accomplishments and advantages:

- (1) superb training in Greek, Latin, Etruscan, Indo-European, Anatolian, Near and Middle Eastern and even Asian languages and scripts;
- (2) a comprehensive knowledge of the available data: these she had analyzed carefully according to signs and their positional frequencies within sign groups and within the overall corpus and the evidence they offered for morphological variations and inflection;
- (3) knowledge of the historical picture of languages in the Aegean from 1900 to 300 BCE including Aegean-basin words ending in the suffix *-nthos*.

Kober not only identified what would come to be seen as inflectional patterns, but she also organized the inscriptions into thematic groups, editing the Knossos texts for *Scripta Minoa* II and editing and re-editing for Sir John Myres the bulk of *Scripta Minoa* II itself. She separated out the standard signs of the signary, analyzing positional frequencies of signs within word groups. Lastly, as we see here and as we discussed above, she used ‘the Grid’ (Figs. 3 and 4). It was not Ventris’s invention, but a natural way of arranging and putting on display the variety of options for representing consonant + vowel combinations in an open syllabic writings system. Sometime before her death in May, 1950 Kober had placed about twenty signs correctly into her grid without hazarding, at least publicly, guesses as to values.

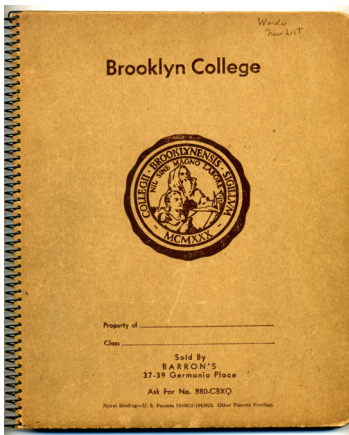


Fig. 3. Kober spiral notebook cover.
Words New List.
Palaima.
PASP Archives.

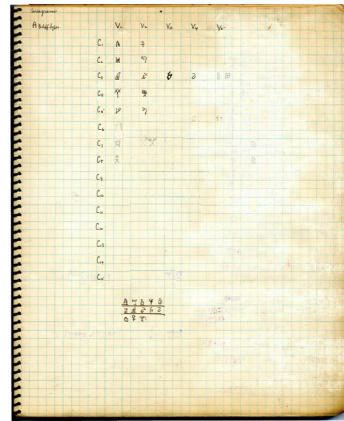


Fig. 4. Kober provisional placement of Linear B signs into a Grid inside spiral notebook. No precise date.
Palaima. PASP Archives.

When Kober died, Bennett (Fig. 5) succeeded her in helping Sir John Myres make the publication of texts in *Scripta Minoa* II as accurate as possible. Between 1947 and 1950, Kober and Bennett were in close contact. They exchanged data: Kober gave Bennett the 1800 inscriptions from Knossos, and Bennett gave Kober the 636 fuller tablets from Pylos. Bennett's study of Minoan fractions¹⁵ gave Ventris encouragement to continue his work on the linear scripts. Bennett's identification of variants of the individual signs of the Linear B signary removed 'noise' from the data.¹⁶ Bennett had worked on first-stage analysis of Japanese encoded messages during World War II. So he was familiar firsthand with decipherment techniques as neither Kober nor Ventris was.

Ventris prevailed. He applied techniques of analysis he learned from Bennett and Kober with intuition rather than analytical genius. His real strength, as more was learned about the data in the period between mid-May 1950 and his announcement of his tentative decipherment to Bennett and Myres on June 18, 1952 (Fig. 6), was his periodic testing of the material while not worrying if these tests brought into question hypotheses he had about the language principally represented in the Linear B inscriptions.¹⁷ During the period of his work notes, carefully edited for us by Anna Sacconi, right up until the final months, Ventris was forcing his idea of Etruscan-Pelasgian values upon the Linear B data. Ventris had a gift for learning modern languages quickly, but he had only four

¹⁵ Bennett 1950.

¹⁶ Bennett 1947.

¹⁷ Palaima 1993.



Fig. 5. Bennett and Ventris (circled) among participants at Colloquium at Gif-sur-Yvette. Palaima. PASP Archives. After *Études Mycéniennes* frontispiece photograph.

years of ancient Greek at Stowe School.¹⁸ This explains the few inaccuracies in the grid he sent to Bennett in June, 1952 (Fig. 7).

Of chief interest here are the viewpoints Kober, Ventris and Bennett had or did not have on personal name structures, on a Sumerian or Etruscan or ‘Pelagian solution’, and on the Phaistos Disc during the seminal period of the study of Aegean scripts and the very birth of Mycenaeanology.

In her 1948 *AJA* article “Minoan Scripts: Fact and Theory,” Kober states point blank that “[f]or many years after its discovery, the Phaistos Disk was a favorite subject for articles and was ‘translated’ several times. As a matter of fact, very little can be done with it at present.” It was in her view “of Cretan origin until proved otherwise.”¹⁹

¹⁸ Ventris also had learned some Greek and Latin at preparatory schools to which he was sent at age nine. Robinson 2002, 18.

¹⁹ Kober 1948, 87.

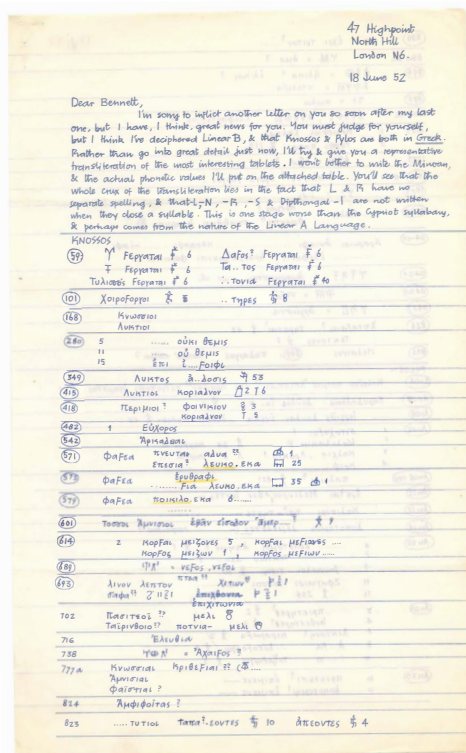


Fig. 6. Ventris letter to Bennett 061852 announcing decipherment. Page 1. Palaima. PASP Archives.

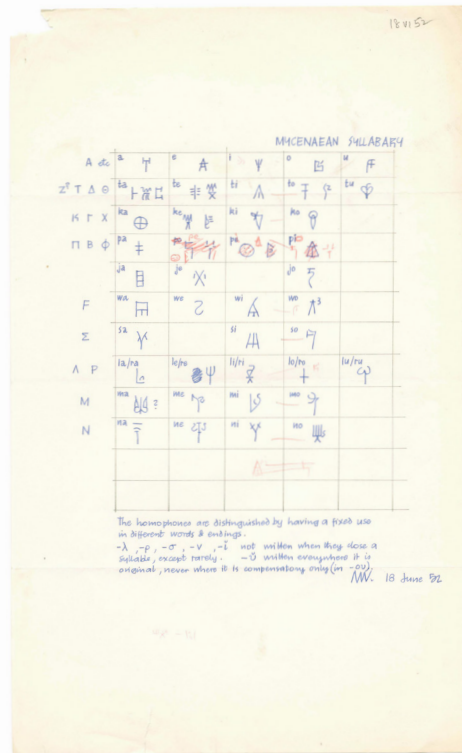


Fig. 7. Ventris letter to Bennett 061852 announcing decipherment. Page 5 showing Grid and Bennett's corrections in red. Palaima. PASP Archives.

Kober was careful in her use of language and citations and her critical judgment. Nowhere in her entire survey of fact and theory does she mention Michael Ventris's first article in the 1940 *American Journal of Archaeology*, a truly misguided and unmethodical youthful excursion into a Pelasgian solution for Linear B and the Minoan language.²⁰

Kober states that articles on the Phaistos Disc 'translated' it. This is different from 'deciphering' it. Kober here anticipates the tenet of Elizabeth Barber 26 years later: "Not only is there not enough statistical information [from the Phaistos disc] for anyone who claims to have deciphered a script [with only 241 attested nonalphabetic signs] to prove his claim, but there is not enough for anyone else to disprove it."²¹ What Barber is saying here is this: every proposed decipherment of the Phaistos Disc necessarily fails to pass the

²⁰ Ventris 1940

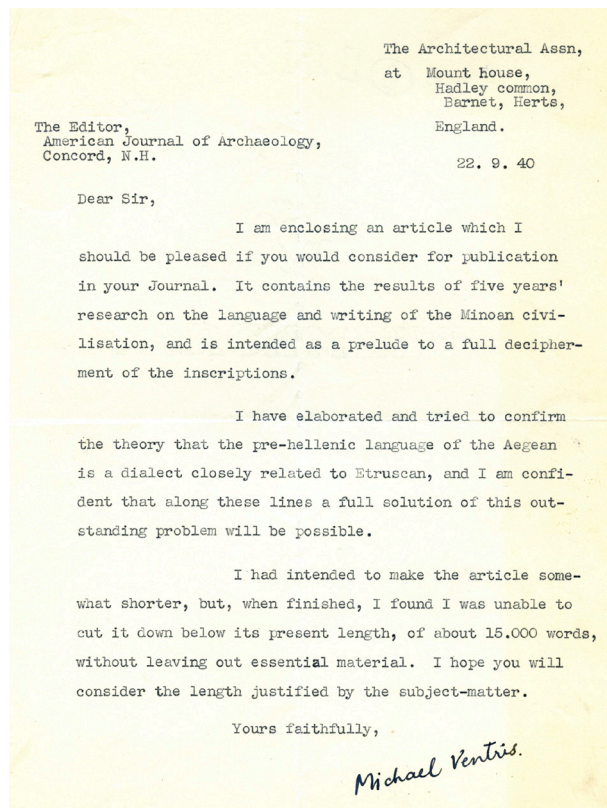
²¹ Barber 1974, 19.

standard of ‘probability’.

While not proposing a decipherment or a translation, Kober made sound observations about the possible affiliation of the disc. She noted (1) that the inscription was to be read from outside in on both faces and (2) that the first sign-group on face A was preceded by four dots (based on Sir Arthur Evans’ drawings *SM I*, 280), while the first sign-group on face B was preceded by five dots (based on Sir Arthur Evans’ drawings *SM I*, 282)—this point is now disproved by the work of Louis Godart;²² and (3) that perhaps these dots signify that these are the 4th and 5th sides in a series, thereby suggesting that three other such printed text surfaces might have existed. So much for Kober being incapable of imaginative conjecture.

Kober pointed to parallels for such circular writing in Linear A on a gold ring from Mavro Spelio and on clay cups from Knossos (noted also by Evans), and to the ‘boxing in’ of sign groups in Cretan Hieroglyphic and Linear B. For formal resemblances to signs on Crete, she used the well-known parallel of the Arkalokhori axe.²³

**Fig. 8. Ventris letter to editor
of *AJA* 092240. Palaima.
PASP Archives.**



²² Godart 1995, 63.

²³ Robinson 2002b, 307, for the Arkalokhori axe characters.

As for linguistic analysis, Kober observed: “Forty-five different signs are found; how many there were in the script to which it belongs cannot even be conjectured.”²⁴ Her final judgment is clear: “Of all the Minoan scripts that of the Phaistos Disk is least likely to furnish the clue for decipherment unless more inscriptions of this type are found.”²⁵

Note that while Kober did classify the Phaistos Disc as among “Minoan scripts,” she viewed the sign parallels with other inscribed objects found in Crete as ‘resemblances’ and not ‘identical’ signs. She saw the disc as of little value in discovering the language or languages that were represented in the Hieroglyphic, Linear A and Linear B texts. She therefore concluded that debating its origin was fruitless for her main purpose: the decipherment of the Minoan scripts.

In 1940, when Ventris submitted to *American Journal of Archaeology* the manuscript (Fig. 8) that would eventually become his article “Introducing the Minoan Language,” he was eighteen years old. His father Col. E.F.V. Ventris was not long dead (February 14, 1938). Col. Ventris and Ventris’s mother Anna Dorothea Janasz ‘Dora’ Ventris were divorced in 1935.²⁶ His mother had committed suicide just thirteen weeks before (June 16, 1940).

In a letter to Evans written on Easter Sunday March 24, 1940, Ventris recalls proposing to Evans two years earlier, in 1938, when Ventris was 15 years old, that the language of the linear scripts was likely Sumerian. He retracts this idea in his Easter letter of 1940. He had been removed from Stowe School after the German invasion of Poland in September 1939 cut off the income his Polish-born mother had from family estates there.²⁷ And he was directed toward a career in architecture, admitted to the Architectural Association in January 1940.

It is not hard to imagine Ventris’s sense of isolation at this time. It was undoubtedly a form of escape and solace for him to work on this article. It reached *American Journal of Archaeology* some six months after Carl Blegen’s discovery of Linear B tablets at Pylos. This coincidence, I would guess, disposed the editor Mary H. Swindler to rush Ventris’s global overview of the Minoan scripts into press (Figs. 9 and 10). I would say Swindler did so ill-advisedly, but for the views of Emmett L. Bennett, Jr. that we will discuss here at the end of my paper. Kober subjected Ventris’s article to a *damnatio memoriae* in her survey of meaningful work on the Minoan scripts. What did he have to say that caused her to do this and how did he say it?

²⁴ Kober 1948, 87

²⁵ Kober 1948, 88

²⁶ Robinson 2002, 26.

²⁷ Robinson 2002, 30-31. Robinson clarifies that Ventris was staying on at Stowe for at least one more term, a not uncommon practice.

November 8, 1940

Michael Ventris, Esq.
The Architectural Association
Mount House
Hadley Common
Barnet, Herts, England

My dear Mr. Ventris,

I was delighted with your manuscript: "Introducing the Minoan Language". I am preparing to set it up immediately. I believe this forms a definite contribution to the subject, and I should like to get it out as soon as possible, although I shall be putting it ahead of other manuscripts in the order in which they should be printed.

It will be possible for you to see only one proof which will be sent to you by Clipper on onion-skin paper, and which should be returned to me by Clipper immediately upon receipt. I am considering printing it in the December issue.

Very sincerely yours,

MHS:SA

Mary H. Swindler

Fig. 9. Mary Swindler letter to Ventris 110840.
Palaima.
PASP Archives.

With the temerity of a teenager, Ventris flatly declares: "The as yet unanswered question of where the Phaistos Disc came from, and what language it is in, does not bear directly on our problem either. It is obviously foreign, probably from Asia Minor, and almost certainly not in Minoan or anything like it."²⁸ In this he is following the views of Sir Arthur Evans, reinforced by Pernier, that the Phaistos Disc "is itself of non-Cretan origin, and that it probably attaches itself to an old Anatolian element of which some later traditions are to be found in Lycian remains".²⁹ The decisive factor here in Evans's opinion is the complete absence from the 241 signs on the Phaistos Disc of the most frequently occurring signs in Cretan hieroglyphic (still among the most frequent as seen in *CHIC*) script: for example, the 'eye' (*CHIC* 005), the 'trowel' (*CHIC* 044) and the 'double axe' (*CHIC* 042). Evans thought other signs had Philistine, Carian or Lycian formal parallels.³⁰ Evans reasoned in this way. If the Disc is of

²⁸ Ventris 1940, 497

²⁹ *SM* I, 285 and cf. 22-28.

³⁰ *SM* I, 286-287.

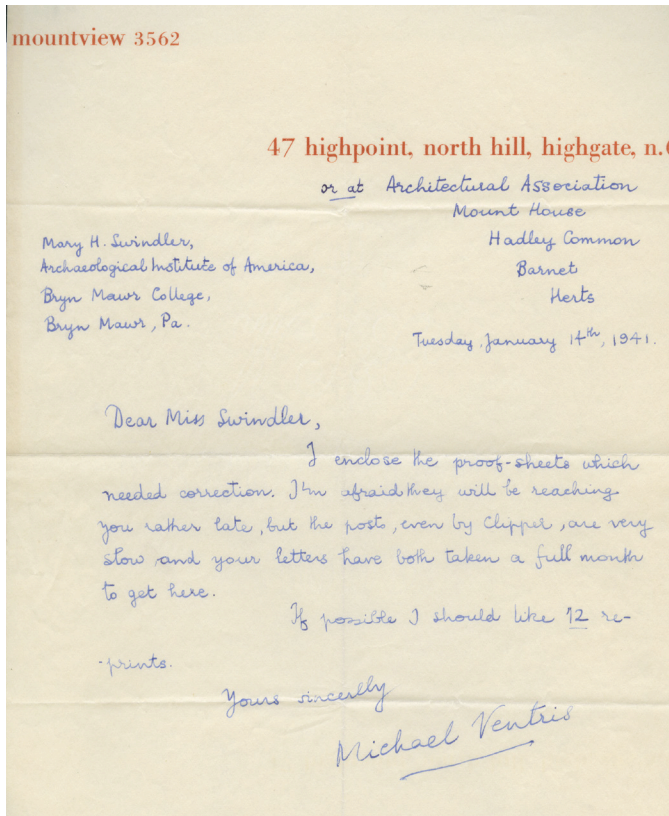


Fig. 10. Ventris letter to Swindler 011441. Palaima. PASP Archives.

Cretan origin, it should not use yet another totally independent pictorial script, even in a different media (stamped signs). It should be an extension, somehow, of Cretan Hieroglyphic.

Ventris eventually argues that human figures among the characters on the disc are “Armenoid in type, and their clothes are most like those of certain Asianic peoples.”³¹ For the clothing he points to the “specifically Hittite *tiara*” without citing Evans who makes the same point.³² But he cites Evans as thinking that “the ethnological evidence points to Lycia.”³³ Ventris departs from Evans in reading the texts on both sides from right to left, i.e., from outside inwards, and he points to two decisive facts: (1) the characters all face right and on the analogy of Hittite, Minoan and Egyptian they should be oriented toward the beginning of the text; (2) the characters have clearly been stamped right to left

³¹ Ventris 1940, 499.

³² *SMI*, 277.

³³ Ventris 1940, 499 and note 16.

as seen when one sign font overlaps another.³⁴

Evans used statistics of sign-group length in support of his idea that the language of the Phaistos Disc was different than the language of Cretan Hieroglyphic.³⁵ Evans also made an assumption that certain signs on the Disc functioned as ideograms, individually and in combination. Ventris, as later Kober, rejects taking any signs as ideograms. Ventris therefore arrives at statistics for the length of sign groups that are different than those of Evans:³⁶

	Ventris	Evans
1 sign	0%	20%
2 signs	8%	40%
3 signs	26%	35%
4 signs	38%	5%
5 signs	20%	0%
6 signs	3%	—
7 signs	5%	—

Ventris uses his statistics only to argue that the disc system is a syllabary. Evans, however, compares statistics from the Phaistos Disc and from Minoan seals and graffiti to argue for a difference in the languages being represented by the different ‘scripts’.

Unlike Kober, Ventris extrapolates from the 45 distinctive sign forms on the disc to an overall character set “that can hardly have been much more than 60.” This guess is based on Ventris’s observation that “most of the signs occur several times.”³⁷

Ventris isolates radicals (or ‘roots’) and studies the beginnings and endings of sign groups. In this he is already performing the kind of morphological analysis that is fruitful in cryptanalysis. He notices, because of his (and Kober’s) direction of reading the texts, a 4:1 (dis)proportion between prefixes and suffixes.

This sets the Disc apart. In Ventris’s opinion, “[t]here is not, so far as I know, a single recognizable prefix in any Minoan inscription: suffixes, on the other hand, are frequent and easily distinguishable.”³⁸ This remarkably bald claim for the complete absence of prefixes in all known Minoan texts is decisive

³⁴ For photographs, drawings and discussion of the Phaistos Disc and related data, see now conveniently and succinctly Robinson 2002b, 296-315.

³⁵ *SMI*, 284.

³⁶ Ventris 1940, 498; *SMI*, 284.

³⁷ Ventris 1940, 498.

³⁸ Ventris 1940, 500.

for his conclusion that “the Phaistos Disc is not in Minoan or anything related to it.” He does, however, point to parallels in pictorial forms of signs and in what he can ferret out of language structure with Hattic inscriptions in Anatolia.³⁹

What the ‘prefixes’ represent is another matter. Ventris, noting, as did Evans, the frequent occurrence of the ‘helmeted head’ together with the ‘shield’,⁴⁰ at the *beginning* of 30% of all sign groups, proposes that this combination might be “a personal determinative, or something of the sort.”⁴¹ What he means by this he does not explain. Perhaps that these sign groups contain ‘titles’ or ‘names’ or nouns identifying individuals.

Because several radicals occur without the 2-12 prefix, Ventris regards this sequence as a sign of inflection, “[p]ossibly a plural affix.”⁴² He then adds that in the fifth sign group on Face A, the ‘helmeted head’ + ‘shield’ combination was ‘added afterwards, so its omission evidently did not affect the syntax of that particular sentence.’ It is hard to make any sense of such a statement. A later addition, obviously in the ‘proofreading’ stage, of an omitted element of a sign group is, quite to the contrary, proof that the once omitted and now inserted element is important to understanding how the sign group fits into the meaning of the text.

Note, however, that Ventris’s prefix:suffix ratio disproportion would have been no problem if he had read the texts inside out as Evans did. His prefixes would have become suffixes and vice versa. It may be that Ventris resisted following Evans’s direction of reading the text and bringing this ratio into line with the other Minoan material because he was convinced that the language behind the disc was not the same as that behind the other Minoan texts.

Lastly we come to Emmett L. Bennett, Jr. In his 1947 dissertation in surveying the development of Minoan scripts, he makes no mention of the Phaistos Disc. He was one of the most serious adherents to the view that any discussion of the Phaistos Disc should be discouraged, if not prohibited, at regular Mycenological conferences. Yet, as editor of *Nestor* and as a scholar who had an insider’s view of the decipherment of Linear B and of ‘real-life’ cryptanalysis, he received many published and unpublished proposals for decipherment of the disc and was regularly in polite, though skeptical, correspondence about it.

Most remarkable is his account of the foundations of the Ventris decipherment. As we have seen Kober subjected the teenage Ventris’s 1940 *AJA* article to a *damnatio memoriae*, placing it effectively in the same category

³⁹ Ventris 1940, 499.

⁴⁰ *SMI* signs 2 and 12.

⁴¹ Ventris 1940, 498.

⁴² Ventris 1940, 498 and n. 15.

(‘worthless’) in which she put his Mid-Century Report questionnaire. Kober replied that because the questionnaire called for all sorts of pure speculation, it was a “step in the wrong direction and a complete waste of time.”⁴³ Remember she was dying of cancer at the time and struggling with her last bits of strength to correct the errors Sir John L. Myres kept reintroducing into successive iterations of proofs.

Bennett’s account, however, of “Michael Ventris and the Pelasgian Solution,” in 1989, makes no mention at all of the section Ventris devotes to the Phaistos Disc, despite the fact that Bennett sees as the main value of Ventris’s article the attention it pays to name-radicals and to their prefixes and suffixes in Etruscan.⁴⁴ Bennett is right that Ventris was focused on personal name structures because of the appearance in *PoM* IV (1935) of drawings of KN tablets **Ap 639** and **As 1516**. The texts of these tablets are now known to be lists containing 42 women’s names and 65 men’s names all clearly marked by ideograms. Etruscan inscriptions provided Ventris with many personal names to analyze. As Bennett points out, in 1940 Ventris (1) had proceeded by separating radicals from prefixes and suffixes in a long list he had compiled of Etruscan personal names; (2) he then tested how these would be represented if such a language were written in a syllabary like the Cypriote syllabary; (3) he then compared the Cypriote syllabary with the Minoan scripts (particularly Linear B) and arrived at enough parallels (fourteen) to give him confidence that analysis of texts in the Minoan scripts would make it possible to identify patterns of consonant alternations. He worked at applying the same method to the Phaistos Disc.

The Phaistos Disc then is relevant to our understanding of the procedures that led to Ventris’s eventual success in deciphering Linear B. It was rightly left out of later discussions by his two principal collaborators. But the treatment Ventris gives it in his first article reveals two important aspects of Ventris’s character and habits of mind: (1) his correct decision to concentrate already at this stage on the structure of personal names; and (2) his scholarly integrity in not simply reversing the direction in which the disc is to be ‘read’ in order to fall in line with his inspirational mentor and produce a more positive result.

Bennett is also correct that once Ventris was convinced by the work of Kober that the variation in name structures had to do with inflection rather than morphology, it was inevitable that his work with fuller material from Knossos and Pylos would lead to his constructing a ‘grid’ like Kober’s that would lead to a non-Pelasgian solution.⁴⁵

⁴³ Sacconi 1988, 37.

⁴⁴ Bennett, 1989, 15-17.

⁴⁵ Bennett 1989, 20.

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