

PROBLEMS OF LINEAR A

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The Homeric oral traditional poems - the sole surviving pieces of a once extensive epic cycle - still represent the oldest Greek literature we have: - until, that is, it is proved conclusively that epic verse itself was written down in Linear Script B, which, decyphered finally in 1952, and proved then to be Greek dialect (Mycenean), has ousted Homer from the position of supreme antiquity as far as records written in the Greek language are concerned.

We may then go to Homer for any literary guide to, or support for, modern archaeological and philological investigations. Now Crete is the centre of our present discussion of these Bronze Age Linear scripts, particularly Linear Script A, and we may ask, is there any passage in Homer which may conceivably refer to the linguistic state of the island in the Bronze Age (second millenium B.C.). Here the famous passage in Odyssey 19 (l. 172 ff.) is relevant: Odysseus is representing himself as a Cretan prince, and by way of description of his so-called native land says:

"There is a land called Crete, in the midst of the wine-dark sea; a land which is beautiful and rich, a land surrounded by rushing water: and therein dwell many men, countless number of them, and there are ninety cities, and one tongue is mixed with others, (ἄλλη δ' ἄλλων γλῶσσα μεμιγμένη)" - a revealing statement perhaps - "and Achaeans dwell there, and Eteocretans too, great-hearted men: there are Kydonians also, and Dorians of the three tribes, and bright Pelasgians."

Such is possibly the Homeric picture of Cretan settlement in the Bronze Age. Now for the picture revealed by modern investigations. First perhaps we should make clear the chronological division of the Bronze Age in the Aegean, so that we may have the sequence of archaeological and philological discoveries firmly nailed down to clearly defined periods within this era.

Sir Arthur Evans, the great inaugurator and overseer of the Knossos excavations in Crete, worked out this scheme, in essence, from about 1905 onwards, for a chronological division of the Aegean Bronze Age. Briefly, there are three periods - Early, Middle, and Late, and three sub-divisions for each period (I, II, III), with sometimes a further sub-division of these into A, B, and C. Such periods, referring to Crete, are called Minoan, referring to the Greek mainland, Helladic, and referring to the islands, Cycladic. The dates of all three roughly correspond. (See chronological table appended to this article.)

One has then the following series of settlements:-
 The Early Helladic people, who used copper, and later bronze, and produced a pottery more sophisticated than anything known hitherto in the area, appear to have been a non-Indo-European race, and we are not concerned with them further in this paper. To the Middle Minoan, Middle Helladic period (2000, 1900 B.C.), an invasion of people into mainland Greece is ascribed: they produced the unusual and distinctively characteristic pottery known as Minan Ware - found in mainland Greece, at Troy, and now at Beycesultan, in South-west Anatolia. These people Wace holds to be the first Greeks, and Palmer - one of whose theories, regarding the A script itself, will be discussed in the course of this paper - believes to be Luvians, based on the newly-found evidence of Beycesultan. The transition to Middle to Late, Minoan and Helladic, is rather one of slow development, which gives Wace reason to think that the culture continued to be a Greek one. The end of the Late Minoan and Late Helladic periods was swift and sudden, and we may suspect that it was the same Dorian invaders wiped out Knossos (there is considerable evidence mounting up for their having been sea-borne) who certainly destroyed the great mainland kingdoms of Pylos and Mycenae. After this widespread destruction, we have the earliest beginnings of the Iron Age, whose conventional and almost Homeric epithet "dark" may prove to be something of a misnomer, since, as Wace indicates, it is unlikely that the Greeks suddenly became illiterate until the introduction of the Phoenician script (i.e. c. 8th century), even though we have no material evidence as yet for continuing literacy in this period.

What then of the scripts found by archaeologists in the ruins of the Cretan, and later the mainland Greek palaces (i.e. Linear Script B only here), and known to be datable to the Middle and Late periods of the Aegean Bronze Age? There are three of these:- The Hieroglyphs, and two Linear scripts, classified as A and B, the latter clearly a later development than the former.

First, and probably least important, are the so-called hieroglyphs; more accurate perhaps is Evans' term of conventionalized pictographs, for these characters had, as far as we know, no direct religious connection, like for example the Egyptian hieroglyphic script. However I shall continue to refer to them as hieroglyphs for convenience's sake, provided the reservation on the use of this term is noted.

Material with hieroglyphic inscriptions was discovered mainly by Evans at Knossos. Datable to MM Ia - MM IIIa, it consists of 19 lumps of clay sealed with inscribed signets, and 63 clay sealings, tablets, bars, or labels. This, the earliest of the Bronze Age scripts found in Crete, still remains undeciphered. Few leads are forthcoming as to its nature, origins, and connections. One may suppose that some of its signs were probably influenced in shape by Egyptian models, - either from imported small objects or from merchants' observations - but it is certain that the principles of Egyptian writing were not adopted completely and uncritically. Egyptian hieroglyphs represent a collection of ideographic, phonetic, and determinative signs - a linguistic composition "common to those writing systems", says I.J. Gelb, "which remained in the word-

syllabic stage". Here we may compare the Syrian and Palestinian, or West Semitic, experiments in writing systems, from c. 2200 B.C. on, which aimed at the "design of a script, which, from its very beginning was entirely phonetic". It appears so far that the Cretan hieroglyphs conform more to this pattern than to the Egyptian, for the small number of signs probably means they are phonetic in value.

We can trace a series of stages in the development of this hieroglyphic script. First, seal-impressions showing its earliest form; these are dated by Evans to MM Ia. Second, seal-stones, with groups of one to six signs of an obviously more advanced script, datable to MM II. Third, a cursive form of this script, which is found incised on clay, dated roughly to MM IIIa.

As far as purpose goes, this script appears to be a means of identification, - more efficient than the earlier makers' and owners' marks. To this "embryonic syllabary for spelling out names and titles", write Ventris and Chadwick, two important additions were made, dictated by the requirements of the palace administrative system, and characteristic of all later Aegean scripts:-

- "1) The development of a list of standard ideograms as abbreviations for the commodities being counted, and as a more graphic indication of the tablet's contents to the non-literate members of the household. With doubtful exceptions, ideograms (in the sense of symbols which stand for a definite word-meaning rather than for a conventional sound-value) only occur with numerals in all three Aegean scripts, never as the spelling of ordinary words or sentences . . .
- 2) A written system of numerals and fractions . . ."

The second of these Bronze Age scripts, and of course the one which immediately concerns us, is Linear Script A, datable from MM IIIb - LM Ib. For the meantime suffice it to say that this first linear script, as the successor of the hieroglyphs, comprises syllabary, ideograms, and a numerical and fractional system. We have one tablet, from Mallia, which shows hieroglyphs together with an incipient form of the A script, but this is our only record so far of the transition. Other A inscriptions have come to light from Knossos, Phaistos, Tylissos, Zafer Papoura, Zackro, and Palaiocastro, but the most important Linear A material is the collection of tablets from the Royal Villa at Haghia Triada, which, apart from being the largest and best preserved collection, also represents what is apparently the latest form of A script. But before I can discuss more of Linear A itself, the perspective of Bronze Age scripts must be completed, and only when Linear B - so far the only script to be cogently deciphered - has been briefly discussed are we free to retrace our steps to the previous A script and its problems.

Discoveries of Linear B tablets have been considerable, and, unlike the hieroglyphs and Linear A, not merely confined to Crete: we have now over 5000 texts in hand - an enormous number, especially when we compare the scanty amount of hieroglyphic and A inscriptions at present available. B tablets first turned up in Evans' excavations at Knossos, from 1900 on - it was Evans who, encountering two Linear scripts, of which one was apparently derived from the other, named them respectively B and A. However, the discoveries of more

tablets (several hundred in fact) by C.W. Blegen at Pylos, in 1939, on the site of the old Helladic palace in the north-west Peloponnese, were of inestimable importance for the eventual decipherment. In 1950, and again in 1952, more B tablets turned up at Mycenae - and again these were of prime importance in helping scholars working on the script towards a solution.

The publication by Michael Ventris, an architect who worked on the tablets in his own time, inspired as a schoolboy by Evans' lecturing on his Minoan finds, and John Chadwick, a Cambridge philologist, of a decipherment of the B script in 1953 burst like a thunderclap upon scholars. The vital article, 'Evidence for Greek Dialect in the Mycenaean Archives' published in the JHS for 1953, established beyond reasonable doubt that the language written in Linear B was in fact Greek. Only a few diehards resisted - among them Sir John Myres - and they not for long.

The story of the decipherment is long, and is dealt with lucidly in both the immense work of scholarship, 'Documents in Mycenaean Greek', which codifies the results of the decipherment, and in a smaller volume published by Chadwick recently, after Ventris' untimely death, called simply 'The Decipherment of Linear B'.

Some facts are, however, worth noting. Not least the consistent and in many cases ridiculous efforts of scholars prior to about 1944 to find out what language the Linear B script might represent. In Ventris' and Chadwick's own words: "the search for possible candidates spread over an absurdly wide area, and included Hittite, Egyptian, Basque, Albanian, Slavonic, Finnish, Hebrew, and Sumerian. Apart from Evans' own work nearly every attempt to discuss the script prior to 1944 may be safely and decently consigned to oblivion."

Early experiments on Greek readings for the Knossos tablets, in 1930 and 1931, i.e. prior to the Blegen and Wace discoveries at Pylos and Mycenae - had been doubtful. Meanwhile archaeologists were persisting in attempts to free Mycenae from the Evans-imposed cultural domination of Knossos, and Nilsson, in a series of articles from 1927 to 1933 expounded the theory that Greek religion and myth, and not only the Greek language, had firm roots in the Mycenaean age. The next contributions were forthcoming from America:- Blegen's Pylos discoveries; the somewhat monumental work of Alice Kober, consisting of a systematic analysis of the script, and careful deduction of conclusions from this; Bennett's codification of Linear B weights and measures. Both Bennett's and Miss Kober's evidence added to the now steadily-growing belief "that Linear B at Knossos represented a new language introduced from without." The demonstrable lateness of Blegen's B tablets meant that they were less useful in this connection.

In 1951 and 1952, Ventris' Work Notes were circulated privately; these contained a review of fuller evidence for inflection. The syllabic grid, an essential item in the mechanics of decipherment, was slowly being built up, and in 1953 the decisive article "Evidence" appeared, in which the Linear B language was identified as a Greek dialect ancestral to Arcado-Cyprian (then the oldest known stratum of

the Greek language), a large number of interpretations was proposed, and a table of sign-values published.

What precisely did all this mean? Undoubtedly the Ventris decipherment, the solution of a fifty-year-old problem, was one of the greatest and most exciting scholarly events of this century. The Greek language barrier was now set back from the time of the first inscriptions in the Phoenician characters, c. 8th century B.C., to the Bronze Age period, from LM II to LH, i.e. c. 1450 - 1200 B.C. Hitherto, the Arcado-Cypriot dialect, with its forms in Homer and isolated inscriptions, had been held to be the earliest known form of Greek. The decipherment confirmed many linguistic hypotheses (e.g. the descent of the Greek genitive in $\omicron\upsilon$ from IE **asya* - since the intermediate Linear B, or Mycenaean Greek, form is in fact -*ojo*), and is invaluable generally for the construction of a far more accurate picture of Greek dialectal distribution and development. It is not only the philologist who benefits: the ancient historian too is indebted to Ventris and Chadwick, since the revelation of the Pylos and Mycenae tablets particularly as economic records enables a study of these detailed and illuminating inventories and accounts to be made, and more accurate observations of the society, administration, and general economic background of these kingdoms to be carried out.

So much by way of a suggestion of the importance of the decipherment generally. What of its meaning for the position of Linear Script A?

The relation of B to A is still shrouded in some mystery. Statistically speaking, there are 87 signs in B ("84, if possible variants are excluded"), of which 45 have close equivalents in A, and 10 more, doubtful parallels. There are 29 B signs which are apparently innovations, including many of the rarer signs - 15% altogether has been estimated, by way of a proportion. Mention had best be made here of the Cypriot syllabary, which also has its origins in the late Bronze Age, but which continued in use into the classical period. A Cypriot syllabary constructed from inscriptions found at Enkomi and Ugarit shows some signs of which parallels or equivalents appear in B but not in A. This adds some weight to the view that the relationship of the A and B scripts was "once-removed" at least.

Certainly, whatever the language of Linear Script A, it is not the same as that represented by B, - but of course we should allow for a closer link, possibly, between actual scripts, than between the languages expressed by them. Early in 1953, before the publication of the Ventris decipherment, Dr Bennett had written:

"The difference between the Linear A and the Linear B scripts is a serious one; not a matter of gradual development, nor of an elegant variation, but of a radical adaptation of the old to the new, or perhaps even a new construction following roughly on an older model. The language also, and the names appearing on the accounts are clearly different, and where the same sign is used in both Linear A and B, there is no guarantee that the same value is assigned to it. At the same time, the affinity of Knossos in LM II script (i.e. B) and in methods of book-keeping is clearly shown to be with the Mycenaean

mainland rather than with the rest of Crete."

Linear B then, with its collection of 5000 tablets, is now, in most essential respects, deciphered, although a very great deal remains to be worked over as yet. Linear A is very much worse off. As regards the available material, apart from the handful of earlier sealings etc. from Knossos, the bulk of it comes from the Haghia Triada site, to which I have referred above. These texts represent what is probably the standardized form of the A script: they number in fact a bare 150 rectangular clay tablets of which only thirty are reasonably complete - and most of these are barely legible - and a few sealings (cretule), found in the Royal Villa, and associated with remains of LM Ia frescoes and stone jars. Other inscriptions are mostly datable to MM IIIb. We may conclude from this that Linear A, in its standardized form, came into general use apparently at the beginning of MM IIIb and did not survive the introduction of Linear B at Knossos.







Generally speaking, these Haghia Triada tablets are of "page" shape, displaying from four to nine short lines of script. The layout is often cramped - entries and individual words are often divided between the lines, with the word-divider written as a dot in the middle of the line-height. One may compare here the Mycenaean tablets, which are frequently of "palm-leaf" shape, with one or two long lines of script. Previously, in the hieroglyphic inscriptions, the direction of writing appeared to be variable; now, in Linear A, it seems to be uniformly left to right.

The subject matter of the Linear A tablets appears to be the record of rations of various products, agricultural and otherwise, issued to named individuals. Even here, the A material compares badly with texts in Linear B: the descriptions seem to be the very barest - there is nothing corresponding, for example, to the detailed Pylos furniture catalogues. Chadwick notes that in the Haghia Triada tablets there are no visible equivalents of the Linear B ideograms denoting persons or animals. This would seem to be true on the whole, although Gould and Pope appear to have found ideograms for both MAN and OFFICIAL, and Gordon for ARCHER.

The Linear A script is made up of syllables, ideograms, and metrical and numerical signs - all of these at a more developed stage than the previously hieroglyphic script, which as we have seen inaugurated these three essentials of Aegean Bronze Age scripts.

The syllabary itself comprises about 75 signs: 45 of these have "possible cognates" in B, and in some cases hieroglyphic originals can be traced. We must note that identical sound-values with B in these possible cognates are not proved; here again we are hampered by lack of sufficient material.

The ideograms are the next group of the signary for consideration. There are thirty simple ideograms, of which the following five signs represent some of the most frequently recorded commodities:

 WHEAT	 BARLEY (G. & P.)	 CORN	 OLIVE	 FIG
	 OIL (V. & C.)			

Four of these (WHEAT, BARLEY/OIL, OLIVE, FIG) have hieroglyphic origins, and their Mycenaean counterparts tend to appear in the same order. In addition, there is a series of sixty compounded ideograms, or simple ideograms to which 'ligatures', i.e. single signs - phonetic, ideographic, or metrical - have been joined. The greatest number of ligatures are added to the ideograms

Φ WHEAT

and

⇒ OIL/BARLEY

They make perhaps some distinction between the types of commodity to which they are attached - beyond this, their purpose is so far unexplained; we may note the ligatured amounts are totalled under the simple commodity ideogram; such distinction as there is, therefore, would appear to be slight.

Lastly, there are the numerical system and metrical signs. Except where the ideogram $\Delta\bar{\Delta}$ "by weight", or perhaps representing a unit weight is used, all the Haghia Triada commodity ideograms are directly followed by whole numbers and/or fractions. Whole numbers are shown as follows:-

/ UNIT

- TEN

○ HUNDRED

⊙ THOUSAND

We may note the absence of metrical symbols (except of course $\Delta\bar{\Delta}$), which are characteristic of Mycenaean accounts. The values of the whole number series being clear, there have been some attempts to assign values to the fractional series also, from which the only results are as yet that

$\zeta - \frac{1}{2}$

$\eta - \frac{1}{4}$

$\xi - \frac{3}{4}$

in all probability. An alternative theory has been put forward, that the fractional signs represent a series descending by one-and-a-half, from one-half to one-thirty-second: all we can say at present is that this is possible, since the number of fractional signs would indicate that values like one-thirtieth, one-thirty-second are expressed. Ventris and Chadwick further conclude that there is a possible direct connection between the A metrical signs

+ T c: ζ $\frac{\eta}{2}$

and some Mycenaean signs for weights and measures. For the time being, once more 'ne plus ultra'.

Some work on the script as a whole, however, has gone ahead. In 1945, the Italian epigraphist, Carratelli, published an edition of the Linear A material: a thorough and well-documented piece of scholarship, with photographs and transcriptions of the tablets, seals and some comparative material, and a generous admixture of tables codifying the various signs. Carratelli's L numbers - simple L for syllables, Lc for ideograms, single and ligatured, Lm for metrical signs - are now generally adopted in preference to Myres' classification - a muddled and misleading system of AB notation, published in the second volume of *Scripta Minoa*, a work so unfortunately inadequate in many respects.

The Swedish archaeologist, Arne Furumark, well known for his work on Mycenaean pottery, showed, in an article circulated privately in 1956 amongst scholars working on or concerned with Linear A problems, a partly-constructed syllabic grid on the Ventris model.

Once more the lack of material has frustrated any major advances here. The previous year, 1955, had seen the publication by Goold and Pope, two Cape Town scholars, of a report entitled 'Preliminary Investigations into the Cretan Linear A Script', prepared for the Linear B Seminar of London University's Institute of Classical Studies. This is a careful, though now somewhat out-of-date, analysis of the material at hand, including an introductory survey, a comprehensive index of signs, appendices dealing with word-frequency, and two charts showing the signary with values generally agreed (i.e. on the basis of Linear B, which must immediately invite our caution).

The main hope for decipherment rests, as Chadwick states in a recent article assessing the Linear A position, on "identification of the meaning of sign-groups on the basis of their usage". By this method, necessarily using B values - and of all assumptions this is perhaps the most reasonable, providing we regard it as an assumption and nothing more. - we have a number of possible identifications:

- a) 'ku-ro', which appears to be a totalling formula; compare B 'to-so', 'to-sa', "so much, so many"
- b) 'ki-ro', which may be used (from the indications of contexts so far) after the manner of B 'o-pe-ra', "owed" or "deficit".
- c) the sign-groups written over various vessels on the tablet classified as HT 31:
 - (i) 'su-pu'
 - (ii) 'ka-ro-pa₃'
 - (iii) 'su-pa₃-ra'
- d) the word 'ku-ni-su', frequently associated with the WHEAT ideogram, Both c) and d) appear to have equivalents, possibly, in some Semitic languages (vid. inf.).
- e) the sign-group found on objects with a religious association:

'j/a-sa-sa-ra'

- the form 'a-sa-sa-ra-me' occurring twice.

In addition to these, some proper nouns already known in B have appeared:

- e.g. places - B 'Ku-do-ni-ja'
 A 'Ku-do-ni'
- people - B 'Qa-qa-ro'
 A 'Qa-qa-ru'

We may note in passing the acceptance of B 'o'/A 'u' in this word - a point further developed by the Semitic school of theorists working on Linear A.

The script, with forty-five of its signs interpreted by these B values, looks as if it has many open syllables, being generally without complicated consonantal groups. But was it a foreign borrowing, and if so, for what language was it originally designed? This question, and the problem of how much of Linear B is representative also of Linear A (the failure to distinguish between 'l' and 'r' for example, and the use of the semi-vowel 'j' and 'w' as glide-sounds between consonant and vowel) are of prime consideration, but as yet they remain most uncertain.

Linear A, like Linear B before the Ventris decipherment, has been exposed to abundant theorizing regarding its origin, nature, and affinities, though no such impressive list of absurdities can be compiled for it as yet. Indeed, the suggestions so far put forward are confined to more reasonable and at least theoretically possible geographical limits. Greek is out, since Linear B is now proved to be Greek, and Linear A is not the same; but the languages of Asia Minor, Hittite, for example, and Luvian, are worthy of consideration. Perhaps also there are affinities to be proved with early Georgian dialects. That covers the Indo-European field in this area fairly extensively. The Semitic linguistic group is the other obvious alternative. Here, in Bronze Age times, the most important languages were Akkadian, Ugaritic, and Hebrew. Both sides have their protagonists, sane and silly. There is also no doubt something to be said for theories allowing affinities, for example, with the proto-Hittite stratum, or Hattic language, which is non-Indo-European.

To deal first, however, with the Indo-Europeanists. In 1955, Signor Carratelli himself admitted the possibility of Linear A's having affinities with Indo-European, but refused to commit himself to this view. His fellow countryman, Meriggi, was somewhat more positive. Meriggi is well-known for his extensive work on Hieroglyphic Hittite; in 1956, he published work drawing parallels between certain Asia Minor languages and Linear A (for example, the Lycian name β adunimi' and Linear A 'wa-du-ni-mi'). Meriggi was closely followed by Furumark (vid. sup.), who suggested that certain '-ti' endings in Linear A are perhaps the Indo-European 3rd person singular verb-forms: (for example, Skt. 'vadati' "he says", Hitt. 'akwantsi', etc.).

The most recent and perhaps the most forceful Indo-Europeanist is Palmer of Oxford, who has had considerable experience in the field of comparative philology generally. He suggests that Linear A is either to be identified as, or else has decided connections with, the Luvian language of south-west Anatolia. Now Luvian is a language, little known it is true, but fairly certainly belonging to the Indo-European group; it is known to have some relationship with both cuneiform and hieroglyphic Hittite - a closer connection with the latter, probably, than with the former. Palmer's argument, a towering structure of hypotheses, rests solely upon one assumption which, unfortunately, is the weakest part. Briefly, his contention runs as follows: in Linear A we have the word 'a-sa-sa-ra' appearing on religious objects. (Where 'j' is prefixed to this word it would seem to be used as a glide-sound.) In Hittite, we have the words for a mother-cult goddess 'Išhaššara-' translated "Lady". In Luvian, working by analogy, from other Hittite forms with existing parallels in Luvian, this word would presumably read 'Ashassara', though we have as yet no actual occurrence of this word. The similarity, however, between this conjectured form and the A word cannot be doubted; what is more, twice this word 'a-sa-sa-ra' appears with a suffix '-me-'. In Luvian a suffix '-mi-' means "my", and in Hittite we actually have the form 'Ishassara-mis' meaning "My Lady". As I have said, the fact that the Luvian is so far nowhere attested is the weak point;

the undoubted similarity in words, however, albeit hypothetical, and the evidence for the existence of this type of cult in Crete and at Beycesultan (within the Luvian linguistic area at the beginning of the second millenium B.C.), and further possible evidence, both archaeological and philological, for the widespread extent of the Luvian civilization (taking Palmer's account of the Luvian origins of the word Parnassos, for example, and his own interpretation of the geographical distribution of the pottery known as Minyan Ware), might all add up to something considerable. But the fact that similar mother-cults are attested over a very wide area indeed without necessarily being inter-related, and that Minyan Ware has not actually been found in Crete - however much of it there is in Asia Minor and the Greek mainland - would seem to detract from Palmer's Luvian - Linear A hypothesis. In fact, however much Palmer veils it with subsidiary and in many cases shaky evidence, the hard fact remains that the whole thing rests solely on the postulated Luvian form of a goddess' name. And should the contention of another scholar, Dr Tritesch, be accepted, viz. that 'isha-' is in fact a Hattic noun (i.e. proto-Hittite and non-Indo-European), and '-sara-' a Hattic suffix, the matter of Linear A's conjectural relations with Asia Minor would seem to be rather more complicated and perhaps less confined to Luvian than Palmer would have us believe. The suggestion of a Hattic origin for the language of Linear A is something of an interesting speculation, but cannot, regrettably perhaps, be considered more than this at present.

So much then for an indication of the Indo-Europeanist theories. It is a point worth noting that Palmer's theory - the most elaborate of that group - is based on religious inscriptions, whereas the Semiticists have been on the whole quite successful with sign-groups on the Haghia Triada tablets themselves.

First for consideration is Cyrus Gordon, an American scholar with an impressive record in Semitic philology and to a lesser extent, archaeology. His two articles in ANTIQUITY, stating first the general Semitic character of Linear A, and then declaring that in fact Linear A represents Akkadian (the lingua franca, it is true, of the nearby area of the East Mediterranean and hinterland in Bronze Age times), were marred by sub-standard scholarship and an unduly rash overall approach. Gordon seems to have rushed in where the more cautious Pope (the joint author of the Linear A report referred to above) feared to tread; Pope in fact quickly rose in answer to Gordon: in articles in ANTIQUITY and MINOS he pointed out some of the weaknesses and straight-out faults in Gordon's work, whilst proceeding himself to identify Linear A, more carefully, mainly on the basis of the frequency of triliteral root words, certain initial consonants, some vocabulary, and a few somewhat tenuous inflections, as a Semitic language.

Pope was supported by Davis of Witwatersrand, who perhaps takes up a position between him and Gordon, being rather more in agreement with Pope in most essentials so far expounded.

Before a few elementary tests are applied to this Semitic theory, we may note words on which identification is agreed:

"ku-ni-su"	(some sort of grain)	-Akk. "kunasu"
"su-pu"	(vessel names)	-Akk. "sappu"
"ka-ro-pa"		-Akk. "karpu"
"su-pa-ra"		-Akk. "saplu"

It will be seen at a glance that these words are essentially technical terms - they denote types of the commodity whose sign they are associated with on the tablets. It should therefore be borne in mind that these words are quite possibly borrowings from another language, arising from trade and other forms of commercial and cultural association.

A further reservation, one posed by Chadwick, may be added: the undoubted fluidity of the triliteral root, the characteristic word-formation of Semitic languages. Chadwick objects that merely to be guided by three consonants alone in identifying words, ascribing them to a definite linguistic group, is too tenuous; one needs to pay more attention to vowels. This, however, is not demanded as strictly by Semitic identifications as by Indo-European ones: the Semiticists so far seem to have produced some clear identifications following the tri-consonantal pattern and heeding such vocalic rules as are essential. It almost seems as if Chadwick is suggesting that Semitic identification may be made, but with no great regard for the prime features of Semitic languages generally.


But to apply his tests to the words so far identified. The first concerns the word 𐎗𐎠 + 'ku-ro', which appears to be some sort of totalling formula on the tablets, meaning perhaps "all". Gordon's first reading of this sign-group (taking the typical Linear B r/l interchange) was the Semitic root 'kull-', "all", (Akk. 'kalu', Heb. 'kol'). This is maintained by Pope, and by Davis. But the absence of any occurrence of this word as an actual totalling formula in any Semitic language induced Gordon to change his identification, in his second ANTIQUITY article, into Akkadian 'mullu', "paid out", "issued". A pity, since a definite B value, 'ku', has had to be altered, somewhat suspiciously, for this new reading, and what authority have we then for accepting as primary assumptions any other values from the B script once one has been altered for convenience? It is doubtful furthermore that the fact that 'kull-' does not occur specifically as a totalling formula in Semitic as far as we know is actually as conclusive as Gordon would take it to be; in no Greek dialect apart from Mycenaean is τόσοϛ the familiar equivalent of 'to-so', used to signify totals, for Linear B book-keeping is nowhere attested, and it is therefore possible that Linear A's totalling formula may also be peculiar to it.

Second, the vessel names of HT 31:

- 1.2 'su-pu' is identified as Akk. 'sappu', Ugar. 'sp'.
- 1.3 'ka-ro-pa' is identified as Akk. 'karpu'.
- 1.5 'su-pa-ra' is identified as Akk. 'saplu', Ugar. 'spl', Heb. 'sepel'.

~~The identification seems to be faultless enough: note however~~

the technical nature of these words; the vessels they describe may well have been imported into Crete and their Akkadian names taken over into the native language, (or local dialect).

Third, the word 'ku-ni-su', which often appears (e.g. on HT 95), with the WHEAT ideogram,  Gordon and Pope identify this with Akkadian 'kunasu', "emmer" (a species of wheat); this seems convincing enough - Furumark (an Indo-Europeanist) himself lends some support to this reading. Here again however it may be a question of a specific technical term being borrowed.

Fourth, the inflections noted so far - perhaps the most problematic of all, since there is little help to be expected from the contents of the tablets by way of morphological material, bare lists and accounts that they appear to be, together with the ever present difficulties of epigraphy and lack of sufficient evidence. Pope, in his article in MINOS, is the best authority regarding inflections. Certainly the word 'ku-pa-na-tu' would seem to be the plural of 'ku-pa-nu', but the case of 'da-ku-se-ne'/'da-ku-se-ne-tu' (taking the Akkadian plural in '-eti') is less positive.

Fifth, an equation which belongs to the work of Pope and Davis only: viz. that Linear A 'o' - Semitic 'u'. Both their examples however of the use of this single sign as a conjunction are apparently abbreviations or ideograms. Pope's further suggestion, that 'o' in Linear A is used as Semitic 'u-' prefixed to words, seems as yet to be unsubstantiated.

So much for the Semiticists for the time being. Apart from the one fairly clear inflectional form 'ku-pa-na-tu' revealed by Pope (and inflectional forms are, of course, all the more convincing evidence, since vocabulary is known to be fairly easily adopted from one language into another, but not so morphological phenomena), apart from this then, it rather seems that the Semitic words we have so far are almost as likely to be foreign borrowings as native growths. Are there any indications in the script that the language represented might be Semitic rather than Indo-European? None, since the origin of the A script is inextricably entwined with the problem of the nature and origins of the Cretan hieroglyphs, and there is the possible additional complication, already referred to, that the script may not necessarily have been designed for this particular language.

In conclusion, one may perhaps be allowed to venture the suggestion that even if Indo-European affinities are proved decisively this does not cancel out the possibility of the co-existence of Semitic affinities, and vice-versa; we may recall the Homeric line at the beginning of this paper - *ἄλλῃ δ' ἄλλων γλῶσσα μεμιγμένη* and one tongue is mixed together with others". The work of Benveniste on the Indo-European root, establishing its simplest pattern as CvC, reveals it as not far removed from the CCC pattern of the Semitic root (especially when we examine forms such as the Ayin Waw and Ayin Yodh verbs, whose central consonant is in fact a semi-vowel), and there are a growing number of revealed similarities between these two linguistic groups (which may of course ultimately amount to nothing more than general similarities of any linguistic group

with another), some of which are ably catalogued by Whatmough in Chapter VI of his 1956 Sather Lectures.

A further possibility is that Linear A represents a language hitherto unknown, though this is - it must be admitted - extremely unlikely. Again, it may have affinities with that age-old problem, Etruscan. Since, however, Etruscan itself is now known to have connections with Asia Minor, i.e. with Hittite, and some early Georgian dialects of the Caucasus region (supported perhaps by legendary links of Anatolia with the Etruscan people), any co-relation established between Linear A and Etruscan would add considerable weight to the argument that the script represents some predominantly Indo-European language. This has recently gained another adherent - Peruzzi, who holds that an Indo-European language was definitely known in second-millennium Crete, although he leaves aside, for the moment the direct linking-up of Linear A with some specific language of Asia Minor.

Such then is the state so far: a little more ground to the Indo-Europeanists perhaps than their opposite party, but a clear possibility nevertheless that both could be represented. The most obvious thing of all is of course the depressing lack of A material: above all new evidence must be brought to light, - and something more substantial than the third part of the Dictaeon Inscription in the Ashmolean, recently published in the Classical Institute's Bulletin. We also need a fully annotated edition of the Linear A texts so far; Carratelli's article, published in 1945, is not readily available, and some important work, as we have seen, has been done in the fifteen years succeeding its publication.

But nowhere is the need for more of this modern trend towards scholarly partnerships more evident than in dealing with Linear A and its problems. The philologist must have the archaeologist to discover and help co-relate his material: the archaeologist needs philological observation to establish his own conclusions and help colour in his sketched outlines. One does not know where Linear A might turn up next, if it turns up at all. It is unlikely that it would be found at any considerable distance from Knossos and Hagia Triada but, as Herodotus says, in the fullness of time anything can happen, and we must content ourselves with the hope that, should more vital material come to light, it will yield more conclusive results, and perhaps thereby abolish so much of the cautionary tale atmosphere which must surround any account at present of the Linear A question.

CHRONOLOGICAL TABLE

HIEROGLYPHS	{	c. 2000 B. C.	Foundation of palaces at Knossos and Mallia.
	{	c. 2000-1925	MIDDLE MINOAN Ia
	{	c. 1925-1850	MIDDLE MINOAN Ib
	{	c. 1850-1700	MIDDLE MINOAN II (Knossos and Phaistos).
	{	c. 1700	1st destruction level at Knossos (Evans).
	{	c. 1700-1660	MIDDLE MINOAN IIIa
	{	c. 1660	1st destruction level at Mallia (Chapouthier). Phaistos (Banti). Minor earthquake at Knossos (Evans).
LINEAR A	{	c. 1660-1580	MIDDLE MINOAN IIIb
	{	c. 1600	2nd destruction level at Knossos only.
	{	c. 1580-1510	LATE MINOAN Ia Minor earthquake at Knossos (Evans).
	{	c. 1510-1450	LATE MINOAN Ib 2nd destruction level at Phaistos, Agia Triada, Mallia?
	{	c. 1450-1405	LATE MINOAN II (Knossos only). 3rd destruction level at Knossos.
LINEAR B	{	(MAINLAND	
	{	c. 1405-1340	LATE HELLADIC IIIa
	{	c. 1340-1200	LATE HELLADIC IIIb Destruction of Pylos and Mycenae sites outside citadel.
	{	c. 1200-1100	LATE HELLADIC IIIc Destruction level at Mycenae (citadel).

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