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MARGINAL NOTES ON THE MINOAN LINEAR B

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The decipherment of the Minoan Linear B by VENTRIS was greeted with general, enthusiastic approval. Mr BARNETT called this achievement »the Everest of Greek Archaeology», Mr DÜRING, »the Humanistic Atomic Bomb». The first, critical and strongly negative appreciation has recently been published by Mr BEATTIE.

VENTRIS was not the first scholar to claim that the language of the Linear B writings is Greek. His approach to the problem, however, followed other lines than those of most of his predecessors. Statistical investigation of the Minoan material proved that 1) all the Linear B texts are written in one and the same language, and 2) this language is not the same as that of the Linear A texts (cf. DOW p. 115 ff.). This statistical evidence, of course, does not exclude the possibility that some text may contain words in another language. According to DOW ca. 13 nouns common to A and B have been registered. Mr BEATTIE in his critical study says that VENTRIS based his theory on historical and archaelogical grounds. As far as I have been able to follow his method, I have understood that he founded his hypothesis first of all on the structure of the B language itself, which seemed to fit in exactly with the pattern of Greek. VENTRIS had for a long time considered the language to be non-Greek, some kind of Etruscan.

The older view that Minoan culture as a whole was absolutely non-Greek has been shaken mainly by Mr WACE and Mr BLEGEN. The more knowledge acquired by archaeology of the Late Minoan culture the more evidence there seems to be for the theory that the bearers of this culture in Knossos as well as in the Mainland sites were Greeks (cf. DOW p. 119). The »Minoan», »Pelasgian» or »Aegean» language, which is regarded as the source of so many Greek cultural words, was obviously non-Greek. It is thus of the greatest importance when statistical counts show that the difference between Linears A and B depends not only on a development of the script but also on there being two languages phonetically, grammatically and lexically different.

Mr BEATTIE says somewhat obscurely that the language of Linear B »is as likely to be Greek as anything else, although I maintain that there are other possibilities». But if, as now seems to be proved, Linears A and B really represent two different languages, it is easier to believe that the latter is Greek than that there has been two totally different pre-Greek languages, both equally unknown to us.

According to Mr BEATTIE, too, it is quite plausible, that, as VENTRIS claimed, Linear B is a syllabary, each sign representing a pure vowel or a sequence consonant + vowel. The total number of sings seems to be 88. The 12 consonants identified by VENTRIS combined with his 5 vowels give only 60 syllables, to which must be added the 5 pure vowels. What, then, are the 23 remaining signs? And are the varying forms of a given sign only graphical or did they express some difference of pronunciation? The late Professor A. W. PERSSON once pointed out that there might be some small additional marks in the signs indicating a phonetic value, like the *r*-stroke of the Hittite hieroglyphs.

It was in any case possible to write Greek with the ca. 50 signs of the Cypriot syllabary, which had the same structure as that suggested by VENTRIS as underlying the Minoan characters (cf. BEATTIE p. 5).

In their main paper VENTRIS and CHADWICK state (p. 86) that their deciphering is based on a statistical count of the whole material, which has given them information about the frequencies of the various signs and their positions (initial, middle, final) in the groups and about the frequencies of the varions groups. Mr BEATTIE regards (p. 8) this statement as totally untrue: »the amount of calculation involved is inconsiderable». In my opinion, the statistical work presented by VENTRIS in his Work Notes alone is very considerable. But, again, VENTRIS is not the only statistician: rich statistical material has been published and utilized e.g. by ALICE KOBER, KTISTOPULOS, BENNETT, HENLE etc. (see DOW pp. 81 ff., 97 ff.). Mr BEATTIE ought to prove that the results of VENTRIS and CHADWICK are incongruent with the statistical facts revealed by these other researchers. When he then continues (p. 9): »In terms of statistics and calculable probabilities Mr Ventris' hypothesis has very little to commend it», he is referring to the statement by VENTRIS (p. 94) »coincidence seems insufficient to account for the exceptionally long name *E-te-wo-ke-re-we-i-jo*, which on values and orthography determined beforehand (and out of 200 billion possible permutations of syllables in an eight-sign word) so exactly yields the patronymic *'Ετεϝοκλεϝήϊος*». As to the probability of a coincidence just in this case Mr BEATTIE writes (fn. 3): »But

given 200 billion variations anything might happen». The fact is, however, that the probability of *Etewokereweijo* being a coincidence is (on the base of the 65 identified syllables) $\frac{1}{65 \cdot 64 \cdot 63 \cdot 62 \cdot 61 \cdot 60 \cdot 59}$ or, from the point of view of the mathematical probability, minimal. Another question is of course — and there the mathematics do not say anything — whether we consider this word to be an intelligible Greek word or not. And again, if it be a Greek word, can it have another meaning than that suggested by VENTRIS.

Nor do the »astronomical odds against coincidence» referred to by VENTRIS in connection with the god-list (p. 95) convince Mr BEATTIE: for *a-ta-na-po-ti-ni-ja* these odds are $\frac{1}{65 \cdot 64 \cdot 63 \cdot 62 \cdot 61 \cdot 60 \cdot 59}$, for the whole of the list there are 17 multipliers in the denominator! The question here too is: are these Greek words or not?

According to the statistical count, the Linear B language shows grammatically at least two genders, three cases, and two numbers for adjectives and nouns (VENTRIS p. 86). These results are also considered unreliable by Mr BEATTIE (p. 3): »The resemblances may not extend beyond the mere sounds and in other respects may be accidental. Consider in this light *πόρος, πορθέω, πορθμός, πόρτις*. But again and again, it seems, Mr VENTRIS put two words together in this way and then proceeded to identify the final syllables of each on the hypothesis that one word was a by-form of the other.» This identification, which in principle is totally independent of the language in question, was probably effected through parallels like

ABC, ABD, ABE
XYC, XYD, XYE
PQC, PQD, PQE
etc.,

where there seems to be a strong functional parallelism between the elements AB, XY, PQ etc. on the one side and between C, D, E on the other. The hypothesis that the series AB etc. represents stems and the series C etc. elements of inflection or derivation is thus natural. This very method was used by the Indian grammarians, when they analysed the Sanskrit words and found the stems and endings, a task which Greek and Roman grammarians were never able to carry out. It is further worth noting that Linear A does not show any alterations of endings corresponding to this hypothetic inflection in Linear B.

In my Notes p. 4 ff. I remarked that there is a far-reaching similarity between the methods of breaking real ciphers and the deciphering of unknown writings and languages. The analysis of Linear B by KOBER, VENTRIS, BENNETT etc. has carefully followed the normal methods of decipherment and, consequently, its results should be correct in principle. If the language is assumed to be a known one — e.g. Greek — the further deciphering will follow the same lines as the breaking of a normal substitution cipher. The most important point is, thus, to find the right starting point. VENTRIS (p. 89) isolated an ending, which seemed to be characteristic of men's names and of the masculine names of traders, and which belonged to the supposed *-u*-Column of his grid. He noted that the preceding signs all corresponded to a c o n s o n a n t plus t h e v o w e l *e*, the latter also occurring in the nom. plur. of the same ending. He was thus »irresistibly reminded of Greek *-εύς* plur. *ῆϝες*». In the material there are 18 cases of this type, of which 13 according to Mr BEATTIE are unknown to later Greek.

It is interesting to see that Ventris happened to start from the same type of word as did GEORGE SMITH when deciphering the Cypriot syllabic script. SMITH's main document was a bilinguis written in the Cypriot syllabary and Phoenician. The Phoenician text gave SMITH the idea of looking for the translations of the words *melek*, *Milkyaton*, *Kition* and *Idalion*. The number of signs (ca. 55) suggested the syllabic character of the script, and this hypothesis was strengthened by the fact that in the stems of the hypothetical place names *Idalion* and *Kition* no common final *-i-* was found: it had thus to be inherent in the preceding consonant sign. SMITH identified these names through a common ending, which he interpreted as the genetive suffix. The name *Milkyaton* (*Mi-li-ki-ja-to-ne*) was easily identified through being longer than any other word in the text. It showed the same sign for *l* (thus *li*) as *Idalion*. There was, further, no difficulty in finding the word corresponding to the Phoenician *melek*, since it was the only word occurring twice. On its wo appearances this word had a different penultimate sign. From their position in the text SMITH concluded that the first was a nominative, the second a genitive case. He then asked himself in which language the penultimate letters in these cases of the word for 'King' are different, and found Greek *βασιλεύς* ~ *βασιλέως*: the language, consequently, must be Greek. This conclusion made on the basis of a superficial impression and of very little material, and furthermore, by a person who in fact knew very little Greek, was totally correct. SMITH himself was able to identify only 18 signs.

This work was later subjected to repeated corrections, and even to day there are certain unexplained details in the texts.

The difficulties encountered in interpreting the Cypriot texts rose from the very inadequate way in which the signs render the Greek phonemes. This parallel is perhaps worth bearing in mind when reading Mr BEATTIE's severe criticism of VENTRIS' Greek readings (p. 6): »Mr Ventris' version of Linear B is inadequate for writing Greek; it lacks the symmetry natural both to speech-sounds and to the conventions of writing; and it does not represent the outstanding characteristics of Greek pronunciation in Mycenean times», and further: »The rule on which this decipherment chiefly depends is that which obliterates the second consonant of every closed syllable ending with *m*, *n*, *r*(*l*), *s*, *y* . . . . Greek cannot be written in this way; or, if it were, it could not be read». In the Cypriot syllabary differences between *d*, *t*, *th*, and *b*, *p*, *ph*, or *g*, *k*, *kh* are not noted, the length of the vowels *e* and *o* is not expressed, and in closed syllables ending with *n* this *n* is omitted. The main difference between the orthographic principles of the Cypriot syllabary and those of the Cretan Linear B as suggested by VENTRIS is the latter's omission of the consonants mentioned in closed syllables, while the Cypriot script here employs syllabic signs with mute vowels: *a-ra-ku-ro* *ἀργύρω* 'of the silver'. The result is as noted by Mr BEATTIE, that according to VENTRIS' system the word *da-ma-te* might be read *Δαμάτηρ*, as VENTRIS read it, or *δάμαρτες* 'wives', or *δάσματε* 'portions', as Mr BEATTIE sarcastically suggests. A similar uncertainty is met with in the Greek of the Cypriot script, as Mr FRIEDRICH notes: a written *a-to-ro-po-se* can be read *ἄνθρωπος* or *ἄτροπος* or *ἄτροφος* or *ἄδορπος*: can Greek be written and read in this way?

Supposing the Linear B language to be Greek, it must in any case be written very unsatisfactorily, even more so than the Cypriot Greek, since the average length of the words itself shows that there hardly can be enough syllabic characters with mute vowels to render the consonant groups. There seems further to be no sign (like the *se* in the Cypriot orthography) expressing the final *-s*, which is so common in Greek that it ought to be statistically very easy to recognize (cf. VENTRIS p. 89). That the Cretan syllabary must be more clumsy than the Cypriot in rendering the Greek phonemes seems explicable a priori by its greater age.

Mr BEATTIE notes further: »There can be no appeal in this matter to scripts which are used to represent languages of a different structure from Greek». The Minoan syllabary as well as the Cypriot have, however, originally been

created to write a language structurally different from Greek (cf. Dow p. 117). But I can hardly imagine that the orthography of the original languages (viz. »Eteocretan» and »Eteocyprian») could have been much more precise. The question is whether we can find in Greek loan words such phonetical peculiarities as would explain the defectiveness of the orthography postulated by VENTRIS, e.g. the homophonity of *l* and *r*.

If there e.g. was no spirant *h* in the pre-Greek language, the Cretans of course had no sign for it and the Greek spirant, too, was left unwritten (c.f. the sharp notes of Mr BEATTIE). In the Cypriot syllabary, too, no sign was used to express the aspiration. Does this depend on a psilosis in Cypriot Greek or on the nonexistence of such a sound in Eteocyprian? Do the pre-Greek loanwords in Greek reveal anything about the spirant in the language from which the loan was made? Or are the consonant clusters of the loan words of a more simple structure than those of the inherited Greek words? Perhaps it would be possible to regard such peculiarities in later Cretan Greek as e.g. $\varkappa = \varkappa$ and χ, $\pi = \pi$ and φ, *πατεδ δοει* = *πατὴρ δώηι*, *μαιτυς* = *μάρτυς* etc. in the Gortynian Laws as reflecting phonetical trends in the speech of the pre-Greek population? The pre-Greek substrate might well have influenced the Greek dialect of Cyprus also.

During their work on the Linear B texts VENTRIS and his collaborators were compelled to accept forms and words which, from the point of view of classical Greek, look very odd. We can see that the clerks made errors in adding fractions (Dow p. 123) and we may be sure that they made orthographical errors too. But naturally no decipherment can ever be based on a supposed error in the text. Irregularities and clerical errors can be accepted only after the context has been thoroughly explained and understood. There are in the Cypriot Greek texts several irregular forms and words, which are not known from other sources, e.g. *ʾΕδάλιον* pro *ʾΙδάλιον*, *Κετιῆϝεσ* though in general *Κίτιον*, *κατήϝοργον* instead of *κατήϝαργον*, Gen.Sg. *Φιλοκύπρων*, Acc.Sg. *ἰϳατῆραν* = Hom. *ἰητῆρα*, Pf. *ἰκμαμενος*, an unknown word, etc., (cf. e.g. HEIKEL's commentary in his Griechische Inschriften sprachlich erklärt, Helsingfors 1924, p. 116 ff.).

The inaccurate orthography resulting from VENTRIS' decipherment destroys indeed, as Mr BEATTIE notes, essential syntactical features of Greek. The fact that the texts are evidently store book-keeping lists or receipts of taxes and other deliveries means that, no matter what language they are written in, no highly developed syntactical structure is needed, as anyone may see

from his own laundry bill (cf. VENTRIS pp. 90 and 99). A longer, literary text in Linear B would certainly be an excellent touchstone for the decipherment, since the readings would then have to comply with the demands of Greek syntax.

VENTRIS was in my opinion partially right, when stating »If the tablets are written in Greek, they can hardly be explained otherwise than we have proposed; but if they are not, their language is probably in the existing circumstances unknowable» (Antiquity 27, 1953, p. 206). I assume that, although VENTRIS seems to be right in principle, many details of his interpretations will change, when the texts are reexamined again and again, as was the case with the Cypriot syllabary texts, before a satisfactory — though not yet complete — interpretation was attained. I would in this connection touch on some details mentioned by Mr BEATTIE in his criticism.

The quasi-bilinguis Py 641, which was found a f t e r VENTRIS had formulated his decipherment and was used by BENNETT to test VENTRIS' results, does not at all convince Mr BEATTIE, who totally rejects the given interpretation. His own explanation of the text remains, however, somewhat obscure. In Ventris' readings there occur certain very puzzling coincidences, e.g. that the decryptment shows a dual suffix when the number in question happens to be 2. Since Nestor according to Il. 11, 632 has a *δέπας* with *οὔατα τέσσαρα*, I see no objection of the reading *di-pa qe-to-ro-we*. The element *qe* (BEATTIE p. 17) seems to be confirmed by its occurrence in the Mycenaean and Pylian tablets as an affix connecting two probable proper names like the latin *-que*. The form *qe-to-ro-po-pi* is interpreted by VENTRIS *τετραπόδφι*, which Mr BEATTIE calls »a monstrosity»: in any case, it corresponds exactly to Sanskrit *catuṣpadbhiḥ!* It seems very commonplace to find the same thing expressed both by the written text and the ideogramms in this tablet. But on the oil jar in the Mycenaean Oil Merchant's House we read *e-ra ka-ta-ro* = *ἔλαι (Fον) καθαρόν*: what else do you expect to find in the house of an oil merchant?

Mr BEATTIE quotes further an instance of the Knossian chariot tablets: *a-ra-ru-ja a-ni-ja-pi wi-ri-ni-jo o-po-qo ke-ra-ja-pi o-pi-ja-pi / i-qi-jo a-ja-me-no e-re-pa-te a-ra-ro-mo-te-me-no po-ni-*(. . .) *CHARIOT*2, and states »Not even Mr. Ventris' ingenuity will turn this into Greek». The readings of VENTRIS, however, give us *i-qi-ja* etc. in *-a* and not *-o*. The line beginning with *i-qi-ja* — which is inscribed in larger characters as introducing the whole tablet — has obviously been written first, and we must translate the text in the order followed by VENTRIS p. 100. His translations give us just those details we may

expect to find in these texts, though none of them can be completely interpreted. The form of the entry *i-qi-ja* etc. seems to be independent of the number of the chariots: this is fully in accordance with VENTRIS theory that Sg. -*α*, Du. -*ᾱ* and Pl. -*αι* are all written with -*α*.

Thus, though I cannot subscribe to many points in Mr BEATTIE's criticism, we must certainly acknowledge the value of his article in so far as it compels the decipherers to reexamine in detail their methods and results. If the language is Greek, many of these results will prove to be correct. If, again, the language is not Greek, it is miraculous that a fundamentally false interpretation should have given so many plausible results. In this case we would be dealing with an unknown language written in an unknown script (cf. my Notes p. 22 f., FRIEDRICH pp. 75 ff. and 123 ff.). The only possible starting point for decryptment would then be the identification of the place names: Knossos, Pylos, Mycenae etc., just as the deciphering of the Hittite hieroglyphs started from the names *Kar-ka-me* 'Karkemish', *A-ma-tu* 'Ḥamāt' etc. This would give us information about the phonetical values of the signs, and the grammatical forms and the meanings of the words could then be gained from the ideograms, numbers etc. But what are the consequences, if VENTRIS has already correctly identified these place names?

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