

MR. VENTRIS' DECIPHERMENT OF THE MINOAN LINEAR B SCRIPT

DOCUMENTS in the script known as 'Minoan Linear B' were unearthed at Knossos in Crete over fifty years ago. About the same time a few examples of the same script were found at Thebes and other places in mainland Greece. But it was not till 1939 that tablets like those from Knossos came to light in Mainland Greece, near the Messenian Pylos, and not till after the Second World War that they appeared at Mycenae itself.

Before the Pylian discoveries, European scholars had made several attempts to read the script; but their conclusions neither persuaded classical scholars nor wakened public interest. In this matter the discoverer of the Knossian documents, Sir Arthur Evans, seems to have imposed his own restraint on others. The acquisition of what may well be the household accounts of King Nestor created a new enthusiasm for the problem; as soon as the war ended, journals on both sides of the Atlantic began to print essays by various writers who hoped to decipher parts of the script. Mr. Michael Ventris was among these writers; and from the outset his methods were bolder and more resolute than those of the others. By 1951 his tentative decipherment of the Linear B script was being circulated privately; by 1952 he was explaining it in lectures addressed to learned societies; and a year later, in collaboration with Mr. John Chadwick, he published a full account of his solution in the *Journal of Hellenic Studies* (LXXIII (1953), pp. 84-103).

Mr. Ventris' claims are as follows: (1) The language of all the Linear B writings is Greek, and that of a pre-Dorian kind allied to classical Arcadian and Cyprian. (2) The script is in the main a syllabary, akin to the classical Cyprian syllabary. (3) By studying the way in which the syllabic signs are used (their frequency, position in the word, combination of one sign with another, etc.), and by inferring the content of the documents from certain signs which are not syllabic but ideographic, it is possible to discover the phonetic value of most of the syllabic signs. Mr. Ventris describes how he carried out the work of decipherment and produces phonetic values for most of the signs and also rules of orthography; and finally, he shows how his conclusions can be applied to various documents from Knossos and Pylos.

Mr. Ventris' theory has had extraordinary success. So far there have been few expressions of disagreement, and no journal has yet published a critical examination of the case. This is surprising enough: for the first statement of such a theory is unlikely to prove correct all along the line. It would seem natural that after the decipherment had been applied to a wider range of documents, a number of details should need modification. In fact, however, the original phonetic values published in the 1953 article are still virtually unchanged; and, in general, the amount of Minoan Greek that can be read and understood is still substantially the same as that which was announced in the first instance. This is the crux of the matter: a few documents can be interpreted, but a great many are incomprehensible. This situation inevitably suggests not merely flaws in detail but some degree of fundamental error. And therefore it is necessary to test Mr. Ventris' theory thoroughly.

I need not describe the Linear B documents in detail here: a full account, with illustrations, is to be found in Mr. Ventris' article. Most of the texts are written on small tablets of unbaked clay. The writer used a fine stylus; he first ruled a series of parallel lines on the wet surface of the clay, and then he wrote from left to right between the lines and from top to bottom of the tablet. Short groups of signs, evidently words, were divided from each other by a short vertical stroke. Most, or all, of the tablets seem to contain lists of people, animals, goods, and the like. The writing consists partly of words, which sometimes occur singly or in short sequences, and sometimes in longer sequences, amounting perhaps to continuous prose. But it consists also of ideograms, representing commodities, quantities, values, etc., and of numerals. Wherever Linear B script is found, the signs are virtually the same in number and form, and words, both singly and in groups, recur in one place and another. Hence it seems that there is one language in use in all localities. Conversely, there is no indication of a second language in any locality.

So far all is agreed. And when Mr. Ventris proceeds to argue, on historical and archaeological grounds, that the language of the tablets is Greek, it is impossible to refute him. Many scholars in the past have held this view, and many still hold it. I, too, am ready to admit that the language is as likely to be Greek as anything else, although I maintain that there are other possibilities. The question that concerns me now is whether, given that the tablets may be written in Greek, Mr. Ventris' decipherment is correct.

Arguing from the number of different signs used to form words (80-90) and from the nature of oriental scripts roughly contemporary with Linear B and also from the later Cyprian script, Mr. Ventris claims that Linear B is a syllabary and that each syllabic sign is of the type *TA* (in which *T*

represents any consonant and *A* any vowel) or of the type *A* (representing any vowel).¹ This, too, we may accept as entirely plausible.

We are now on the threshold of decipherment. Mr. Ventris says that he divided the signs into categories—first of all, those that were in common use, or were used rarely, or were of average frequency; secondly, as they were found at the beginning of words or at the end; and thirdly, as they appeared to occur in special combinations or to alternate with each other at the end of words and in other positions. These calculations suggested to him, amongst other things, the distinction between pure vowels and other signs; for the signs for pure vowels should occur mostly at the beginning of words and rarely in the middle, except as the second part of a diphthong. The figures also suggested certain features of the Greek inflectional system, including number, gender, and case; for many words seemed to show several variations of the final sign in different contexts. Thus early, even although no sign had been given a precise sound-value, the grammatical framework of the Greek language began to show itself.

Apart from the question of frequencies and alternations, Mr. Ventris also set himself to observe how a particular word, or even a group of words, recurred (often with modifications) alongside a particular ideogram or numeral. From these observations he tried to infer the nature of the context and so to determine whether certain words might represent personal names, place-names, occupations, or the like. On the other hand, when he found that a given word recurred again and again in contexts seemingly unconnected with each other, he decided that it probably belonged to a more general kind of vocabulary.

The next part of Mr. Ventris' work was of a complex nature. He tells us that he first constructed a grid or table, on which he arranged the syllabic signs lengthwise and crosswise according to the alternations which he had already observed, and that, secondly, he proceeded to examine particular words and word-endings which seemed to offer some scope for conjecture. So, by trial and error, giving experimental values to certain signs and testing each of them against the grid, he gradually identified a number of the signs. That is to say if he thought that a word might contain the suffix *-oio* or that it might correspond to Grk. *πατήρ*, he would apply the values *-yo*, *pa-*, *te-*, etc., to certain signs; and he would try to find out whether these values made plausible words or parts of words in other contexts; and he would control the whole operation by ensuring that the frequency of the signs concerned under varying conditions corresponded to that of the syllable concerned in later Greek.

Now all these experiments involved a high degree of conjecture—much higher than Mr. Ventris seems to have realised. And we have not yet considered all the factors: for, even while he was assigning sound-values to the signs, Mr. Ventris was also formulating the rules of orthography, which would not only determine the interpretation of the documents in general but would also pinpoint variant spellings, variant word-forms, and phonetic trends. This, too, involved much guess-work and many arbitrary decisions. For the present, however, I leave this important matter and consider first the use of the grid.

In his article Mr. Ventris shows us a table of comparisons, consisting of pairs and series of words that are identical in all syllables but one, or nearly so identical. Unfortunately this table is printed in Roman script, not in the original signs of Linear B, and the sounds are already divided into *Vowels* and *Consonants* and fully classified. We are not given the original grid, without sound-values, and we are not shown all the stages of identification. Mr. Ventris assures us that the grid was completed before the equivalents were added, but he does not give us the figures from which he made the table. He does not say how often *ke* or *pa* occur in various positions, or how often each alternates with *ki*, *ka*, *ko* or *pe*, *pi*, *po*, or how often each occurs before or after other signs. Let us suppose that he used *all* the texts available to him, and that he counted every single sign in initial, medial and final positions, and so obtained three figures and an overall total for each sign, as well as an assortment of information about alternatives or concomitants. Are we then to suppose that these figures fell naturally into groups, so that the signs to which they referred could be disposed lengthwise and crosswise in such a way that they would ultimately be found to correspond to series of the type, *i*, *pi*, *ti*, *ki*, etc. and *pa*, *pe*, *pi*, etc.? This is evidently what Mr. Ventris means us to believe. Yet, if we bear in mind that the tablets are relatively few in number and in many cases fragmentary, that they deal with a very small range of subjects, that the words in them are often tabulated in lists, and not used in ordinary syntactical combinations, and that some words are repeated again and again in identical or similar contexts, it appears very unlikely that either the words or the syllables in them could give a fair representation of the language to which they belong. On the other hand, if we consider that mere enumeration of *syllables* (as opposed to individual speech-sounds) must give a flat, undifferentiated image of the sound-pattern of any language, we must infer that Mr. Ventris has taken considerable risks in constructing his table. Most striking of all, however, is Mr. Ventris' assumption that, if two words begin with the same succession of signs

¹ The symbols *T* and *A* will be used from time to time in the argument that follows to represent, respectively, any consonant and any vowel. They may also be used in conjunction

with letters; e.g. *Ta* = a syllable consisting of any consonant plus the vowel *a*.

(two, three, or more in number) and differ only in the last sign, these two words are necessarily or even probably connected with each other in structure and sense. In the case of short words, especially, such an assumption is wholly unjustified; the resemblances may not extend beyond the mere sounds and in other respects may be accidental. Consider in this light πόρος, πορθέω, πορθμός, πόρτις; πότερος, ποτήριον, ποτάομαι; τρίβω, τρισί, τριχός; etc. 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. We should be content to accept both his hypotheses and his conclusions if his table were found when complete to consist exclusively of Greek words containing prefixes, roots, suffices, and endings that alternated in Greek style. A brief scrutiny of the table shows that this is not the case; many of Mr. Ventris' comparisons lie in the realm of conjecture—as much now, after decipherment is complete, as at the outset.

Consequently I regard the table of comparisons and the grid with strong suspicion. But now I pass to the evaluation of the signs.

Mr. Ventris noted first of all five signs which occurred more commonly at the beginning of words than elsewhere and identified them collectively with the five pure vowels of Greek. Next he observed that the rarest of these five vowel-signs sometimes occurred also at the end of words. When this sign was final, it was, on the one hand, preceded by any of thirteen other signs, which were all of about the same frequency, and on the other hand it was sometimes replaced by a particular *one* of these thirteen signs. His interpretation of these facts is as follows. The vowel-sign which occurs initially and finally is *u*. The thirteen signs which precede *-u* when it is final form the series *Te*. And the particular sign that sometimes replaces *-u* in these words is *-we*. Now, if it is assumed that final *-s* is never written, the conclusion is that all the words concerned belong to the Greek *-ηf-* declension, showing nom. sing. *-εύς* and plural *-ήρες*. Furthermore, since these same words sometimes appear with yet another final sign, this sign can be interpreted as a further case-form of the declension, to wit *-wo*; thus we identify the genitive *-ήρος* or *-ήρων*. For Mr. Ventris decides that not only final *-ς* but final *-v*, and also *-ρ* and post vocalic *-ι*, should be left out.

Thus by a single experiment, Mr. Ventris settled the value of the sign for *-u*, the entire series of thirteen signs for *Te* (though not of individual signs within that series, except *we*), and also one member of the series *To*. His grid then indicated to him all the other members of this *o-* series, and all that remained under this head was to identify each sign in the series through trial and error.

This procedure is so bold that we must consider it with some care. I have described it much as Mr. Ventris himself describes it, although more simply and more briefly. But it can be put more simply still. What Mr. Ventris is dealing with here is a list of words, each of which admits three variant signs at the end. One of these three variants is a sign that is otherwise rare at the beginning of words. Another of the three is one of a group of thirteen signs that form the penultimate sign in all the words. On this evidence, and nothing more, Mr. Ventris identifies the *-εύς* declension. I shall consider the validity of this identification later on; for the present it is enough to note the extreme paucity of the evidence. For Mr. Ventris does not say how many words the list contains, or how often each word occurs, or what circumstances suggested to him that these words should be *-εύς* nouns rather than something else.

In the meantime I continue to follow his account of the decipherment. Having found *-wo*, Mr. Ventris fixed his attention on another sign in the same column of the grid. This sign was often final, and it occurred at the end of words which the context suggested might be in genitive relationship to neighbouring words. So Mr. Ventris decided that it signified *-yo*, and that it often represented the last element of the genitive ending *-ιο*. Sometimes, however, this same sign belonged to the *-yo* suffix of adjectives (final *-ς*, *-v*, and *-ι* being omitted, according to rule), and thus, by further consideration of the grid, it became possible to pick out *-ya*, the feminine counterpart of adjectival *-yo*. From this point Mr. Ventris moved on, guided by various contexts, to identify adjectives ending in *-ιος* (*-i-yo*) and so to pick out the whole series *Ti* and to fix the pure vowel *i* itself. And so on. Before long he had recognised each of the five pure vowels and every sign in the series *wA* and *ya*. All the other signs had been classified according to their vocalic element, and it remained only to fix precisely the consonantal value of each.

This final phase involved a good deal of experimentation. For example, Mr. Ventris conjectured that a group of tablets bearing ideograms which are thought to represent severally 'men', 'women', 'children' contained a reference to the concepts 'father' and 'mother'. He then took two words, each of two signs, that recurred in these tablets and assigned to them the value *pa-te(r)*, *ma-te(r)*. These identifications started the process of delimiting the *p*, *m*, and *t* series of consonants. Again, two categories often mentioned in Cretan tablets were guessed to be boys and girls, represented by *ko-wo*, *ko-wa*, *i.e.* κόρφοι, κόρφα; and two terms which recurred in connection with these categories were identified as *me-zo*, *me-u-jo*, *i.e.* μέζων, μείων. And thus the *m* series was carried a stage farther. Yet again, the total figure at the end of a list of numbered commodities sometimes has alongside it a word that might signify 'total'. This word, with its variable ending,

was interpreted as *to-so-i(-de)*, *to-sa(-de)*; and so the *t* series received another item, and the *s* and *d* series began to come into line.

By this time there were few common signs that did not have a phonetic value set alongside them. In his *JHS* article Mr. Ventris was able to offer an interpretation of no less than sixty-five syllabic signs.

As the phonetic evaluation of the signs advanced, the rules of orthography became more definite. At an early stage, however, the decipherer found himself compelled to allow for variant spellings and even for misspellings and grammatical faults. He postulates the existence of two signs of roughly the same value (a_1 , a_2), or the occasional omission of a *y*, *w* glide, or even a grammatical mistake. In some of these matters Mr. Ventris plainly goes too far; it is impossible to correct a Mycenaean writer's spelling, still less his grammar, before you are sure of the rules which he observed and also of the phonetic value of his script.

I mention only the more important of Mr. Ventris' spelling laws. They are as follows:

- (a) The five vowels written are *a*, *e*, *i*, *o*, *u*. No distinction is made between long and short vowels, or between long and short diphthongs.
- (b) Diphthongs with *-u* are shown; diphthongs with *-i* are not normally shown—except initial *ai-*, and *ei*, *oi*, *ai* before final *-s*. Intervocalic *i* is shown by the series *yA*.
- (c) The continuant consonants are *m*, *n*, *r*, *s*, and *z*. Greek *l* is shown as *r*. The continuants *m*, *n*, *r*, *s* are omitted at the end of a syllable, whether medial or final.
- (d) The stop consonants are *p*, *t*, *k*, labiovelar *q*, and *d*. Of these *p*, *k*, *q* are voiceless, voiced, or aspirate; *t* is voiceless or aspirate; *d* is the only representation of a voiced stop sound. There is no doubling of consonants.
- (e) Consonantal groups of *stop + stop*, *stop + continuant* are indicated by two signs, each of which has the vowel quality of the vowel that follows the group.
- (f) When *s + stop* occurs initially, the *s* is omitted.

Armed with his table of phonetic values and with his rules of orthography, Mr. Ventris devotes the rest of his article to the discussion of various types of document, grammatical categories, syntactical combinations, dialectal peculiarities, and so forth. Here are some of his results.

Tablets from Cnossos contain the words *Ko-no-so*, *Ko-no-si-jo*, *Ko-no-si-ja* and also *A-mi-ni-si-jo*, *A-mi-ni-si-ja*, and Pylian tablets often have *Pu-ro* at the top. This is at first sight encouraging. But of course we do not know whether Mr. Ventris used these particular words in the first instance to establish the value of one sign or another. Consequently it is uncertain whether his transcription of any word confirms the identification of any or all of the signs in that word or is merely the first guess by which one or all of these signs were identified. At any rate, no tablet has yet appeared in which the interpretation of these words as place-names is confirmed beyond reasonable doubt by the context.

We go farther and find phrases that make sense within the prescribed rules and are in harmony with an ideogram. For example, if *to-sa pa-ka-na* is followed by a sign representing a sword with a numeral beside it, then *τόσσα φάσγανα* seems a reasonable transcription. Nevertheless, the rules of orthography admit other interpretations, and one wonders why both the word for 'sword' and the ideogram appear. The coincidence is the more impressive if the phrase is longer. Thus *Pu-ro i-je-re-ja do-e-ra e-ne-ka ku-ru-so-jo i-je-ro-jo* followed by the ideogram for woman and a figure 13 may be rendered Πύλος· ἱερείας δοέλαι ἴνεκα χρυσοῖο ἱεροῖο. WOMEN 13. On reflection it may be seen that ἴνεκα is in the wrong place and bears an unusual sense,² that the form *do-e-ra*, δοέλαι is unsubstantiated, that *i-je-re-ja* is a false form for *i-je-re-wi-ja*, and that the phrase as a whole has no meaning that is both obvious and plausible. In relation to the Mycenaean civilisation, the sale of slave-women for 'sacred gold' is pure fantasy. Yet the general aspect of the transliteration is undoubtedly Hellenic; and if all else were confirmed, we might not raise any strong objection to this particular case.

Again, when we find a text consisting of (line 1) *a-ta-na-po-ti-ni-ja-* (line 2) *e-nu-wa-ri-jo pa-ja-wo po-se-da*], we may well feel a thrill of excitement on recognising Ἄθάνα Πότνια Ἐνυφάλιος Παϊάφων Ποσειδά[ρων. The thrill vanishes, however, and disappointment succeeds it, when we consider that this is a most implausible quartet of gods; that Ἄθάνα Πότνια could hardly be written as one word and would almost certainly appear in the order Πότνια Ἄθάνα; that Enyalios as a cult-name is not likely to be earlier than Homer, for whom it is a conventional epithet; that Paian is properly an invocation and only by poetic artifice a name; and that *po-se-da* is an incomplete word—the only comparable word in Linear B is *po-se-da-o* and not *po-se-da-wo*. The words on the tablet are only the left-hand portion of a text which one would expect to be, like its fellows, an account of men and goods. It would be more than venturesome to base a theory on this fragment.

² Mr. Ventris uses the obelisk-sign to mark 'suggested spellings, meanings, and compounds which are not paralleled in classical Greek, or implicit in the accepted etymology'.

He uses a question-mark 'where serious difficulties stand in the way of the meaning or spelling proposed'.

It is indeed a difficulty that the intelligibility of the documents seems to diminish as their length increases. Here is one of several longer texts quoted by Mr. Ventris:

<i>Pa-ki-ja-ni-ja to-sa da-ma-te</i>	DA 40
<i>to-so-de te-re-ta e-ne-e-si</i>	MEN 14
<i>wa-na-ta-jo-jo ko-to-na ki-ti-me-na to-so-de pe-mo</i>	GRAIN 2 $\frac{1}{60}$
<i>o-da-a₂ o-na-te-re e-ko-si wa-na-ta-jo-jo ko-to-na</i>	
<i>a-tu-ko e-te-do-mo wa-na-ka-te-ro o-na-to e-ke de pe-mo</i>	GRAIN $\frac{1}{60}$

He offers the following explanation:

Σφαγιάνια τόσα †Δαμάτηρ	DA 40
τοσσοίδε †τελεσταί ἐνέενσι (= ἐνεισι)	MEN 14
φαρναταίοιο κτοίνας κτίμενα τοσσόνδε σπέρμο	GRAIN 2 $\frac{1}{60}$
? = αὐτὰρ †ὄνατῆρες ἔχονσι φαρναταίοιο κτοίνας	
*Ατυχος †έντεσδόμος †φανάκτερος †ὄνατον ἔχει [τοσσόν]δε σπέρμο	GRAIN $\frac{1}{60}$

Here, among other peculiarities, σπέρμο with final -ο is supposed to be a sign of Arcadian affinities (in token of which *e-ne-e-si*, *e-ko-si* are transliterated ἐνέενσι, ἔχονσι, despite the fact that *n* is not written). But it is doubtful whether even in fourth-century Arcadian neut. -μα would appear as -μο, and in Mycenaean times it is most improbable. Now this sentence, if it is a sentence, has an agricultural flavour throughout; and that is something. But anyone acquainted with the Greek tongue can see that this is not a Greek text. A κτοίνα is not a farm or field, as Mr. Ventris thinks, but a village community (Hesychius' explanation, δῆμος μεμερισμένος, refers to the division of a township into smaller units, *i.e.* villages); and κτίμενος would not, as Mr. Ventris thinks, mean 'established', nor even 'cultivated', but merely 'built', 'constructed'. It is unnecessary to remark that ὄνατῆρες, έντεσδόμος, and φανάκτερος are fictitious or that ἐνέενσι is morphologically unacceptable. The *whole thing* is nonsense from beginning to end.

The farther I follow Mr. Ventris, the more I fear that he has led us off the track. From time to time he tries to reassure me; he says that if we are not on the right road we cannot be far from it, or that we may have diverged slightly but may soon be back on the road, or even that the road is none the worse for not being exactly what we expected it to be. The Greek, he says, departs from the conventions of classical Greek because it is five hundred years earlier than Homer, because it is a dialect with which we are not familiar, because a few of the syllabic signs may have been wrongly evaluated, and so forth. These excuses, by their multiplicity and constant repetition, intensify my doubts. Therefore I go back to the starting-point and explore the road again.

I consider first the general pattern of Mr. Ventris' syllabary. There is nothing improbable in the notion of Greek being written with 80–90 syllables of the type *TA*. The Cyprian script in the classical age shows it is possible to write Greek using fewer syllables and without distinguishing the length of vowels or even the length, voice, and aspiration of consonants. The Greek alphabet itself, although it is fairly exact about consonants, did not show the length of every vowel; and in the beginning it did not distinguish vowel-length at all.

Nevertheless, there must be a limit to the number of phonemic differences that can be left out. Otherwise the script will become too inexact to be of any use. So in Greek, if you do not show separately the five cardinal vowels and both the -i- and -υ- series of diphthongs, you run the gravest risk of being misunderstood. If you do not write -v and -ς and -i, you destroy the syntax of your sentences. If you confuse ρ and λ, you obliterate the distinction between important suffixes and you obscure many roots. If in addition to all these things you omit a variety of medial consonants, you create havoc.

Mr. Ventris' syllabic pattern is really far too simple, and we may say with confidence that it is insufficient for the writing and reading of Greek. It irons out the sound-system of the language. On the other hand, just because it is so imprecise, it enables Mr. Ventris to discern Greek words in groups of syllables that look entirely un-Greek to the classical scholar.

Another fault of Mr. Ventris' syllabary is that it is unsymmetrical. Nature imposes a degree of phonetic symmetry on all languages, and so also on the scripts that are used to represent them. For example, people who distinguish *k* from *g* in writing will generally find it necessary to distinguish *p* from *b*, and any other unvoiced sound they use from its voiced counterpart. Mr. Ventris imagines that the Mycenaean habitually marked -u- diphthongs, yet wrote the no less important and far commoner -i- series as if they were not to be distinguished from the cardinal vowels. But occasionally they would depart from this rule by writing *ai-* initially (though not *Tai-*) and by adding *i* to the cardinal vowel when the *ai-* or *-oi-* diphthong preceded final -s. Such inconsistency borders on the incredible. Again, Mr. Ventris holds that *p*, *k*, *k^w* were not distinguished from *b*, *g*, *g^w* or from *ph*, *kh*, *kh^w*, but that *t* and *d*, *s* and *z* were regularly kept apart. This, too, surpasses belief. Such features are not to be excused on the ground that the Greeks took over a syllabary which had been

εἶχε, or ἔσχε (if indeed he did not have ἐκεῖ, εἶ κε, ἔλκει, or something else in mind). *Pylans*, who were concerned either with *tribes* or *wheat*, would hardly know what to make of *pu-ro*; they might in the end come to the conclusion that it meant an *ankle* (σφυρόν) or (since we are allowed sometimes to count *o* as *a* in Arcadian style) a *hammer* (σφυῖρα).

An obvious objection to this part of Mr. Ventris' case is that many of the ambiguities need not arise if only he allowed the Greeks to make full use of their script. Anyone who wanted to write *κακός* and *χαλκός* according to Mr. Ventris' decipherment would naturally put down, not *ka-ko* twice, but *ka-ko-se* and *ka-ro-ko-se*. This would not be perfect, but it would be a few degrees better than *ka-ko* for both words. Here Mr. Ventris cannot argue that the Greeks inherited a barbarian script, and, with it, the rules of orthography devised for a barbarian tongue. If that had been the case, someone—whether genius or simpleton, or merely a man of good sense—would surely have improved on *ka-ko* and the like. The temptation to write something to indicate an *-λ-* or an *-s* must have proved irresistible.

I pass briefly over minor points of spelling that appear illogical and improbable. For example, we are told that *sk*, etc., are written simply as *k*, etc., but that *ks*, *ps* are shown in full. Although there is a rule that *κτε*, etc., are shown as *ke-te*, etc., we come across striking exceptions. For example, *wa-na-ka-te-ro* is made into *φανάκτερος*, a purely fictitious word. *E-wi-ri-po* is read as *Εὔριπος*, although initial *e-u-* is common and might well have been used here. *Ru-ki-to* is alleged to mean *Λύκτος*, on the analogy of *ru-ki-ti-jo*, *Λύκτιος*; but the analogy would be more likely to work the other way. These things are really unacceptable, and they are serious enough in themselves. They are, however, subordinate to the general criticisms which I have already set forth. The essence of the matter is that Mr. Ventris' rules of spelling have one advantage and only one; they allow him to make something like Greek out of many successions of syllables that would otherwise be thought barbarous. Against this, they have the serious disadvantage that neither Mr. Ventris nor anyone else can ever be sure what is in fact meant.

Mr. Ventris' decipherment started from the identification of suffixes. Provided that the language is Greek or akin to Greek, it would seem reasonable to begin with suffixes. If the first identifications were right, all might be well (but need not be so). If the first few were wrong, the rest would certainly go astray. At all events, if the commonest final syllables were equated with Greek suffixes, some of the texts must look like Greek. And in a three- or four-syllable word, if the last one or two syllables are read as a Greek suffix, there is a fair chance that the one, two, or three syllables before them may correspond to a Greek word-stem or resemble a Greek word-stem. The main question is, whether Mr. Ventris' first suffixes (*-εύς*, *-τήρ* etc.) are correctly identified.

Having fixed the value of *-εύς*, *-ῆφες*, *-ῆφος*, Mr. Ventris was able to identify eighteen nouns of the *-εύς* type, together with some other words of the same form which he calls personal names. Of the eighteen nouns, thirteen are entirely unknown in later Greek; and some are so baffling that Mr. Ventris himself offers no explanation of their form or meaning. The rest—*χαλκεύς*, *ἱερεύς*, *κναφεύς*, *κεραμεύς* and *βασιλεύς*—are all to some extent ambiguous, owing to the confusion of *r* and *l* and of voiced, unvoiced, and aspirated stops and owing to the omission (whether postulated here or not) of continuant consonants before a stop. Not one of them can be regarded as confirmed by the context in which it occurs. There are eleven supposed examples of agent nouns in *-τήρ*, of which two are fragmentary endings without roots (i.e. *-te-re* alone); nine are unattested in Greek and inexplicable, and two are known. The two known words are *ἰατήρ* and *στατήρ*, both to some extent ambiguous in form owing to the omission of *r*, *s* and the confusion of *t* and *th*, and neither confirmed by the context. The corresponding series in *-τρια* or *-τειρα* contains five words, of which one is actually attested (but only in Eustathius), two are not attested themselves but associated with attested masculines in *-τηρ* (both infrequent in later Greek), and two are unknown. And so on. More than half of all the nouns listed by Mr. Ventris occur in his article for the first and only time in the history of the Greek language; others are raked from the pages of Hellenistic authors and supported by references to Hesychius. Some, like *ἐντεσδόμος* 'armourer' and *μελλιδάμαρτες*, presumably 'wives-to-be', are so absurd in both form and meaning that none but an enthusiast could accept them for a moment.

Surely it is clear that Mr. Ventris has gone astray at the very beginning. He took a list of words—not a very long list—with three variant endings; one of these endings occurred sometimes at the beginning of other words, but not very often; another was one of thirteen signs that formed the penultimate sign in all the words in the list. On this evidence alone Mr. Ventris identified his *-εύς* declension, the vowel *u*, *we*, *wo*, and twelve signs in the *e*-series. It would have been a miracle if he had been right. That he was *not* right, the thirteen unattested words among his *-εύς* nouns clearly show. It is not hard to think of many other combinations of sounds that might have suited the conditions: e.g. *-ξ*, *-κες*, *-κας*, and the series *Tā* in the penultimate syllable.

If Mr. Ventris could proceed from his eighteen *-εύς* nouns and give us a list of words beginning with *εὐ-*, he might alleviate, although he would not remove, our doubts about his original discovery. In fact, he does claim to have found ten personal names with initial *Εὐ-* and two with initial *Εὔρυ-*. One must remember, of course, that the sign *e* as such did not occur among the *-εύς* nouns but was

separately identified. It is not therefore a case of initial *e-u-* confirming final *-e-u*; we are concerned with initial *e-u* and final *Te-u*, and *u* is the only common factor. The *εϋ-* list, however, is at first sight impressive. Unhappily two of the ten *Εϋ-* names violate Mr. Ventris' own rules of orthography, and one of the two *Εϋϋ-* names depends on the 'Arcadian' trick of *o* for *a*. Some of the other names are only legible at all through the latitude provided by Mr. Ventris' rules of spelling. Some, on the other hand, admit an embarrassing number of interpretations; for what is the use of writing *e-u-ρo-ro* for *Εϋπορος*, if you cannot then distinguish this *Euporos* from *Εϋφορος*, *Εϋβορος*, *Εϋβολος*, *Εϋβωλος*, *Εϋφρων* (and *Εϋφρώ*), or from other persons whose names might be written in exactly the same way? There is, however, one vital objection that can be laid against all Mr. Ventris' personal names; that the contexts in which the names occur give no reliable indication that these are personal names at all. In view of this, it may be concluded, from the simple form of each of the names and the marked frequency of the signs that compose them, that their resemblance to personal names need be no more than mere chance. It is certainly not enough to prop up the rickety framework that underlies Mr. Ventris' *-εϋς* declension. And belief in the *Εϋ-* nouns is severely shaken when in addition to those listed by Mr. Ventris in his article there appear strange forms such as *e-u-qo-ne* and *e-u-de-to-ge*.

From the *-εϋς* terminations Mr. Ventris moved on to another group, *-ιος*, *-ια*, then to *-τηρ*, *-τρια*, and so on. He kept on building up little collections of words that share one suffix or another. Each collection contains one or two known words within the category in question—generally rather short words, each consisting of two, three, or four signs. And one or other of these short words is, of course, the starting-point of the identification, the primary conjecture. Other words with the same termination are added to the list. Some few of them turn out to be reconcilable with known words or nearly so; others can be declared personal names. The residue are given a hypothetical form and a hypothetical meaning, and are marked with asterisks, obelisks, and question-marks. The reader of Mr. Ventris' article is then asked to accept the entire collection as Greek.

It is clear that throughout this process the element of conjecture is increasing and not, as one might expect, diminishing. There is no confirmation of the *-εϋς* nouns, or of *-ιος* adjectives, or of *-τηρ*, *-τρια* nouns. Each suffix in turn is a card which forms a new partition or ceiling in an ever-growing house of cards. But however big the house may grow, it is still made of cards, delicately balanced one against the other. The identified suffixes lean on each other; they do not *verify* each other. The very large number of unidentified and unexplained words gives warning that the limit of the structure has almost been reached.

What Mr. Ventris has given us by his transcription is not in fact the Greek language but a language of his own making. It is a strange language, which looks like Greek because he has been careful to provide it with a selection of Greek suffixes. Since he has shorn most words of the whole or part of their inflection and some even of part of their suffixal element, he has been able to dispense almost entirely with the requirements of Greek syntax. And by devising spelling-conventions of primitive simplicity, he has ensured that the syllables preceding the suffix of each word may occasionally be intelligible as Greek word-stems. He has given us first of all a specious interpretation of short, often fragmentary, texts, which, because they are fairly plentiful and repeat the same few words over and over again, may be thought by the inexperienced to confirm each other. They do not in fact serve this purpose. Apart from the words and simple phrases which he took as the basis for his first guesses, he has not given us a single text consisting of several words in direct sequence that makes sense from beginning to end as a Greek phrase or sentence. Instead, we get texts like the so-called Demeter text which I have already quoted or the following from Knossos:

a-ra-ru-ja a-ni-ja-pi wi-ri-ni-jo o-po-qo ke-ra-ja-pi o-pi-i-ja-pi CHARIOT (2?)
i-qi-jo a-ja-me-no e-re-pa-te a-ra-ro-mo-te-me-no po-ni-]

Not even Mr. Ventris' ingenuity will turn this into Greek.

Many scholars may have accepted Mr. Ventris' hypothesis, or may have decided not to oppose it, because they are impressed by his references to statistics. He tells us, for example, that 'a very severe discipline' is imposed on the earliest stages of a decipherment by the use of a grid; he even says that if the initial moves are wrong it should be 'quite impossible' to force any part of the texts into showing the slightest conformity with the vocabulary or grammar of a known language. The suggestion that long lists of figures stand guard behind these pronouncements is enough to scare anyone who, like myself, is naturally afraid of arithmetic. In this case, however, we may easily banish our fears. We have seen the early stages of the decipherment. We have found that the amount of calculation involved is inconsiderable and that its results are wholly unconvincing; the early stages of the decipherment were in this respect allowed to run riot. Moreover, we have seen how Mr. Ventris *does* force texts into conformity with the grammar and vocabulary of Greek, and how the texts successfully resist this process. Accordingly, we shall not be easily scared when Mr. Ventris tells us that the correspondence of a list of words with the names of four Greek deities seems 'ensured by astronomical odds against coincidence'. The fact is, as we have seen, that none of these names can be accepted with confidence and that the list as a whole is far from plausible. What

we have here are three words and part of a fourth which happen to *resemble* either the names or the surnames of certain Greek deities. All things considered, it seems likely that a coincidence *has* occurred here. And this seems the more likely when we set the four names of deities against the vast number of texts of which Mr. Ventris can make no sense at all. In terms of statistics and calculable probabilities Mr. Ventris' hypothesis has very little to commend it.³

When all the texts have been transcribed according to Mr. Ventris' table of values, there is hardly any Greek to be seen. What little Greek there is has been put there by Mr. Ventris himself, through his identification of *-e-we*, *-i-jo*, *-me-na*, *-te-re* and *pa-te*, *ma-te*, etc. It would be vain to search among the remaining texts for substantially *more* Greek than has so far been discovered, for the simple reason that Mr. Ventris did not put any more in.

Three years have passed since the publication of Mr. Ventris' article. During that time he and others have often repeated the claims made in that article and have sought to interpret more texts in accordance with the rules set forth in it. The new interpretations are seldom attractive and never convincing; the Appendix to this article contains a detailed refutation of one of the best known among them. It should be stressed that, in spite of all recent work in this field, the 1953 article is still the sole foundation of the decipherment. The passage of months and years may seem to have given authority to Mr. Ventris' theory, and absence of criticism may be thought to have justified all his conclusions. But in fact the theory is no stronger now than it was to begin with. The objections set forth in the preceding pages are, I suggest, of fundamental importance. If Mr. Ventris can show that they are unsound or irrelevant, his theory may be allowed to stand; if not, it must be revised very drastically or rejected altogether.

APPENDIX

When Mr. Ventris offers to interpret one of the longer texts in Linear B script, it is sometimes easy to refute him. For example, he transliterates a word as *ta-ra-nu* and says that it is equivalent to Gk. θράνυς; and he points to an accompanying ideogram which, he says, represents a footstool. The obvious answer to this is that the ideogram really is a drawing of *a flat pan with two handles* and that the text therefore belongs to the well-known group of texts concerned with domestic utensils.⁴ Again, from a tablet found at Mycenae Mr. Ventris produces a list of plant-names referring to condiments and medicines. But three of the names depend upon our disregarding Mr. Ventris' own rules of orthography,⁵ and there are other words which cannot be reconciled with a botanical context. It seems very likely that this list of plant-names is the result of coincidence.

The task of disproving Mr. Ventris' interpretations is often laborious; if we were to examine each of them in detail we should need a great deal of space. Accordingly, I have chosen one text in which the application of Mr. Ventris' values to the syllabic groups seems to give good sense and in which there is a close correspondence between the syllabic groups, on the one hand, and the ideograms and numerals, on the other. This text has probably done more than anything else to convince classical scholars that Mr. Ventris' decipherment is right. If his interpretation of it can be disproved, there is little chance that any of his other interpretations will be able to stand.

The text of the tablet is as follows:

(line 1)	<i>ti-ri-po-de</i>	<i>ai-ke-u</i>	<i>ke-re-si-jo</i>	<i>we-ke</i>		☞
	<i>ti-ri-po</i>	<i>e-me</i>	<i>po-de</i>	<i>o-wo-we</i>		☞
	<i>ti-ri-po</i>	<i>ke-re-si-jo</i>	<i>we-ke</i>	<i>a-pu-ke</i>	<i>ke-re-a₂</i> [
					<i>ka-u-me-no</i> [
(line 2)	<i>qe-to</i>					☉
	<i>di-pa</i>	<i>me-zo-e</i>		<i>qe-to-ro-we</i>		☉
	<i>di-pa-e</i>	<i>me-zo-e</i>		<i>ti-ri-o-we-e</i>		☉
	<i>di-pa</i>	<i>me-wi-jo</i>		<i>qe-to-ro-we</i>		☉
	<i>di-pa</i>	<i>me-wi-jo</i>		<i>ti-ri-jo-we</i>		☉
	<i>di-pa</i>	<i>me-wi-jo</i>		<i>a-no-we</i>		☉

³ Mr. Ventris says: 'coincidence seems insufficient to account for . . . *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 'Ετεφοκλεφής'. But given 200 billion variations, *anything* may happen. Those who take the trouble to decipher the rest of the tablet Sn. 01 will find the context far less impressive than Mr. Ventris indicates.

⁴ The ideogram bears only a superficial resemblance to a footstool in Mycenaean art. The words accompanying *ta-ra-nu* are turned by Mr. Ventris into fanciful descriptions of footstools, which do not endure close scrutiny. The series of texts in which *ta-ra-nu* occurs contains several ideograms of vessels, none of furniture.

⁵ The three offending words are *ka-ra-ko* for *qa-ra-ko* (βληχών, γληχών), *ka-da-mi-ta* for *ka-ra-mi-ta* (καλαμίνθη), and *ma-ra-tu-wo* for *ma-ra-to* (μάραθος). These amount to a third of the list, and are sufficient to discredit the rest. Also in this text are the expressions *ka-na-ko e-ru-ta-ra* and *ka-na-ko re-u-ka*, sc. κνᾶκος ἐρυθρά and κ. λευκά. These have been identified with the κνήκος, *Carthamus tinctorius*, and κ. ἄγρια, *C. leucocaulos*, of post-classical Greek. Note, however, (i) that the epithets ἐρυθρά, λευκή are not applied to κνήκος by Greek authors; (ii) that *C. tinctorius* has yellow or yellow-brown flowers, and *C. leucocaulos* yellow (not white) flowers in a hood or cup of whitish sepals; (iii) that *C. tinctorius* produces by a complicated process a red dye, *C. leucocaulos* produces no dye at all; and (iv) that neither they nor any other *Carthamus* can be reckoned as condiment or medicament.

Mr. Ventris' interpretation (in its most restrained form) is :

τρίποδε	?	?	?		two tripods, two-handled
τρίπους	?	πόδες?	οἴφωφης		one tripod, two-handled
τρίπους	?	?	?	?	[one tripod, ?-handled]
?				?	three jars, two-handled
δέπας	μέζοε (<i>sic</i>)		τετρῶφες		one pot, four-handled
δέπαε	μέζοε		τριῶφες		two pots, three-handled
δέπας	μέφιον		τετρῶφες		one pot, four-handled
δέπας	μέφιον		τριῶφες		one pot, three-handled
δέπας	μέφιον		ἀνώφες		one pot, without handle

I.e. l. 1 deals with *tripods*, with double or single handles, and the rest with *vessels*—mainly of the δέπας-type, and these large or small, and having four handles, three handles, or none at all. The words for *tripod* and *vessel* appear in the singular and dual inflections.

In this text, eight or nine words appear to be satisfactorily explained. These are: *ti-ri-po*, *di-pa*, *me-zo-e*, *me-wi-jo*, *o-wo-we*, *qe-to-ro-we*, *ti-ri-jo-we*, *a-no-we* and *po-de* (?). If we include the inflected forms *ti-ri-po-de*, *de-pa-e*, *ti-ri-o-we-e*, the number rises to eleven or twelve. And if we add also forms which are simply repeated, the total is nineteen or twenty words. Against this list, we set eight or nine words that cannot be satisfactorily explained. These are: *ai-ke-u*, *ke-re-si-jo*, *we-ke*, *e-me*, *a-pu-ke*, *ke-re-a₂*, *ka-u-me-no*, *qe-to*, and *po-de* (?). If repetitions are included, the number will be ten or eleven. Thus on one reckoning *one-half* of the text is explained and *half* unexplained; and on the other reckoning *two-thirds* are explained and *one-third* unexplained.

It is true that Mr. Ventris and others have interpreted *ai-ke-u* as Αἰγεύς, *ke-re-si-jo* as χρήσιος, Κρήσιος, and *we-ke* as ἔχει, etc., and that, in defiance of the word-division, *ke-re-si-jo we-ke* has been made into Κρησιοφεργής. But none of these suggestions passes above the level of conjecture, and for most of them, even as conjectures, there is little or nothing to be said. Mr. Ventris has on occasion identified *e-me* with Gk. ἡμι-, but this carries no weight; and the equation of *po-de* with πόδες seems to create more problems than it solves. There is no proof that *a-pu-ke* and *ka-u-me-no* form one word, or that, if it did, ἀποκεκαυμένος (Arcadian ἄπυ-) would suit the context. *Ke-re-a₂* can hardly be σκέλεα. Accordingly, we shall not allow these proposals to divert our attention from the fact that at least a third, and perhaps a half, of the entire text *cannot* be read and understood according to Mr. Ventris' decipherment. And this is a large proportion, enough to cause us to examine the half or two-thirds that *can* be understood with great care.

It is well that we should remember the multiplicity of phonetic values attached to each word on Mr. Ventris' hypothesis. It looks at first sight as if this Pylian text could be read straight off without any difficulty; and we might well overlook the fact that the particular interpretation that we are asked to accept is only one possibility out of many. One reason why the transcription looks simple is that ll. 2 and 3 contain few stop consonants, and these few are taken at their obvious value—the unvoiced, unaspirated stop; thus *p* in *di-pa*, *ti-ri-po*, *q* in *qe-to-ro-we*, *t* in *ti-ri-po*, *ti-ri-o-we*, *qe-to-ro-we*. But in fact the values *p*, *t*, *q* are merely symbolic; each stands for a variety of consonants and consonant-groups. And the vowels attached to them are also symbolic, being long or short or (by spelling convention) mute. And even the continuants (*m*, *n*, *r*, *w*, *y*), although they admit fewer variants than the stop consonants, are by no means stable. If we apply these variations to the words in the text we find that *ti-ri-o-we* might be interpreted in 5760 different ways and *qe-to-ro-we* in 92,160 ways. Even so short a word as *di-pa* could mean about 300 different things. If, then, we consider any two or three words together, the number of variants obtained by multiplying together the individual totals for each is truly enormous. And if we took into account *all* the possible values of *all* the words in the text, the final figure might well be described as 'astronomic'.

We are thus reminded that, if any Pylian had written a text according to Mr. Ventris' evaluation of the signs and with his spelling conventions, it would be a miracle if any reader were to recognise what that text meant. I hasten to admit that in the present instance the reader might be able to recognise the nature of the subject-matter from the ideograms and to recognise some of the words, and also, that, as a native speaker of early Greek, he would be able to reject, whether by instinct or by intelligence and training, many possible readings that would be foreign to the language and to the context. Nevertheless, when all allowances of this kind have been made, the number of variants that remained to perplex him would be numbered in thousands, not in tens or digits.

Elsewhere Mr. Ventris has often to search among the less-obvious phonetic variants to find a plausible interpretation, and we are thus made aware of the vast number of alternatives that exist. Here it happens that the most obvious and direct evaluation of the signs is the one that makes sense. For all that, alternatives do exist, and in such numbers that, if this particular interpretation could be proved correct, Mr. Ventris would be more than usually fortunate.

If Mr. Ventris' hypothesis were right and if his interpretation of the Pylian text were also right, we might fairly assume that the word-stems and word-endings in it would, at least occasionally, be confirmed elsewhere. We might find, for example, that the stem *ti-ri-* was identifiable either in synonyms (τρίτος, τριῶν, τριχά) or in homonyms (τριζω, τριβω) or in suffixes (μέτριος, πάτριος).

The suffix *-o-we*, if it occurs elsewhere, might be compounded with other numerals or with descriptive adjectives, or it might be intelligible as a spelling of -όφεις, -οφεν. Let us see what happens. The following words are transcribed with Mr. Ventris' values from the original form as given in Mr. Bennett's *Index*.

<i>o-wo-</i>	cf. <i>o-wo-to, o-wo-ze, o-wo</i>]
<i>qe-to-ro-</i>	cf. <i>qe-to-ro-po-pi, qe-to-ro-no, qe-to-ro-po</i>]
<i>ti-ri-</i>	cf. <i>ti-ri-to, ti-ri-se-ro-e, ti-ri-po-di-ko, ti-ri-sa-ta, ti-ri-ti-jo, ti-ri-ti-ja, ti-ri-jo, ti-ri-jo-pa₂, ti-ri-no, ti-ri-ti</i>], <i>ti-ri-da-ro</i>
<i>a-no-</i>	cf. <i>a-no-po, a-no-zo-jo, a-no-qa-ta, a-no-qa-ta-o, a-no-wo-to, a-no-ke-wa, a-no-ke-we, a-no-de-jo-si-wo, a-no-no, a-no-ra-ta, a-no-ze-we</i>

A few words in these lists might be understood as Greek, but none without difficulty. E.g. *ti-ri-to*, which occurs about twenty-five times, looks like τρίτον; but it never occurs in a context where τρίτον is probable. *Qe-to-ro-po-pi* means τετραπόφι to Mr. Ventris; but τετραπόφι is a monstrosity. *A-no-wo-to*, where it occurs, can hardly mean 'having no handle', and *o-wo-to* does not seem to mean 'having a handle'. In all cases where a meaning seems possible it is entirely unconfirmed.

There are other indications no less disquieting. In the limited range of Linear B texts available to us, initial *a-no-* is almost as common as initial άνο-, άνω-, άνου- are in the entire corpus of classical and post-classical Greek literature. It is plainly improbable, however, that in the period following the Dorian invasion so many words with this simple beginning should have vanished from the Greek language. Phonetic and morphological decay of this kind is unprecedented. But we shall find presently that Mr. Ventris' theory requires it not only for *a-no-* but also for *-ro-we*, *-To-we* and for *-po-(de)*, *-pa*, and for other syllables as well.

If we consider not only initial *qe-to-ro-* but all words beginning with the syllable *qe-*, we find that there are twenty-five words in this category. None of them, apart from the three with *qe-to-ro-*, looks like Greek, and it can be stated with some confidence that one or two words amongst them, e.g. *qe-re-me-e*, *qe-ra-di-ri-jo*, could not by any means be twisted into Greek.

Turning to suffixes, we find the following frequencies:

<i>-ro-we</i>	cf. <i>a-ro-we, pa₂-ni-ro-we, de-ro-we, a-ra-ro-we, ai-ta-ro-we, a-ko-ro-we,]ro-we</i>
<i>-ro-we-e</i>	cf. <i>a-ko-ro-we-e</i>
<i>-To-we</i>	cf. <i>u-po-we, a-do-we, a-qi-zo-we, ko-ra-o-we, ?-ko-we,]ko-we</i>
<i>-To-we-e</i>	cf. <i>qi-ko-we-e, ?-ko-we-e</i>

The total number of words in this list is sixteen, and it might be augmented if the final syllables of *po-ro-we*], *sa-ro-we*], *a-ko-ro-we*], *to-we*], *a-re-ro-we*], *o-ro-we*] were known. Of these sixteen, no less than seven have final *-ro-we*, and one has final *-ro-we-e*. But in Greek there are *no* words ending with -ρώης, and only *one* word, άμφώης, in a single occurrence, with -ώης. Even if we took into account -ρόεις, -όεις, and any other suffix of like form that actually existed in later Greek, we should not be able to redress the balance. For most, if not all, of the words listed above are obviously not Greek; and it would be no more than wishful thinking to associate *a-ro-we* with άρώω or *a-ko-ro-we* with άγρός.

The *-po*, *-po-de*, *-pa* suffixes yield similar results:

<i>-po</i>	cf. <i>a-ro-po, a₂-ro-po, ka-na-to-po,]na-po, po-po, o-wi-po-po, da₂-ru-po, pa₂-i-po, me-sa-po, e-po, a-no-po, e-wi-ri-po, wa-po, te-o-po, ai-ki-po, ka-po, ka-ka-po, u-po</i>
<i>-po-de</i>	cf. <i>da-da-re-po-de,]re-po-de, po-si-da-i-po-de, ki-ka-ne-po-de, ri-po-de,]ki-ri-po-de,]ri-po-de,]po-de</i>
<i>-pa</i>	cf. <i>to-so-pa, e-re-pa, i-pa, sa-pa,]ti-pa, ri-pa, me-ta-pa, o-pa, ka-pa</i>

All three suffixes are well attested. Yet it would be a bold spirit who equated any of the words listed above with any known Greek word. Most of them are utterly un-Hellenic in aspect. It is worth noting, too, that none of the words with *po-de* are extensions of words ending in *-po*, although the *ti-ri-po*, *ti-ri-po-de* relationship would lead us to expect this phenomenon. Nor is there any sign of a grammatical connection between *-po* and *-pa*, such as we should certainly find between Greek -πος and -πα in classical times.

Still seeking confirmation of Mr. Ventris' readings *ti-ri-po*, *ti-ri-o-we-e*, etc., we examine words made up of syllables that actually occur in the words of the Pylian text. We might look for Greek words in such cases; but we find none. For example, *ti-ri-no*, *e-wi-ri-po*, *pa-to-ro*, *de-ro-we* are as obscure as any of the strange words that can be discovered by applying Mr. Ventris' values to Mr. Bennett's *Index*.

Finally, if we select from the Pylian text syllabic groups such as *ti-ri-* and *to-ro-*, and examine their use in initial, medial, and final position, we find no more Greek words under this condition than we have found hitherto. When *-ti-ri* is final, it produces *po-ro-ti-ri*, *?-ti-ri*, *ja-ti-ri*. With *to-ro-* in the initial position, we have *to-ro-pa₂*, *to-ro-qa*, *to-ro-wi*, *to-ro-wi-ka*, *to-ro-wo*, *to-ro-no-wo-ko*, *to-ro-wa-so*,

to-ro-o, *to-ro-ki-no*, *to-ro-ge-jo-me-no*; and with final *to-ro* we find *da-to-ro*, *pa-to-ro*, *e-ru-to-ro*, *qo-e-to-ro*, *ru-wo-to-ro*, *ke-to-ro*, *a-ke-to-ro*, *pu-to-ro*, *re-u-ko-to-ro*, *]we-to-ro*, *ka-to-ro*. Again it is impossible to equate any word in these lists (except perhaps *e-ru-to-ro*) with a known Greek word. The words are numerous enough, but they do not suggest the Greek suffixes $\tau\omicron\rho$, $\theta\rho\omicron$, $\theta\lambda\omicron$ or the root $-\tau\omicron\rho\omicron\varsigma$.

Having reviewed all the evidence set forth above, we are bound to conclude that Mr. Ventris' decipherment of the Pylian text is in no way supported by the application of the same syllables and syllabic groups to other texts. The evidence is far from scanty; in certain cases it is abundant. But the syllabic groups, either in the same position in the word as in the Pylian text or in other positions, stubbornly refuse to yield Greek words. And some of the groups, notably *a-no-* and *-ro-we*, are far more frequent in the Linear B script than their equivalents ever were in classical Greek. Accordingly, those who subscribe to Mr. Ventris' interpretation of this text must acknowledge also the flood of un-Greek words that it carries with it. Either they will have to postulate the occurrence of an un-Greek language alongside Greek in the Linear B documents or they will have to hope that elements which now appear un-Greek may by some miracle be proved to be Greek in the end.

I now turn to examine that part of the Pylian text for which a plausible interpretation has been proposed. And at this stage it is important to remember that a substantial number of words in the text are still unexplained, that the phonetic evaluation of the text as a whole is only one of many variants, and that the phonetic values attached to the syllables and syllable-groups concerned are by no means confirmed in other texts. These are formidable obstacles; but there are worse to come.

First I consider individual words in the text.

1. The word *ti-ri-po(-de)* is applied twice, and was probably applied three times in all, to basins mounted on three-legged stands. This corresponds to the Homeric and classical use of $\tau\rho\acute{\iota}\pi\omicron\upsilon\varsigma$, and is therefore at first sight acceptable. There are, however, two possible objections. First, Gk. $\tau\rho\acute{\iota}\pi\omicron\upsilon\varsigma$ is as much an adjective as a noun, and some centuries before Homer it might be expected to appear as an adjective rather than as a noun. Secondly, since ll. 2-3 deal with vessels and not with stands, one would suppose the subject of l. 1 to be not the three-legged stands but the basins which they support. We might look to see $\lambda\acute{\epsilon}\beta\eta\varsigma$ etc. $\tau\rho\acute{\iota}\pi\omicron\upsilon\varsigma$, not $\tau\rho\acute{\iota}\pi\omicron\upsilon\varsigma$ alone.

2. Gk. $\delta\acute{\epsilon}\pi\omicron\varsigma$ means 'cup', 'goblet'. Such cups were made of metal and generally had one or two handles; very large, ornate cups might have as many as four handles. But the ideograms that accompany the word *di-pa* on the Pylian tablet do not represent cups, but jars or pots. And four of the pots have not one or two 'handles', but three or four; and the fifth has none. Since they are listed with tripods and are few in number, we may perhaps suppose that like the tripods they were made of metal. We may also guess that they were comparable, though not necessarily equal or nearly equal, to tripods in size; *i.e.* that they were fairly large pots. The fact that they have 'handles' fitted to the rim suits this hypothesis. For a cup or goblet would have large handles, fitted to the rim and side or to the rim and bottom, and a large earthenware jar would have small handles, on the rim and side or on the side only; and small earthenware cups are hardly to be considered here. Only a big metal pot is likely to be held up by means of lugs protruding from the rim.

So the identification *di-pa* = $\delta\acute{\epsilon}\pi\omicron\varsigma$ does violence to the facts of the context, so far as they can be discerned. This conclusion is supported by a Knossian tablet which associates the word transcribed *di-pa* with a round, heavy-looking vessel, not at all like a cup. Another Knossian tablet shows a vessel on a tripod stand, with the syllable *di* inscribed upon it. Possibly *di* here stands for *di-pa*; but the vessel once more is not a cup, but an amphora.

In any case, the notion that the first syllable of Gk. $\delta\acute{\epsilon}\pi\omicron\varsigma$ might in Mycenaean times be written *di-* is unwarranted. In classical Arcadian *i* occurs for *e* before *n*, and occasionally before *d*. But it is not likely that this tendency began as early as the Mycenaean age or that it ever spread beyond the mountain-valleys of Arcadia. And there is no trace of a shift from *e* to *i* after a dental stop or before a labial stop in any part of the Peloponnese. (Gk. $\acute{\iota}\pi\pi\omicron\varsigma$ cannot be admitted as evidence here. It is by no means clear how $\acute{\iota}\pi\pi\omicron\varsigma$ developed out of **ekwos*, if at all. But in any case the conditions are quite different. On the one hand, there is no by-form **\acute{\epsilon}\pi\omicron\varsigma*, **\acute{\epsilon}\pi\pi\omicron\varsigma* in Greek, and on the other hand, there is no **\delta\acute{\iota}\pi\omicron\varsigma*.)

3. The word *qe-to* in l. 2 should on the analogy of *ti-ri-po(-de)* and *di-pa* mean 'amphora'. But there seems to be no Greek word that suits this hypothesis. Certainly we must reject on phonological grounds any connection with $\pi\acute{\iota}\theta\omicron\iota$. It may be worth noting that the Knossian tablet (already mentioned) which contains the word *di-pa* also contains a word *qe-tu*. According to Mr. Ventris, I suppose, *qe-tu* might be an 'Arcadian' form of *qe-to*. But even if this were so (and it is highly improbable), the Knossian text would not confirm that *qe-to* meant a kind of vessel, any more than it shows *di-pa* to mean 'pot'. It would merely illustrate a second time that *qe-to* and *di-pa* could be used in association with vessels, and it would show that the type of vessel concerned need not always be the same. There is nothing to show that either word did not signify an occupation, a commodity, a measure, or the like.

4. To readers of Mr. Ventris' article, the words *me-zo-e*, *me-wi-jo* are old friends. Mr. Ventris identified them, and also *me-zo* and *me-wi-jo-e*, at an early stage in the process of deciphering the

script. The Knossian texts in which they were first recognised are supposed to give lists and numbers of boys and girls, who are thus divided into 'large' and 'small' categories. It is quite unlikely, however, that children would be so classified, rather than by an age-limit or by an exact measure of height. An exact division by age or height might, it is true, be designated simply as 'large' and 'small'; but this again is unlikely. It is also unlikely that in the Pylian text pots would be marked simply 'large' or 'small'. Homer distinguishes them by a measure of capacity, and that is the obvious way. But in any case, whether in relation to children or vessels, Mr. Ventris' assumption that *me-zo*, *me-wi-jo* mean 'large' and 'small' respectively remains a guess, which the mere multiplication of instances does nothing to confirm.

If *me-zo(-e)* is outwardly unobjectionable as the equivalent of μέζον, μέζω, the same is not true of *me-wi-jo* in relation to μείον. For the IE base of μείον is *mey-, *mi-. Neither in Greek nor elsewhere is there any sign of a variant *mew- or *mewi-. The mere fact that Mr. Ventris' transcription now yields *me-wi-jo(-e)* is no proof to the contrary.

Besides, Mr. Ventris' interpretation of the inflected forms *me-zo-e*, *me-wi-jo-e* will not bear serious examination. In the Pylian text he has to assume that the first *me-zo-e* was intended by the writer to be *me-zo*. I.e., the writer meant to write the neuter singular, but for one reason or another he wrote the neuter dual, which he was to use in the following phrase. This excuse cannot be accepted. Speakers of inflected languages make such slips but rarely; and in the present case correction would have been easy. In fact, the association of *me-zo-e* with both *di-pa* and *di-pa-e* in the Pylian text appears to be in harmony with the use of *me-zo-e* and *me-wi-jo-e* in a number of Knossian texts. The forms with and without *-e* are evidently used alongside each other and without any distinction of number. The natural inference from such cases is not that writers of Linear B constantly confused the singular, dual, and plural inflections but that Mr. Ventris' evaluation of the sign which he renders *e* is mistaken.

In addition to these considerations it should be observed that uncontracted *oe*, *ae*, *ee* are by no means probable in nominal inflections, even in the second millennium B.C.

5. The Pylian text contains four different words ending in *-owe(e)*, occurring six times in all. There are sixteen examples of this suffix elsewhere in the Linear B corpus. In classical and post-classical Greek the suffix *-ως* occurs in one word only and on one occasion. I have already remarked on this discrepancy between Mr. Ventris' Greek and the Greek that we know (see above, p. 11).

This *-owe(e)* suffix in Mr. Ventris' transcription is said to represent Gk. *-ῶφες*, *-ῶφες*, a compounded form of οὔς, οὔρατος. But in classical Greek compounds formed from this word end in *-ούρατος*, contracted *-ωτος*, and they occur as early as Homer. Against these compounds we have to set the isolated occurrence of ἀμφώης, 'two-handed', in Theocritus. Does ἀμφώης confirm the existence of a series of compounds with final *-owe* a thousand years earlier? It is very hard to believe that it does. For ἀμφώης seems likely to be a coinage of late epic poetry or even of Hellenistic poetry. It is made by removing the common suffix *-τος* from ἀμφώτος and replacing it by the more archaic *-ης*. Indeed, the contracted form ἀμφώτος from ἀμφούρατος is almost a pre-requisite for the creation of ἀμφώης.

We should in any case doubt whether *-owe* is the proper form for a derivative from οὔς in Mr. Ventris' transcription. IE **-ōus-es* should give early Greek **-ōuhes*, **-ou'es*, with the syllabic break after the diphthong *ou*. It is not a case of intervocalic *w* between *ō* and the ending *-es*, but of a true diphthong *+ -es*. According to Mr. Ventris' rules, strictly applied, the inherited form should be written *-o-u-e* or *-o-u-we*. And there is no reason why the rules should not be strictly applied in this case; for the IE diphthongs are not likely to have undergone serious modification in the Mycenaean age.

6. We have to ask whether the numerical elements prefixed to the suffix *-owe(e)* are correct. Provisionally we may accept *o-wo-* = οἰφώ- and *ti-ri-o*, *ti-ri-jo-* = τριώ-; they conform to the rules of orthography. With *qe-to-ro-we* and *a-no-we* the case is not so simple.

In Greek we find ἄουτος 'deaf' (Hesychius), ἄωτος 'without handles' (Philetas, Plutarch, etc.); Homer has ἄουτος 'unwounded'. We also find in Homer ἀνούρατος, 'unwounded'. But nowhere do we find ἄνουτος, 'without ears, handles'. It is therefore curious that some centuries before Homer *a-no-we* should occur, apparently in ordinary use, with *n* bridging the hiatus between prefix and stem.

In a word formed from IE **qetwr-* and *-ōuses*, we should expect to find **τετραῶφες* or, if we were to allow Mr. Ventris' substitution of 'Arcadian' *o* for *a*, **τετροῶφες*. We do not expect the contracted **τετρῶφες*, *qe-to-ro-we*; for even in historical times *τετρα-* is often uncontracted when followed by a vowel. This is not a matter upon which one would in other circumstances lay much weight. Its importance is increased, however, by Mr. Ventris' insistence on final *-ae*, *-oe*, *-ee*, all uncontracted.

Lastly, it should be remarked that *o-wo-we*, οἰφώης, 'with single handles', differs in sense from the other words ending with *-owe(-e)*. It refers to the *form* of the handles, not to the number of handles on each vessel. Although it is quite possible in theory that οἰφος and οὔς could be compounded in this way and that the compound should have this meaning, it is not at all likely that such

a compound would be used alongside others meaning 'three-handled' and 'four-handled'. There would be no way of telling that the word did not mean 'one-handled'; and, given the existence of a series 'three-handled', etc., we should expect a different kind of expression or a different kind of compound to denote the form of the handles (*e.g.* σύν ἀπλοῖς οὔρασι, ἀπλ-ούρατος or σύν οὔρασι ἀπλοειδέσι). On this account the presence of *o-wo-we* in the text does not support the validity of the other three adjectives, but rather tells against it. It reminds us that *-we* is a very common suffix in Linear B documents and that *-To-we*, too, is well-attested. The appearance of *To-we* four or five times in one of Mr. Ventris' transcriptions is indeed not very remarkable; it might arise from many causes and need have nothing to do with lugs or handles.

If individual words in Mr. Ventris' transcription are open to criticism, the transcription as a whole is in no better case. Each entry on the tablet is supposed to give the name of a vessel, and most entries in addition state the number of handles on the vessel and indicate its size. But the ideograms which accompany the written entries also specify the kinds of vessel and the number of handles on each. The only matter on which the words give information and the ideograms do not is the size of the vessels. On the other hand, the words do not say how many vessels of each kind there were, but the ideograms are accompanied by figures which serve this purpose. We can only wonder that the writer did not have at his disposal an ideographic sign for 'large' or 'small' and that he did not think it necessary to write out the words corresponding to his figures. It is more than surprising, however, that in so short a document there should be so much duplication of phonetic script and symbols as there is. In these circumstances ability to read and write seems largely wasted.

This Pylian tablet is presumably, like so many others, a record of goods held in, due to, or paid from the palace. Yet it fails to say who was the owner, creditor, or debtor; it does not say where the goods came from, where they were, or where they were going; it does not suggest what purpose they served or on what occasion they were listed; it omits all mention of materials, contents, values. It would be useless to seek any information of this kind in the obscure portions of l. 1 (Αἰγεύς, Κρήσιος, etc.); for it is a safe assumption that each entry in l. 1, like those in ll. 2-3, is complete and self-contained and contains nothing that could illuminate the other entries. We cannot avoid recognising that, on Mr. Ventris' interpretation, the tablet is a mere catalogue of vessels without any apparent context.

The most curious feature of all is the writer's insistence on handles. One might imagine that handles were a criterion of size; but this is evidently not the case here, since the vessels in ll. 2-3 are already divided into 'large' and 'small', and the classification by handles cuts across this division. We should in any case suspect the validity of a list that has no one-handled or two-handled pots but knows only those with three or four handles or none at all. Vessels lifted by one man's hands usually have one or two handles; those intended for two men to lift have either two handles or four. Three handles are a rarity.

It is important to observe the deficiencies and inconsistencies of Mr. Ventris' text as well as its actual content. A single-handled tripod is apparently worth mentioning (*o-wo-we*), but a double-handled tripod is not; and whether the third tripod had handles or not, evidently no attempt was made to say so in words. Again, the amphora handles, apparently single, are not called *o-wo-we*; and indeed they are not mentioned. Thus out of nine items, three are not described in terms of handles; but at least two of the three had handles, and they seem to have differed from each other chiefly in the shape of these handles (see below). Accordingly, it appears that handles are far less conspicuous in this text than we might at first suppose, and they are least conspicuous where there is seemingly most reason to mention them.

Our examination of Mr. Ventris' interpretation has given the following results. Of the three words that are supposed to refer to a kind of vessel, *qe-to* and *di-ḫa* are unacceptable, and *ti-ri-ḫo(-de)* is not above suspicion. Both the adjectives *me-zo-e* and *me-wi-jo* are to be rejected without reservation. The 'handle'-epithets present a more impressive appearance than the rest; but the *To-we* suffix is very far from convincing, and the forms *a-no-we* and *qe-to-ro-we* are suspect. And there is a serious discrepancy in meaning between *o-wo-we* and the rest. Thus every single word in the text is open to criticism, and in most cases the criticism is so severe that there is little chance left of Mr. Ventris' proposal being right. In these circumstances the individual words do not support each other; the case is rather that the faults discovered in each unite to throw discredit on all. When we take into account also the apparently purposeless character of the text as a whole, its repetitions, omissions, and inconsistencies, we cannot but conclude that Mr. Ventris' interpretation is seriously mistaken. To reinforce this conclusion we have at hand the three points that came to our notice at the outset—the unexplained portion of the text, the variable value of the transcription within the limits of Mr. Ventris' own rules, and the absence of any confirmation in other texts for the phonetic values applied here.

All that remains to support Mr. Ventris is a vague but prevalent feeling that, even if his interpretation is virtually meaningless, somehow it 'hangs together' and that this degree of intelligibility could not depend on mere coincidence. For my own part I should be content to confront those who are of this opinion with the linguistic arguments which I have already advanced. But in order to demonstrate that such obsession is unreasonable, I shall now show, first, that an alternative inter-

pretation of the context of this tablet at least as probable as that of Mr. Ventris can be achieved and, secondly, that the working of coincidence in Mr Ventris' interpretation is not so extensive or so complicated as it might seem at first sight.

It is evident that Mr. Ventris has failed to produce a convincing synthesis of words and ideograms, or even to account adequately for the ideograms alone. The question arises whether there is a better explanation of these matters.

The tripod-handles and amphora-handles differ noticeably from the pot-handles. They are circular grips, and project outwards from the side or shoulder of the vessel. Evidently they are true handles, to be grasped with the hand. On the first tripod the handles are double; *i.e.* each ring has a dent in its circumference and so makes a grip for two hands together. The handles of the second tripod are plain circles and give a hold for one hand only. We may infer that the first tripod is of a large, heavy type that might need two men to lift it. The second tripod would then be smaller and lighter, lifted by one man with both hands or by two men, one on each side, using one hand each. The form of the third tripod is unknown. The amphora ideogram in l. 2 has rings like those of the second tripod. We may guess that it represents a vessel about as heavy as the second tripod, or at any rate not heavier. If so, the tablet as far as the beginning of l. 2 will represent vessels in descending order of magnitude.

The handles on the pots are set along the rim, not on the side or shoulder; and they project upwards, not outwards. They seem, moreover, to be oval or eye-shaped rather than round. And when they occur, it is in threes and fours. Now these pots might be very heavy, and more than one man might be needed to move them; or, if they could be moved by one man, it might be convenient for him to have more than two grips to catch hold of from time to time. But then it would be hard to understand why of three vessels which, according to Mr. Ventris, were about equal in size one should have four handles, another only three, and a third none at all.

If, however, the vessels from beginning to end of the tablet, and not only from the beginning to the amphora ideogram, are arranged in descending order of magnitude, we may suppose that all the pots are smaller than the tripods and amphorae, or at least no bigger. We may further conjecture that the pots with four lugs were the largest, those with three of middle size, and those with no lugs the smallest. In this case we shall have to leave unexplained the repetition of the four- and three-lugged types in ll. 2-3; the cause of this repetition might lie in a difference of contents, value, or the like.

Now it is possible to explain the lugs not by reference to Mr. Ventris' suffix *-owe(e)* but from their actual shape and arrangement. These lugs are not, I suggest, handles but 'eyes' or rings for the attachment of chains or cords, Gk. κρικός rather than οὔς. The pots were to be kept hanging from a beam or hook or they were to be raised and lowered on the end of a cable. The number of lugs is limited to three or four by the need to support the vessel securely and maintain its balance; two might not serve, and five or more would be too many. The size of the pots can hardly be determined accurately from the number of lugs, but those with four may well have been both larger and more open at the top than those with three.

Pots without lugs would simply be grasped with the hands and made to stand upon a flat surface or grill.

The Pylian tablet on Mr. Ventris' interpretation is concerned with vessels of three kinds and with their handles. But we now say more accurately that it deals with tripods of two sizes, having different handles, with amphorae, and with pots of three sizes, distinguished by a varying number of rings and chains. How are we to relate the words of the text to these objects? We shall not, of course, attempt an alternative transcription and alternative interpretation to those of Mr. Ventris; we shall limit our attention to the external evidence of the writing.

The first thing to be noticed is the relationship between the words transcribed *qe-to* and *qe-to-ro-we*. Since both words are used in connection with vessels of similar, though not identical, shape, it seems reasonable to suppose that *qe-to-ro-we* is a case-form, derivative, or compound of *qe-to*. We must not lose sight of the possibility that *qe-to* and *qe-to-* are homonyms without any common meaning, but we may provisionally believe that they are related.

We may then identify, also provisionally, the *-ro-we* of *qe-to-ro-we* with the 'suffix' of that form which is recorded seven or eight times in Linear B documents (see above, p. 11). And, remembering that *-we* is a common final syllable, we shall cease to attach any special significance to its appearance in this text after *o-*, *jo-*, *o-*, *no-*; the occurrence elsewhere of *-do-we*, *-po-we*, *-zo-we*, *-ko-we* sets our minds at rest on this score.

Next we may compare the words which Mr. Ventris renders *ti-ri-po(-de)* and *ti-ri-o-we-e*, *ti-ri-jo-we*. We shall not assume that the two words last-mentioned are variants of the same word, the *j*-glide being inserted or removed at random, or that the syllables transcribed *ti-ri-* are a prefix meaning 'three' or that they refer to legs and rings. We need go no farther than to guess that all three words are case-forms, derivatives, or compounds of a stem *ti-ri-*.

So by a few simple observations and inferences we have formed the outline of an interpretation of the text. We find the two elements *ti-ri-* and *qe-to-* (*cf.* also *ke-re-* in l. 1) running through the whole document, apparently in some sort of gradation or subordination. Without venturing to say

that Mr. Ventris' transcription of the words is correct, far less to reinterpret his transcriptions, we may claim that this outline offers the possibility of a really significant interpretation in which the ideograms and figures would be *equivalent* to the words but not a *repetition* of them. And in this respect it is clearly superior to Mr. Ventris' interpretation.

The extent and importance of coincidences in the original text of the Pylian tablet and in Mr. Ventris' transcription of it should be calculated accurately, and neither over-estimated nor under-estimated. It is in any case important to keep the repetitions and correspondences in the original separate from those which depend on the method of transcription. The following occur in the Linear B form of the text (for convenience I represent the words in transcription):

Word.	No. of occurrences.	Remarks.
<i>ti-ri-po(-de)</i>	3	twice (at least) with a tripod
<i>di-pa</i>	5	always with a pot
<i>me-zo-e</i>	2	twice with <i>di-pa</i> and pot
<i>me-wi-jo</i>	3	three times with <i>di-pa</i> and pot
<i>qe-to-ro-we</i>	2	twice with <i>di-pa</i> and four-lugged pot
<i>ti-ri-</i>	2	twice with <i>di-pa</i> and three-lugged pot
<i>o-wo-</i>	1	with ring-handles on a tripod
<i>a-no-</i>	1	with lug-less pot
<i>-we(-e)</i>	6	five times with <i>di-pa</i> and pot, once with <i>ti-ri-po</i> and tripod.

Mr. Ventris' transcription produces the following instances in addition:

Word.	No. of occurrences.	Remarks.
<i>ti-ri-o-we(-e)</i>	2	twice with <i>di-pa</i> and three-lugged pot, <i>if</i> the <i>jo</i> of <i>ti-ri-jo-we</i> is equivalent to <i>o</i>
<i>-To-we</i>	6	five times with <i>di-pa</i> and pot, once with <i>ti-ri-po</i> and tripod.

In considering the foregoing table we must bear in mind that mere repetition in a catalogue may signify nothing. If *ti-ri-po* occurs once with a tripod or *di-pa* with a pot, then either of these coincidences repeated a hundred times will hardly reinforce what is already evident, viz, that *ti-ri-po* and tripod are in some way relevant to each other; and so with *di-pa* and pot. They will not show that *ti-ri-po* means 'tripod' or that *di-pa* means 'pot'. Even if *di-pa* occurs alongside a pot elsewhere (as in fact is the case), we shall be as far as ever from proving the identity of the word *di-pa* and the object 'pot'. So also with groups of words. If *di-pa me-zo-e* occurs once, it may well occur twice, and we shall infer nothing from the repetition. *Di-pa me-wi-jo* we shall treat in exactly the same way. From other Linear B texts it is known that *me-zo-e*, *me-wi-jo* often occur together and that they apparently differentiate objects that are otherwise alike; but we can infer no more than that from their conjunction here. Again, if there is attached to each instance of *di-pa me-zo-e* or *di-pa me-wi-jo* a word ending in *-we*, we must recognise that this phenomenon, having occurred once, may in a list be repeated again and again. We shall not jump to any further conclusion concerning these words and phrases. Indeed, since *-we* occurs also in relation to a different kind of vessel with a different kind of handle, we shall be cautious with regard to any such conclusion.

We shall not relax our caution when Mr. Ventris presents us with the two transcriptions *ti-ri-o-we(-e)* and *ti-ri-jo-we* and tells us that *o* and *jo* are mere phonetic variants. There is no reason to expect such a variation within two lines of a short text, even if it were usual from one text to another or from one part of the Mycenaean empire to another. Again, given the frequency of *To-we* in other texts, it is unnecessary to infer that its repeated occurrences here have any *special* bearing on the sense of the words in which it is used or of the text as a whole.

We may fairly exclude from the reckoning the correspondences *a-n(o)*- 'without' (sc. handles) and *o-wo-* 'single' (sc. -handled). The former does not suit the phonological pattern of the Greek language; the latter does not suit the context that Mr. Ventris postulates (see above, pp. 11 and 12f.).

The list of repetitions and correspondences is thus a good deal less impressive than it seemed to be. Indeed, there now remain only two cases for which it might still seem proper to postulate some other cause than coincidence. These are:

<i>ti-ri-</i>	‘ three ’
<i>qe-to-ro-</i>	‘ four ’

Even if all else be disregarded, these two stems together make a formidable appearance. The question is, are they strong enough to overturn all my arguments and vindicate all those put forward by Mr. Ventris?

In considering *ti-ri-* and *qe-to-ro-*, I recall that *-po(-de)* and *-(T)o-we*, with which they are associated, are unconfirmed and improbable, that the apparent contraction of *qe-to-ro-* with *-o-we* is unacceptable on the conditions with regard to contraction and non-contraction laid down by Mr.

Ventris, that the *-ro* (for *-ra*) of *qe-to-ro* is doubtful. I recall also—and this is of more fundamental importance—that initial *qe-* lacks confirmation, and that both *ti-ri* and *to-ro* in initial, medial, and final position are unconfirmed. In these circumstances it would be amazing if *ti-ri-* ‘three’ and *qe-to-ro-* ‘four’ were to prove correct.

In the Linear B corpus ideograms of metal and clay vessels appear very frequently and it can only be an accident that among the many types of vessel represented tripods are rare outside the Pylian tablet under discussion. It must be accidental, too, that there is apparently only one other tablet that shows a pot with lugs on the rim—a pot with three lugs on a fragmentary tablet from Knossos. Three-legged stands and pots with three lugs or four were presumably common enough in cities of the Mycenaean age.

Now the elements *ti-ri-*, *to-ro-*, and initial *qe-* are well attested. There are many examples of initial *ti-ri-*, and even a few of initial *qe-to-ro-*. We have already observed, however, that in other texts *ti-ri-* and *qe-to-ro-* are not associated with ‘three’ or ‘four’.

If, then, we find in this Pylian text *ti-ri-* with three lugs and three legs and *qe-to-ro-* with four lugs, we have no right to infer that there is an essential connection between these syllabic groups and these numbers. The syllabic groups and the ideograms are alike commonplace. It is remarkable that they should happen to occur together, but the coincidence is not strong enough to support an interpretation of the whole text. Since we have seen that this interpretation is otherwise insecure and verging on collapse, we must conclude that the apparent correspondence between words and ideograms is due to chance. It is, when all is said and done, a simple enough coincidence, of the kind that crops up constantly in the case of simple numbers and simple words.⁶

We have seen that, even without any knowledge of the language of the Linear B script, it is possible to construct an interpretation of this Pylian text that will stand comparison with Mr. Ventris' interpretation. We have seen, too, that the extent of verbal and numerical correspondences in this text is relatively unimportant and attributable to chance. Therefore we may return with confidence to the linguistic arguments that have already been advanced against the Ventris interpretation. If these arguments are well-founded, most or all of that interpretation must fall; and with it must fall much of Mr. Ventris' entire decipherment. If, on the other hand, Mr. Ventris is to maintain both his interpretation of the Pylian text and his system of decipherment, he must find a convincing answer to the linguistic objections to which both are exposed.⁷

I am indebted to Professors A. C. Aitken and A. H. Campbell and to Mr. A. H. Coxon for advice and criticism given to me during the preparation of this paper. Responsibility for the opinions expressed in it is naturally mine alone.

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⁶ Although Mr. Ventris has concentrated attention on the *ti-ri-*, *qe-to-ro* elements, and on the *-Towe* ending, it is well to consider also the following table of words. There is an apparent relationship between these words in Linear B, but it does not correspond to anything, real or apparent, in Greek.

<i>ti-ri-po(-de)</i>	<i>ti-ri-jo-we</i>	<i>ti-ri-no</i>	<i>ti-ri-to</i>	
<i>qe-to-ro-po-pi</i>	<i>qe-to-ro-we</i>	<i>qe-to-ro-no</i>		
<i>a-no-po</i>	<i>a-no-we</i>	<i>a-no-no</i>		<i>a-no-ze-we</i>
	<i>o-wo-we</i>		<i>o-wo-to</i>	<i>o-wo-ze</i>

⁷ The tragic death of Mr. Ventris in a motoring accident was announced as the *Journal* was going to press. The author and editors wish to express their sorrow at this grievous loss to scholarship, and their hope that the new enthusiasm for Minoan studies his work has aroused will continue to bear fruit.

CORRIGENDA

The following corrections should be made in *JHS* lxxvi, 1956, pp. 11-17:—

- P. 11, l. 8: del. *ti-ri-no*; so also l. 55 and p. 17, fn. 6
l. 9: del. *a-no-de-jo-si-wo*
ll. 29-32: for *pa₂-ni-ro-we*, *de-ro-we*, *a-ra-ro-we* read:
pa₂-sa-ro-we, *ke-ro-we* (so also l. 55),
a-ta-ro-we; del. *ko-ra-o-we*
l. 34: del. *to-we*[, *o-ro-we*[, *a-re-ro-we* [
ll. 42-43: del. *a₂-ro-po*, *o-wi-po-po*, *te-o-po*
ll. 44-45: del. all from *de-da-re-po-de* to]*po-de* and
read *po-de*, *po-ru-po-de-ge*
l. 46: del. *i-pa*,]*ti-pa*.
P. 12, l. 1: for *go-e-to-ro* read *za-e-to-ro*
l. 2: for *ru-wo-to-ro* read *re-wo-to-ro*
ll. 57-59: for *ge-tu* read *ge-to*, del. all from "Accord-
ing" to "improbable)" and for "would" read
"does."
P. 15, l. 56: for the first *o-* read *wo-*.

I concede that there is virtually no evidence for or against the alternation of endings in *ti-ri-po*, *ti-ri-po-de*. In other respects my argument is hardly affected by the corrections given above.

A. J. BEATTIE

EDITORIAL NOTE TO CORRIGENDUM TO VOL. lxxvi, p. 17 n. 7.

By a mistake in printing, the short note on Mr. Ventris' death—all that was possible in the brief interval before publication—was inserted as a footnote to Professor Beattie's article on Linear B, instead of as a separate editorial note below. This has given some the impression that Professor Beattie was responsible for its insertion and wording, and since he has been in consequence subjected to adverse criticism, I wish to state that this was not the case. Responsibility rests with the editors alone.

THE EDITOR.